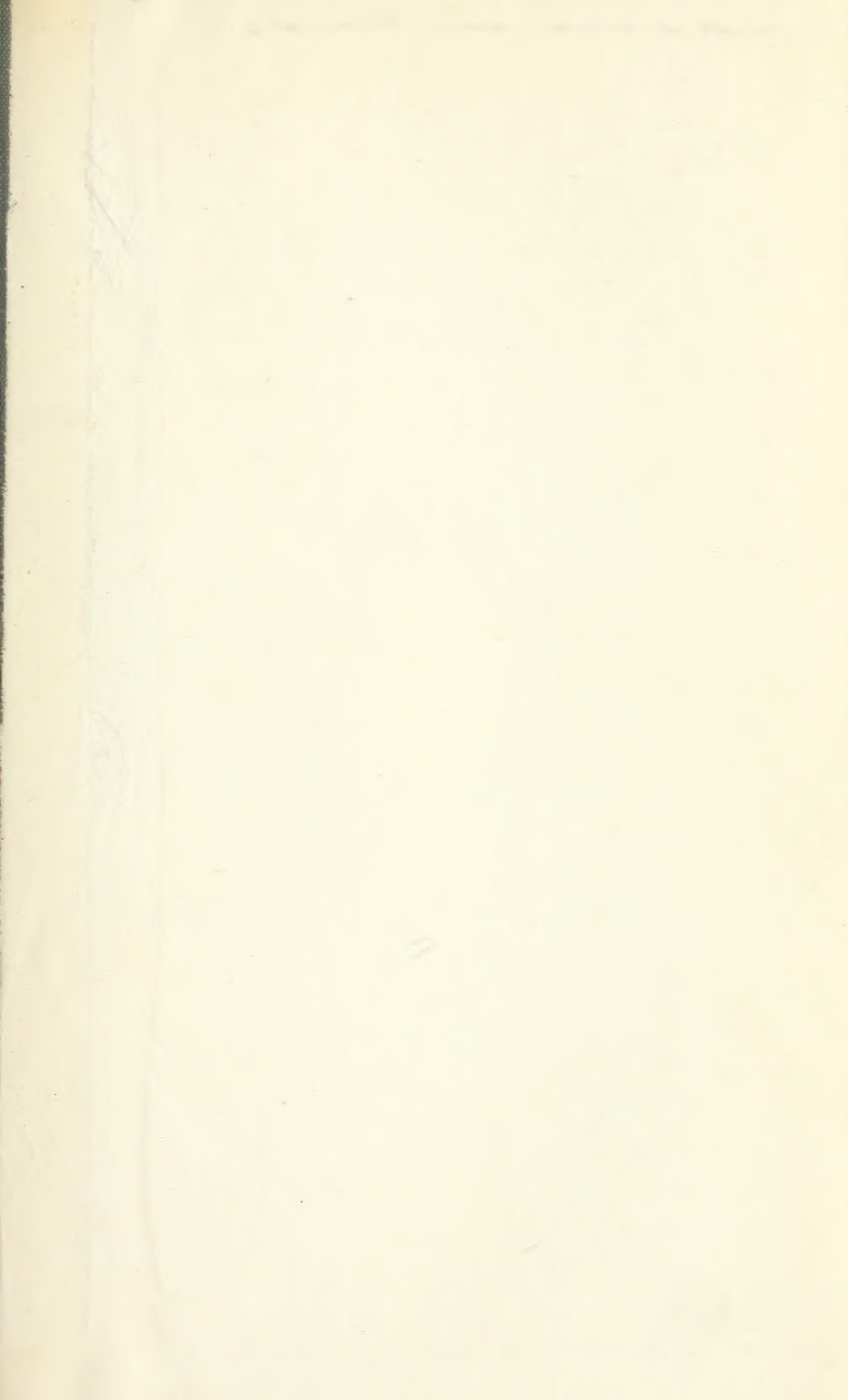


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THE
AMERICAN
JOURNAL OF OBSTETRICS

AND
DISEASES OF WOMEN AND CHILDREN

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ORIGINAL COMMUNICATIONS.

A CONTRIBUTION TO THE PATHOLOGY OF THE MUCOUS
MEMBRANE OF THE UTERUS.

BY
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New York.

(With three woodcuts.)

THE pathological changes of the mucous membrane of the uterus have by no means been exhaustively described. We cannot expect that a membrane, concerning the normal condition and physiological changes of which there are so many diverse views, should, when invaded by disease, furnish us with a clear and well-defined pathology. The mucous membrane of the uterus suffers in its normal state considerable physiological disturbance; it undergoes greater or less changes during menstruation, and during pregnancy it is transformed into the decidua. No doubt such a membrane will offer us an abundance of pathological material. We will also meet no insignificant difficulties when we attempt to draw a marked boundary line between normal and morbid changes. Several factors co-operate to increase these difficulties: the complicated structure of the uterine mucous membrane, which is composed of the two important varieties of tissue, the connective and the

epithelial, and the stroma of which bears the adenoid character; further, the identity of clinical symptoms in a large number of uterine diseases; and, finally, the difficulty of combining clinical observations with accurate microscopic examinations. Ruge and Veit,¹ in their work on cancer of the uterus, have contributed much to its pathology, but much still awaits study and research. To furnish a contribution to the pathology of the mucous membrane of the uterus is the object of this paper.

Before entering upon the discussion of the subject proper, I deem it necessary first to give a description of the lining of the adult uterus in its normal state.

The mucous membrane of the corpus uteri in the adult is smooth, of a pale red color, one to two mm. thick, and closely adherent to the muscular wall without any intervening submucous tissue. This attachment is strengthened by delicate bundles of connective tissue bearing the vessels, and running from the intermuscular interstices to the mucous membrane. At the internal os, the mucous membrane of the corpus is replaced by the lining of the cervix, from which it is easily distinguished by a line of demarcation. The membrane of the cervix blends its columnar with the stratified epithelium in the lower third of the canal. A paler red color and numerous folds, known as *plicæ palmatæ*, are the peculiar qualities of the mucosa of the cervix. The mucosa of the corpus and of the greater part of the cervix is lined with ciliated columnar epithelium, which continues as the lining of the utricular glands. The mouths of the latter may be seen as fine orifices, visible to the naked eye. The glands are tubular formations with a slightly winding course, are often bifurcated, are perpendicular or oblique, and at times even parallel to the surface.

In the cervical canal, besides the long tubular formations, small bottle- or pear-shaped glands are found.

They, together with the vessels, are imbedded in the stroma, which, being of importance, I shall describe in detail.

Microscopic sections—I hardened them in chromic acid, brushed them out, and stained them with an ammoniacal carmine solution—present the following appearance:

¹Ruge and Veit, *Zeitschrift für Geburtshülfe und Gynäkologie*, vols. ii., vi., and vii.

Numerous round cells, like lymph-corpuscles, and spindle-shaped cells are irregularly and closely imbedded in a light, homogeneous, sometimes finely granular, intercellular substance. Some of the cells, especially the spindle-shaped, give off thread-like processes, and form a delicate netting. These cells vary in form, being either round or spindle-shaped, with pointed ends, oval, plano-convex, or rod-like with rounded poles; in short, showing all possible transitions from round to spindle-shaped. Small granules are also frequently met with between the larger cells. In general, all cellular structures are distinguished by a large nucleus, surrounded by a small light rim of protoplasm, and containing one or more shining nucleoli. Often this rim of protoplasm is wanting, so that only a free nucleus is to be seen. Most authors are satisfied with enumerating these elementary formations as I have given them. Leopold,¹ in his essay on the lymphatics of the uterus, is the first who shows the relation of these cells, apparently irregularly packed together, to the intercellular substance, the vessels, and the glands. However, I cannot entirely agree with the explanations which this author gives of his microscopic views. My opinion is as follows: The mucous membrane of the uterus is composed of a delicate reticulum, formed by the cellular offshoots mentioned above. The meshes of this reticulum are filled with lymphoid corpuscles. Both the reticulum with its spindles and the lymphoid corpuscles are imbedded in a light, homogeneous intercellular substance, in which the filaments of the reticulum disappear. Occasionally, these filaments are wanting for some distances; the cells seem then placed side by side, only separated by the narrow, light intercellular trabeculæ which, by this appearance, may be accurately distinguished from the darker, strongly granular imbedded cells. Leopold considers this intercellular substance as lymph-filling lymph-spaces, the walls of which are composed of an endothelial layer; he imagines thus a double reticulum both of connective tissue and of endothelium adhering to the former; he describes the cellular elements of the endothelial network as flat "small plate-like cells," etc.

¹ Leopold: "Die Lymphgefäße des normalen, nichtschwangeren Uterus," *Archiv für Gynäkologie*, vol. vi.

The reasons for considering this intercellular substance as such, and not as coagulated lymph, are the following:

1. In most places, this intercellular substance is not granular, but homogeneous.

2. At places of transition from the normal tissue to the reticular sarcoma, this homogeneous intercellular substance is transformed into fibrous trabeculæ; it shows a more fibrous character in the mucosa of the cervix and in that of the uterus of aged persons. Of course, nobody is able to prove transitions, since the microscopic view offers us only conditions, and no actions. The comparison, however, of two conditions close together justify the above view.

I do not consider the question concerning the origin of the lymphatics of the uterine mucosa solved by the investigations of Leopold, inasmuch as we can perceive the network of fine filaments with spindles without the artificial and easily misleading pictures produced by nitrate of silver. Besides a confirmation of Leopold's researches is still wanting.

These cells may, of course, be interpreted in different ways; they are either lymph-corpuscles or endothelial cells, which give rise to blood or perhaps lymph capillaries; or they are connective-tissue corpuscles, forming the reticulum with their fine fibres. The spindles are generally arranged lengthwise, with parallel course towards the epithelium of the glands and of the surface, thus producing a membranous boundary between stroma and epithelia. The latter rest immediately upon this connective-tissue basis, and the fibres of the latter may sometimes be traced between the epithelial cells.

The connective-tissue corpuscles present various appearances—all shapes from spindle-like to round, accordingly as they are cut cross, obliquely, or longitudinally. The existence of the granules I explain in this way: if the top of such a spindle, or its offshoot—sometimes fine granular for a short distance—is cut transversely, we obtain the impression of granules, which we have previously mentioned, and which, by their small size, appeared strange to us.

This peculiarity of the structure of the mucosa continues for a short distance between the muscular interstices.

The vessels ascend into the mucosa through these inter-

stices; they have thick walls, a winding course, and terminate beneath the surface epithelium in very fine capillaries.

Spindles with filaments, offshoots of the reticulum, are attached to the walls of the vessels, parallel to the course of which they are generally arranged. My remarks on the normal structure of the lining of the uterus, although I have avoided discussing the opinions of other authors, have been rendered more at length than I intended. But in order to understand the morbid conditions of a tissue, we must first have a clear view of its normal state, and hence I repeat: The mucous membrane of the uterus appears as a delicate connective-tissue reticulum, composed of spindle-shaped cells with their offshoots. This reticulum is imbedded in a light, homogeneous intercellular substance, in such a manner that the filaments and intercellular substance, or where the former are wanting, the latter alone, form trabeculæ, now broad, now very scanty, passing between the numerous cellular elements and separating them from each other.¹

The spindles of the network are placed around the vessels lengthwise, and parallel with their course; they present the same feature toward the glands, and form a basis upon which the epithelia, both of the glands and of the surface, immediately rest.

All the changes to which connective tissue and epithelium are liable, may also occur in the mucosa of the uterus. Its structure has often been compared with a lymphatic gland, and named adenoid, on account of the very numerous lymph-corpuscles contained in the reticulum and in the intercellular substance. This comparison holds good in considering the pathological disturbances, not only as to their microscopic appearance, but also as to their clinical course. Even up to this day, the term lymphosarcoma, or lymphadenoma, includes several lymphatic glandular tumors, which, from a histological point of view, have the same or a very similar structure, while clinically they run a very different course. In our observations we meet with a chronic hyperplasia of the uterine mucosa, which has been dwelt upon in detail, first by French authors, especially Recamier, and of late by Olshausen.²

¹ The ciliæ are omitted in the drawing.

² Olshausen: "Ueber die chronische hyperplasirende Endometritis." Arch. für Gyn. u. Geburtshülfe, viii.

It is often difficult to decide as to the benign or malignant character of vegetations of small cellular feature, nor is it always easy to tell where chronic inflammation terminates and new formation begins. What gynecologist has not met with cases of diffused vegetations of the interior of the uterus, where the examination of scrapings showed a structure not essentially different from the normal mucosa? In two cases,

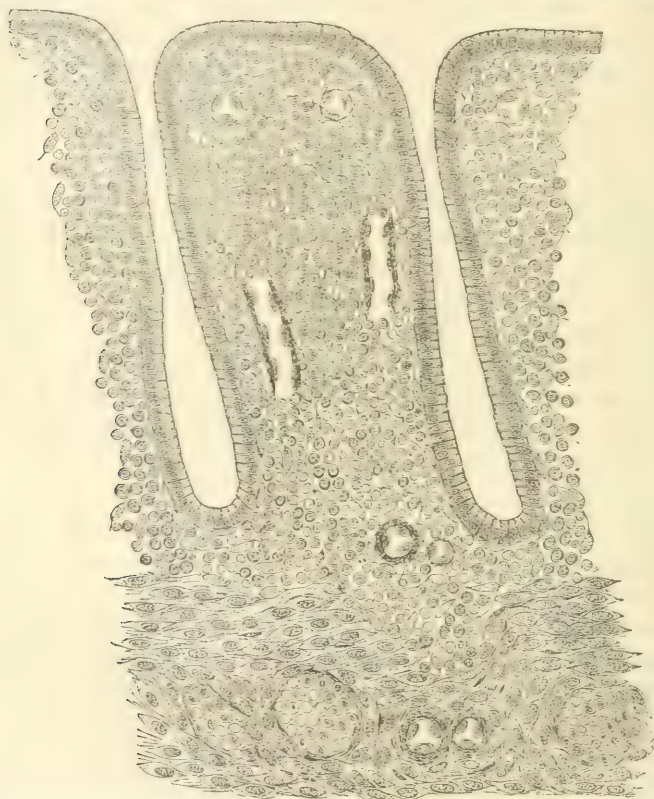


FIG. 1.—Normal mucous membrane, longitudinal section. (The ciliae are omitted in the drawing.)

perhaps, he may find the same thickening of the reticulum, the same increase in the number of the lymph-corpuscles; *i. e.*, chronic hyperplasia; but in the one case recovery may ensue at once, or after repeated curetting; while in the other incessant relapses occur till death. Several cases of Olshausen undoubtedly create the impression of malignity. Why is it.

that one tumor of a lymphatic gland has no pernicious influence on the constitution of the patient, that another produces fatal secondary growths, and that a third is connected with an important change in the blood, called leukemia? All, or the majority of such growths of the lymph-ganglia, present the same or a resembling formation, so that we have no criterion, besides the clinical course, according to which this variety of tumors may be classified. Here a barrier is raised to investigation, which will possibly be removed when we have advanced as much in the knowledge of albumen, and protoplasmic changes from a qualitative, chemical point of view, as we have in the knowledge of the forms, the physical properties, and the changes of cells.

After the above remarks, it will not appear strange, if in the following survey of the growths, I include the chronic inflammatory vegetations :

- (a) Chronic hyperplasia.
- (b) Lympho-sarcomatous growth of the mucosa.
- (c) Myxomatous growth.
- (d) Sarcomatous growth.
- (e) Adenomatous growth.
- (f) Papillomatous growth.
- (g) Carcinomatous growth.

These growths may appear diffused or circumscribed; they may appear at first diffused, and still give rise to tumor formations in limited places; finally, a tumor formation may exist, at first circumscribed, and then gradually extend itself over the whole lining membrane.

It is not my intention to discuss these various forms in their order. For an exhaustive review of each of these groups can only be accomplished with a large amount of material, with exact clinical observation of the disease, and a complete microscopic investigation. With these requisites, Ruge and Veit have successfully investigated cancer of the uterus. I prefer the easier and reverse method, *i. e.*, examining the microscopic examinations of scraped off pieces of the mucous membrane, and seeing under which of the above-named categories the morbid occurrences can be placed. A strict classification is frequently difficult; indeed, even the first case related below

shows us how important changes of the epithelium and of the inter-glandular tissue may take place at the same time.

CASE I.—Relates to a woman suffering from metrorrhagia, not due to a puerperal cause. The mucous membrane was found covered with fungous, partly villous masses, which, when removed and examined microscopically, showed the following:

At places where the features were nearest to the normal, two changes of the interglandular tissue were striking: thickening of the walls of the blood-vessels, and around the vessels a concentric layer of either still normal-sized or enlarged lymphoid and spindle-shaped cells. The lumen of the vessels was encroached upon, the endothelial cells were multiplied, enlarged, and extending far into the lumen, so that the smallest vessels and capillaries seemed to be entirely filled with endothelial cells. The adventitia was thickened, its cells and those spindle-shaped cells which, with their offshoots, were described as a continuation of the stroma, had also increased in volume. They occupied meshes of a delicate reticulum, into which the adventitia had spread, and which blended with the reticulum of the stroma. But the views presented were different at different places. At certain parts approaching more to the normal condition, the enlarged cells were placed close together, separated only by a very scanty homogeneous intercellular basis-substance which was traversed here and there by distinctly visible filaments. The cells were distinguished by a large, coarsely granular nucleus, with two and more well-defined nucleoli; the protoplasmic rim was more finely granular and broader. At other places, representing the more advanced stages, the finely striated nature of the intercellular substance appeared in a more prominent degree. Here was a delicate network with thickened connective-tissue trabeculæ, inclosing enlarged cells of different shapes. For the most part, each mesh was filled by one cell; occasionally a hollow space was discovered filled with a finely granular mass containing two or more nuclei.

The forms of the cell bodies were wholly irregular, angular, polygonal, or lengthened; the nucleus, on the contrary, was either round or oval; but in all cases the cell formations were distinguished by means of their increased, epithelioid character. Hence, the enlargement of the cells, the thickening of the connective-tissue trabeculæ, these were the disturbances of the interglandular tissue. In what degree the separate elements had partaken in the growth could not be stated with certainty. The blood-vessels, endothelial and adventitial cells evidently had taken an important part; the cells of the adenoid tissue—both round and spindle-shaped—were also in a state of exuberant growth and distinguished by their increased volume; finally the tissue had changed into a thickened network, which inclosed in its meshes polymorphous, epithelioid cells.

The appearance of the utricular glands proved to be very interesting. They were scarcely increased in number, but their lumen was distended and their boundary line generally well marked by

thickened connective-tissue bundles with enlarged spindles. On this boundary layer the epithelial cells rested in a state of irritation and growth. They were enlarged and furnished with a large nucleus and several shining nucleoli. At some places features of separation were recognizable: nuclei with constrictions, epithelia with two nuclei, while the protoplasm was traversed by light lines. Here the columnar character of the epithelium seemed to be changed and its place taken by multiform cells.

Most interest was aroused by those places, where the epithelium of a limited part of the gland invaded the neighboring stroma. While the boundary wall was sharply defined around most of the periphery of the gland, it was at this place (comparable with a hedge broken down for a short distance), absent and replaced by the epithelium growing and extending into the surrounding sarcomatous tissue. The columnar type of the epithelium was preserved for a short distance and then replaced by polymorphous epithelial cells.

I think that I am justified in calling this growth Adeno-Sarcoma: Adenoma, because the glands were distended, increased in number, although in a small degree, with their lumina and their boundary lines generally well preserved; sarcoma, especially reticular sarcoma, on account of the peculiar changes of the interglandular tissue.

Of course this variety represents but one of the many possible sarcomatous changes of the mucous membrane. ^{These mucous} frequently the various kinds of sarcoma—round and spindle of the sarcoma, large and small cell varieties—occur, is for the most part a matter of speculation. We must call attention to the behavior of the glands and their epithelia during the interglandular changes. The glands must not be expected to remain passive during such excessive changes in the surrounding tissue, from which the boundary line—the basal membrane—serving as a basis for the epithelium to rest on, and sending fine offshoots between the epithelia, is produced. The above communication confirms this conclusion and tends to prove that a sarcomatous growth affects to a greater or less degree the glands and the epithelium.

But an etiological point should not be left out of consideration. After the discharge of the placenta and membranes, that portion of the decidua which consists of the open, enlarged glandular cavities, together with the large cellular interglandular tissue, remains attached to the walls of the uterus. After a shorter or longer time the *restitutio ad integrum* proceeds from both elements. The growth of the interglandular

tissue which follows the puerperal period, gives us quite other hints for judging of the participation of the glands in the changes than those given by the disturbances occurring in an intact mucosa. Distended glandular remnants, cystic cavities, lined with epithelium, are already present, and that sarcomatous growths should combine with glandular and epithelial

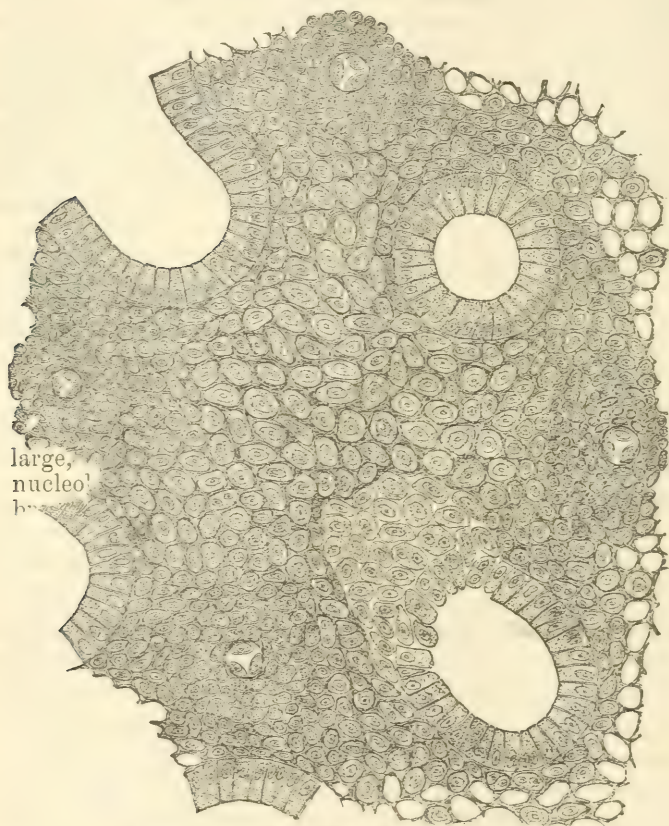


FIG. 2.—Adeno-sarcoma with reticular structure.

changes on such a base, rather than on an intact mucosa, appears evident.

While the above case offered us clinically a diffuse growth and microscopically a reticular adeno-sarcoma, the following specimen revealed a different state, although the glands in an equally significant manner had undergone various disturbances.

CASE II.—The case is that of an unmarried lady, about 25 years old, who suffered profuse hemorrhages from the uterus, combined with severe pains in the back and in the lower part of the abdomen. The physical examination showed a tumor extending from the cavity of the uterus partly into the vagina. After removal, the mucous membrane was found to be changed into a soft, fungous, more or less prominent mass, which covered the whole surface of the uterus, and gave rise to a tumor of about the size of two fists. It was soft, and presented on section the appearance of a medullary growth. Microscopically it was recognized as adeno-myxosarcoma of the lining membrane, presenting partially the characters of a well-defined spindle-cell sarcoma. In a jelly-like, translucent intercellular substance, spindle-shaped and very numerous small round cells were imbedded. The former with their offshoots formed a netting; the latter filled its meshes. At many places the globular cells were so prevalent as to appear like those of a small round-cell sarcoma; only scanty trabeculae of a jelly-like, shining, intercellular substance, in which fine black filaments could frequently be traced, separated them from each other. The cells were about the size of a white blood-corpuscle, although at some places larger ones—three or four times the size—could be seen.

There was no change in the vessels; but the glands showed disturbances, which I dwell upon below.

This sarcomatous growth, mainly consisting of a reticular myxomatous tissue with numerous round cells, invaded the muscular wall and proceeded between bundles of spindle-shaped structure. A participation of the fibrous muscular elements of the uterine wall could not be denied. My explanation for this occurrence is that the proliferation originating from the lining membrane, proceeding and invading the muscular wall, gave rise to a secondary growth in the latter. The changes originating from the mucosa and from the muscular wall could easily be distinguished by their different colors and their different histological features. While the growth of the mucous membrane showed a more yellow, dark color and a reticular structure, that of the muscular wall appeared lighter and with spindle-shaped elements. The spindles took an almost parallel course, and were imbedded in a homogeneous translucent intercellular substance. Sometimes this was replaced by a fine granular mass or the spindle-cells, spreading apart, included meshes filled with round cells, so that, especially close to the mucosa, the round and spindle cells participated equally in the composition of the tumor.

The glands were increased in number, though not in a striking manner, and distended in this changed tissue. They presented cavities lined with enlarged epithelial cells, which at some places showed features of separation and proliferation. Sometimes such cavities were in want of an epithelial covering, and, as remnants of the glands, cysts containing a finely granular mass were found. In most cases the glands were well-defined, because the spindles bounding them and in their neighborhood took a parallel course,

thus forming small bundles of spindle cells, which were separated by a lucid homogeneous or finely granular basis substance. Several groups of epithelial cells in the midst of the sarcomatous tissue made an exception to this rule. They presented themselves either from the side or from above, and evidently were remnants of glands, the boundary walls of which had been transformed into sarcomatous tissue. At some few places the epithelial proliferation extending into the surrounding sarcomatous tissue was again perceptible. While the gland was accurately defined for about three-fourths of its periphery, the remaining one-fourth of the wall was missing and replaced by the epithelial growth invading the stroma.

After having described two varieties of sarcoma of the uterine membrane, I deem it proper to explain with a few brief remarks the various possible growths of the mucosa and their development. The glands may grow and increase in number, the interglandular tissue remaining inactive, in some places being similar to the normal, in others becoming infiltrated with lymph-corpuscles, but for the most parts they are atrophied and compressed by the new formations. The cases of glandular hyperplasia—adenoma—are of no rare occurrence in this territory. The glands are closely crowded together, coiled and worm-like they fill the entire field, and are separated in many places only by very narrow trabeculæ of either normal adenoid or dense connective tissue, all having well-preserved, either very confined or dilated lumen (see Fig. 3).

If the growth is excessive, invading both the mucosa and the muscular wall, we style it an *adenoma destruens* or *malignum*.

If the surface or glandular epithelium grows, dips into the stroma and fills the glands, thus forming alveoli or nests, we have a cancer. The interglandular tissue takes an active part in these changes, either exhibiting a great number of lymph-corpuscles or producing epithelial cells. Finally the interglandular tissue grows excessively (I refer chiefly to sarcomatous disturbances). In this case it would be erroneous to imagine the glands to be compressed, perhaps indicating their former existence by narrow spaces. On the contrary, in many cases they are also in a state of irritation and proliferation and reveal different features. They may be multiplied and may present enlarged cavities—cysts—generally with sharply-marked outlines, the epithelium being absent or well preserved. The latter can give rise to papillary elevations extending into the

lumen; it can occasionally grow and invade the neighboring tissue.

This epithelial proliferation, confined to a limited space, does not alter the diagnosis sarcoma, since not a single spot but the general features decide.

It cannot appear strange that the structure was reticular in both the cases related, as the histological feature of a growth accommodates itself to that of the tissue from which it origi-

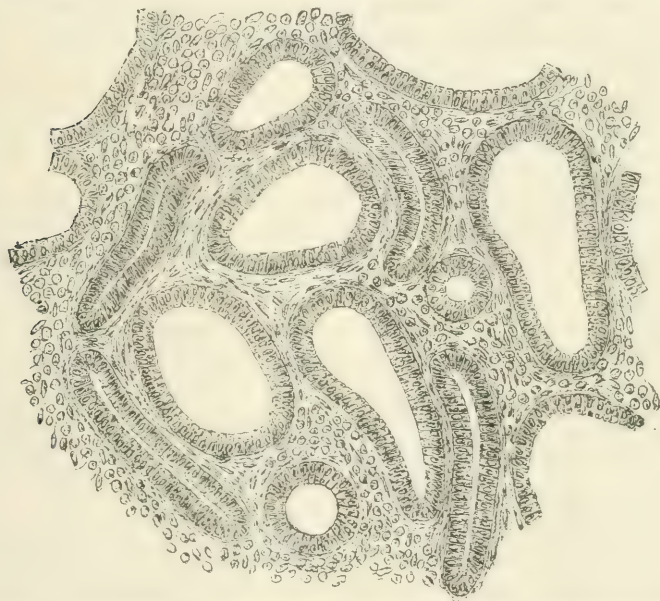


FIG. 3.—Adenoma from a polypus of the mucosa uteri. I have added this specimen to show the changes of the interglandular tissue in comparison with the preceding.

nated. Lympho-sarcoma, myxo-sarcoma, reticular sarcoma, in comparison with spindle-cell sarcoma may be more prevalent.

The papilloma requires a special explanation. The true form, viz., those papillary excrescences which consist of connective tissue and an epithelial covering, are very rare occurrences in the mucosa uteri; these growths for the most part originate from the portio. On the other hand, combinations with adenoma and cysts are very frequent, and represent a great many of the polypi of the mucous membrane. Also a papilloma in a secondary change may take the character of sarcoma or cancer.

Slavjansky describes a papillary myxomatous vegetation of the mucous membrane of the uterine body as *Métrite interne vilieuse*.

While we have declared adenoma, carcinoma, and sarcoma to be the principal forms of the growths of the mucous membrane, their combinations must not be lost sight of. These are of frequent occurrence and can be classified under adeno-sarcoma and adeno-carcinoma. The former we have already described in detail. Whether an exclusive sarcomatous growth can occur without any participation of the glands I am unable to decide.

The latter shows at some places an increase in the number of glands, at others proliferation of epithelial cells, filling the glandular cavities and dipping into the stroma, finally alveoli or nests and cords of epithelia without glandular structure. Even Ruge and Veit maintain the development of cancer originating from connective tissue and secondarily affecting the glands.

These discussions touch still another question, much mooted of late years: Is epithelium only produced from epithelium, and is not an origin from connective tissue admissible? I will discuss the theory of the germinal layers, first established by Remak and His, and applied by Thiersch, Waldeyer, and Billroth to the explanation of the development of tumors, later in the description of cancer. However, in my opinion, the examination of the cancerous degeneration of the mucous membrane of the uterus, its development and course, forces the observer who is not from the beginning prejudiced by the idea of a strict boundary between epithelial and connective tissue cells to the conclusion that epithelium can be derived from connective tissue. This conclusion appears spontaneously, and must first be set aside if we wish to conform the morbid growth to the new theory. The question under discussion has not been yet decided, and it will not be solved until Virchow's views have again been acknowledged. Its history, however, is even now interesting.

In order not to exceed the limits of my paper, I consider it sufficient to communicate the following sketch: Virchow's opinion that connective-tissue corpuscles may produce epithelial cells was opposed by several authors, who maintained the

view that epithelial cells may be produced only from epithelial cells. The theory of the germinal layers, according to which the epithelia are always offsprings of the upper and under germinal layer, while the endothelia are derived from the middle layer, especially from the connective tissue, was applied to the growth of tumors, first to cancer of the skin, and later to all cancers. Some authors immediately accepted the new theory, others took an intermediate, and still another party an opposing position. Essays pro and con were published, discussions of detailed questions, description of tumors followed, which presented the same structure, and were considered by some as epithelial, by others as connective-tissue growths (plexiform angioma).

For a time the new theory seemed to prevail, but of late some investigators have raised their voices in favor of the old. Time will settle this matter, which as yet cannot be regarded as at rest. Meanwhile, we enjoy the benefits and the fruits of this controversy. For this idea, supported and defended by a body of excellent men, acting as a stimulant, has called forth a great number of prominent works. And even though it be erroneous, it has exercised a beneficial influence on science.

A few words remain to be said concerning the prognosis and therapeutics of sarcomatous growths. Obstinate, recurrent hemorrhages will, in the absences of other diseases—tumors, displacements, erosions, etc.—always induce the physician to examine the condition of the mucous membrane of the uterus. If this is found to be thickened and covered with fungosities, a microscopic investigation of the scraped off particles will be the most reliable aid to diagnosis. In case there is a proliferation of small round-celled structure, we are naturally forced to conform our decision to the clinical course. We would pronounce the prognosis favorable if, after a thorough curetting, the abnormal condition should cease and the patient's state of health improve; we should consider it unfavorable if, in spite of repeated removals, the hemorrhages should continue, while the patient's constitution should break down and become cachectic. Whether in this case the fungosities retain their small round-cell character; how the vessels and glands behave in the course of the disease—in short, whether the attempt to find distinguishing features between

chronic inflammatory growths and small round-cell sarcoma (we will style it lympho-sarcomatous degeneration of the mucosa) will succeed—remains to be decided by further examination.

Where the diagnosis clearly indicates sarcoma, there can be no uncertainty as to the prognosis. This, therefore, is unfavorable in both of the above-mentioned cases. The first has not yet reached its conclusion; in the second, a short time after the removal of the tumor, the cavity of the uterus was refilled with new growths, the vaginal wall at some places was covered with rapidly growing tumors; at present (three months after the operation) the patient is in such a condition that death will soon occur.

In cases of such sarcomatous growths, originating from the mucous membrane, may they be diffused or circumscribed, removal of the tumor or curetting of the mucosa is of course of very doubtful therapeutic value; granted it were possible in this way even thoroughly to remove the lining membrane, still the intermuscular connective tissues, the direction on which the growth creeps further, cannot be reached with the curette.

The total extirpation of the uterus is the only therapeutic method guaranteeing us some degree of success, and yet even then local infection or transportation to other organs will often defeat our hopes, and lead to a fatal termination.

318 E. NINTH STREET.

THE IMPORTANCE OF *IMMEDIATE* POST-PARTUM EXAMINATION OF THE PERINEUM IN *EVERY* CASE OF LABOR AND, WHEN LACERATED, ITS TREATMENT BY *ONE SUTURE*.

BY

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THE days of dogmatic inhibition in medical science are rapidly losing their light, and I think we may safely say, are drawing to a close. The light which pathology has shed upon what some few years ago appeared as complex beyond all un-

derstanding, or what was still worse, an unquestioned assumption or a *point d'appui* upon which whole sections of the profession were moved in darkness, brings prominently before us what the world owes to investigators in that branch of the science.

Notwithstanding, however true all this may be, we find even to-day such men as Charpentier, the author of the most recent work on obstetrics, treating the subject of "recently lacerated perineum" in a manner which few even of his own countrymen can indorse, and which might possibly do harm from the eminence of the author. Dr. Charpentier is opposed to the immediate repair; he advises simply to use faintly-antiseptic wet compresses to be applied to the parts, and the legs to be tied together. This method of treatment can hardly be said to be in accord with the "aim of a classical treatise upon a par with the modern art," his apology for writing his book—as his great grandsire might have been able to have told us all this. Dr. C. says, when union does not take place from this treatment, he prefers to do the operation after the lapse of five or six months, because at that time complete involution has taken place; a condition, I will endeavor to show further on, which tends to lessen the chances of success more than if an operation had been done before involution began. Before discussing the question further in this strain, we must start with the fundamental proposition—to *prove the advantage of the immediate over the remote operation*. To do this, we must seek to attain a fairly accurate knowledge of the conditions which, separately or combined, threaten to work or are working disorder in the puerpura. To thoroughly appreciate the knowledge, we cannot do better than take a glance at the absorbent structures or system of the female pelvis. On this point I will quote from Dr. Mundé's late memoir in the October number, 1883, of the *AM. JOURN. OF OBSTET.* He says: "The lymphatic system of the female pelvis is then seen to consist of an immense intricate network of vessels opening on the free surfaces of the mucous membranes lining the uterus and bladder, and on the peritoneum (and probably also in the cellular tissue) by means of innumerable minute orifices, and converging thence into larger canals with numerous peculiar pouch-like sacculations, which cover and intersect all the pelvic organs, finally meeting in the

still larger ducts leading to the ductus thoracicus. . . . It should be stated that the ovaries and tubes are equally well supplied with lymphatics as the uterus, *but that the vaginal mucous membrane possesses a comparatively slight absorbent power.* Only when the epithelium is removed or *the deeper tissues are exposed, as in fresh fissures*, are the lymphatic vessels enveloping the vaginal tube laid open to external impressions."

I have made the above quotation not only because it deserves being well impressed, but also because its teachings bear upon my views, in so much that they show a lacerated perineum to be an extensive surface laid open like the leaves of a book partially extended, and highly endowed with absorbent vessels. So much for the parts concerned in the work of reproduction. Let us now take a glance at what takes place during pregnancy and the puerperium. Before parturition we have an active building-up process in force, and everything is subservient to this work. When, however, this is complete, the reverse process of demolition and carrying away of refuse material is begun. Absorption and excretion are the ruling energies here, and everything becomes subservient to them. If all goes well, this refuse matter is absorbed and duly excreted by the skin, bowels, kidneys, lungs, etc.; but if any interruption takes place in this process, we have not only retention in the blood of these poisonous waste elements, but we also have arrest of normal change and absorption of waste cell elements at the seat of injury. These elements rapidly, on exposure to air, undergo putrid decomposition, and forthwith a poison (sepsin) is generated and taken up by the very lymphatics described by Dr. Mundé, *and not by the veins, as was erroneously supposed.* What happens now? About the third or fourth day, or probably a day later after delivery, we receive the very unpleasant information that Mrs. So-and-so is not so well; she has had a chill, is suffering some abdominal pain (perhaps), and is in a high fever with intense headache; she would like to see the doctor at once. We are now somewhat alarmed and do what we should have done immediately after delivery—make an examination of the perineum WITH OUR EYES. To our amazement we find the perineum and posterior wall of the vagina lacerated to a point much higher up than we can see with the aid of our fingers to open the parts, and the whole of

this wounded surface, perhaps, covered with an ash-colored slough giving exit to a profuse foetid, purulent discharge. Here we have a case of acute puerperal lymphangitis, and not phlebitis, as was formerly taught. Championnière, of Paris, has clearly shown that the lymphatic vessels of the pelvis are the real carriers of pus and poison during the puerperal state as well as the non-puerperal, and that we should no longer attribute any such office to the veins.

That a septic wound, occurring in the parts in question, should not differ in point of pathological significance from a similar wound occurring in any other part of the body is evident. On the surface of one of the limbs for instance, giving distinct red lines up the limb, indicating the course of the inflamed lymphatics towards the centre of the body, we would expect, and really have, the same symptoms as occur to our puerperal patient, differing only in point of locality, and the seriousness of the puerperal state. Two very interesting cases occurred to me some years ago which illustrated such a condition, as follows :

CASE I.—In 1878 I attended a primipara aged twenty-six, and delivered her with Simpson's long forceps. The perineum was not examined, as it was thought when the head extended over the part, it did so without laceration. Everything went well until the morning of the third day after delivery. I was then informed she was not so well, had no particular pain even on pressure over the uterus, but had had a chill. I found her with a handkerchief tied tightly around her forehead to ease an intense headache. This patient had been fairly well attended to since delivery, and had daily injections of Condyl's fluid and linen changed regularly. The injections were now increased in frequency, and twenty-grain doses of quinine given daily for the following three days. During this time her temperature ranged from 103° to 105° F. Pulse 120, almost steadily. On the seventh day of the puerperium I thought I would examine the perineum, as she seemed to be very tender when the vaginal injection-tube was inserted. Here we had a fearful state of affairs, the perineum was torn down to the sphincter and the vagina lacerated in its posterior wall as far as I could see with the aid of my fingers to separate the parts. The edges of this wound were separated by the raised edematous recto-vaginal loose fascia, or cellular tissue, and the whole of this surface was giving exit to brownish-yellow fetid discharge, and studded here and there with what appeared to me at the time to be distinct patches of diphtheritic false membrane. These patches or islands were adherent to the base membrane and would bleed on being disturbed. The same treat-

ment was continued until next morning, when the patients complained of a sore throat. Here, on examination, was found a distinctive pale exudation which covered the soft palate and tonsil, in their anterior part. Her urine was drawn off by a catheter carefully and found highly albuminous. One of the younger children in the house was also attacked with faucial diphtheria about this time. There was now no doubt about the cause of the septicemia. She was put upon Bell's mixture and had an intrauterine injection three times a day of Condy's fluid. This treatment was continued with some unimportant additions until the tenth day, when she was moved to the general hospital, it being thought that the locality of her present abode was highly infected with the specific poison-germs. Her temperature, when I left her in hospital was 106° F. in the axilla, and her pulse was 160. She here went through a very severe siege. The poison ceased to be generated at the seat of injury. The inflammation of the lymphatics assumed a subacute character, and edematous infiltration of the pelvic cellular tissue remained. She continued in a state of confirmed invalidism for several months and ultimately gradually recovered.

CASE II.—The second case bearing upon this subject was that of a primipara aged twenty-three whom I attended in the following January (1879). This was also a forceps case. After delivery it was noticed that there was what appeared to be a slight laceration of the perineum. The legs of the patient were tied together, and the usual antiseptic precautions adopted. All went well until early on the morning of the fourth day, when I was summoned. I found her recovering from a severe rigor, which I was informed had lasted for more than half an hour. She complained of severe pain over the region of the broad ligaments and fundus uteri, severe headache, and some vomiting. Her temperature was 105° F.; pulse 120. From my experience in the last case, I now made an examination of the perineum and found it in a somewhat similar condition. There was a fetid discharge, and patches of a creamy yellow color situated on the lacerated parts which extended high up on the posterior vaginal wall. These patches had a distinct raised membranous appearance, surrounded by a red erythematous edging, gradually fading away into the adjacent parts. I became now convinced that I had another case of sepsis, due to a lacerated parturient canal being attacked by diphtheria germs. To be brief, the same treatment was carried out here as in the former case, and after a severe siege the patient recovered sufficiently to have been considered convalescent on the fifteenth day of the delivery. This was on the 4th of February, and on the 10th I was sent for to explain the cause of a soreness in the vaginal passage and a continued discharge therefrom.

On examination I found on the right posterior wall of the vagina, on a level with the external os, an opening through which I could pass a soft silver probe downwards and backwards in the direction of the perineum for a distance of about one inch. On

the opposite side of the vaginal wall, at a little lower level, I found another opening which seemed to pass upwards and forwards for a short distance. In the course of two weeks the opening on the left side closed completely, but the one on the right side continued its course downwards towards the integument covering the perineum, where a counter opening was made, and "through drainage" established by means of a soft-rubber tube. This tube was withdrawn in ten days; recovery rapidly followed. I may say here that this patient has been ever since a sort of semi-invalid from the effect of retroversion and other uterine troubles. I have used all my endeavors to induce her to allow me to repair her perineum to relieve her recto- and cystocele, both of which are pronounced, but to no avail. She declares she will suffer death before she will go through again what she did in the former instance—*sic transit gloria*.

One of the moral bearings in connection with these two cases is, that we are very likely to have, in any case of lacerated perineum, sepsis, and that the danger of the wound being so attacked will be much greater, should the air be contaminated to the slightest degree by germs of diphtheria, scarlatina, erysipelas, etc. And I hold that it is utterly impossible for us to foretell whether we shall have to deal with these micro-organisms or not, as they float in the air of places apparently the least likely, of all others, to give them a footing.

In both of these cases it must also be remembered that antiseptic precautions were taken and cleanliness enjoined from the moment of delivery; but the fact that there was an extensive area of absorbent vessels left unprotected and exposed to the influences of organisms floating in the air was altogether overlooked. Should such lesions as I have described in these cases have occurred upon the exterior of the body, and come under the care of the surgeon, they would cause sufficient anxiety on his part to warrant the careful coaptation of the wound-surfaces, and the carrying-out of the most accurate principles of antiseptic surgery. But the surgeon is placed differently, from a social standpoint, to that in which the obstetric physician is; what the former sees the public sees; he is exposed to criticism, and feels the necessity of exerting himself to the utmost on point of self-defence. But the obstetric physician, on the other hand, is not expected by the public to recognize, or assume, that there may possibly be such a thing as a wound of the parts through which the child has been evolved. He may know that such a condition may possibly exist in a given

case, but fears to discover it, lest the necessity for an operation will become apparent and involve an explanation to the relations of the patient. He probably has not done the operation before, and feels that to apply three or four sutures to a ruptured perineum is altogether out of his sphere; it properly belongs to the gynecologist, and, will therefore require the summoning of aid. Modest, easy-going physicians hate "a scene" at the bedside, and here we can truly sympathize with such a one, in such a position. He has not only to admit his inability to finish the task for which he had been engaged, but will also allow the impression to go abroad that he has been unskilful in his undertaking, and was directly or indirectly the cause of the injury. This is altogether too much for him, diplomacy comes to the rescue, there may be a rent, or there may not be, he will take the chance; and any way, some *very respectable authors* disclaim the advisability of immediate interference under such circumstances. He ties the knees together and tells his patient she is all right, and when on the fourth or fifth day she experiences a chill followed by a high fever, etc., she has caught cold, probably from indiscretion on the part of an attendant in allowing a door and window to have remained incompletely closed a few moments too long. In fact, some one can recollect distinctly that there had been a draught blowing over the patient. All this is accepted in its fullest extent by the anxious physician, his reputation is safe, and he will now do his best for the patient. Quinine and antiseptic vaginal injections are administered by *the nurse*. Matters do not seem to improve, and after the lapse of a few days the poison has reached, and is being absorbed from the slightly lacerated cervix. The symptoms now become more aggravated, and a consultation is thought necessary and held. Intrauterine injections are recommended and carried out by the attending physician. After each injection there is an improvement, still matters are not clearing up satisfactorily. The patient declares she feels "splendid" (a very bad omen), temperature in axilla may fall, but pulse becomes very rapid and flickering; septic peritonitis, the end comes, she caught cold, blood-poisoning set in, it was the will of God, and every one is satisfied.

Every physician who has had trouble in his early obstetric days can recall vividly the truthfulness of this picture. It is

not one we can easily forget ; but, for all that, it should not have a cloak thrown over it. I have spoken of some very respectable authors of the present day who are in favor of the let-alone course in lacerated perineum cases, and I must say that it appears to me to be a folly highly censurable in these men, high in authority and experience, to persistently act as scientific obstructionists, and I feel here the temptation to recall the words of Dr. Robert Barnes when speaking upon another subject : "Those who have studied the history of obstetric doctrine cannot fail to see that this dread of encouraging enterprise in practice, lest disaster should result from unskilfulness, has cramped the teaching, obstructed the progress of knowledge, and enforced a slavishly timid, yet barbarous practice, which still persists down to the present time. Smellie and Denman taught men with the same feeling of reserve which we should still teach midwives. But, surely, the day is past for all this. We may safely venture to teach men of a higher standard upon more liberal principles. I am not aware that a similar reticence or restraint has at any time, to a like extent, gagged the teachers of medicine or surgery proper. May we not see in this fact a striking testimony that the practice of obstetrics demands, even more than medicine and surgery, steadiness, yet promptitude in judgment, courage under difficulties, and physical skill ?" So much for this great original writer, and I hope my readers will think, as I do, that the justification of this operation rests upon the same basis as that from which the whole art of medicine derives its authority : its design and general effects are to save life. I think, however, that the moral aspect of the question is becoming rapidly changed with the more practical section of the profession, and it will rest with those who leave a perineum after delivery unexamined, and unrepaired when lacerated, to justify their neglect.

Up to the present, we have been looking at the dark side. Let us now take a view of the bright. It will be remembered that Dr. Charpentier gave as one of his reasons for preferring the secondary operation that he saw advantages to be obtained by waiting until involution had been completed. In this, it will be seen, we have a condition the very reverse of that which we want ; for it is well known that one of the chief difficulties we have to contend against in obtaining success in the secondary

operation is tension or lateral traction of the opposing sides, causing the sutures to cut out. It is also known that, in the operation for the repair of a vesico-vaginal fistula, Dr. Bozeman, of New York, used the graduated pressure (intravaginal) of sponge bags, prior to doing the operation, with the view of relieving tension, by dilatation, breaking up adhesions, etc., etc. Now, after delivery we have the very condition—extreme dilatation—we most desire, the head, in its passage through the canal, doing what Bozeman did by art, and thus rendering tension upon sutures inserted *immediately* after delivery out of the question. There is still another important demand for immediate action. It has been shown that, during the period of time occupied by pregnancy, a general building-up of the tissues is the ruling energy at work, and that, after delivery is completed, we have a gradual process of demolition of these built-up tissues, followed by their absorption and excretion. There is, however, I believe, an intermediate period of *rest*, or one in which things remain *in statu quo*, between delivery and the beginning of absorption; and this period lasts from two to three days. My reason for suggesting that this period of rest really *does* exist is based upon the well-known clinical fact that we rarely or ever see a case of puerperal sepsis manifesting itself as such before the third or fourth day after delivery. There are, of course, exceptions, but this has been my experience even where I had good reason to believe the poison had been resident in the house of the patient at the time of delivery. Dr. Robert Barnes has also pointed out this fact in some of his recent writings. It will now be seen what advantage we will gain towards a success in the repair of a lacerated perineum, by taking "*time by the forelock*" and utilizing this period of *rest* to obtain a certain amount of plastic union between the torn surfaces, before the absorption process sets in. As regards the truthfulness of this latter fact, I have sought for and found this union to be sufficiently firm on the third day to warrant a feeling of being satisfied that it would become permanent, which it did in every case.

It has been said that primary union has been known to have taken place by merely tying the legs together. This, I believe, is simply an accidental perversion of the truth, and I think all experienced surgeons will see the error in such a statement.

We see the wound cicatrized over, but this is a very different thing from primary union or cohesion of the opposing torn sides of the parts involved. There are, however, cases, many of which have come under my own observation, which have had the perineum torn right down to the sphincter, and even through it in a few cases, and these patients were apparently quite well and able to perform their household duties after a fashion. But in regard to these very patients there arises a question. At what period of life were they seen? If before middle-life, when the pelvis is well furnished with cellular-tissue padding and ligamentous supports of good tone, all is well. But if seen towards the end of life, when this padding has long since disappeared, and the ligamentous support has lost its tone, we see a different object—a wretched creature worn out with long suffering from having for years been compelled to wear her womb between her legs, instead of within the pelvis. And here I have recalled to mind a piece of advice I once received from a famous old Scotch surgeon: “Whatever you do, sir, see that you do not leave cripples after you; they tell unpleasant tales.”

In illustration of the advantages obtained by the immediate operation which I have been urging, I will relate two very interesting cases. One where the immediate operation was performed, and the other, a lesion of like severity, in which it was not done until five years had elapsed.

In February, 1881, I attended a young primipara. It was an occip.-post. position. Ether was administered, and the long forceps applied. During steady traction the head, suddenly and without warning, slipped under the pubic arch, and a violent pain being in force at the time, shot the head out with such violence that, on inspection, the perineum, sphincter ani, and recto-vaginal wall for two and a half inches up the canal, were lacerated. This was certainly the severest lesion I had ever seen, and gave both myself and friend a slight shock. However, we sponged the parts well with carbolized warm water, and passed a large sponge well up the passage to dam the flow from the uterus, so that we would obtain a proper view of the injury. The edges of the tear were very uneven and jagged, but there was scarcely any hemorrhage from them. In the vaginal part of the tear I put four common gray-thread sutures, tying the ends in the rectum. On arriving at the perineum, I first passed Emmet's sphincter suture, and then three more, leading up to the beginning of the

¹ Full length of my index finger.

rent. The sponge was removed, and the passage well washed out with carbolized hot water, and the patient placed comfortably on her side in bed. The carbolized injections were repeated three times daily for the first week. On the fourth day a troublesome diarrhea set in; the contents of bowel passing through between the suture points into the vagina. The patient now became very much depressed in mind when she found she could not exercise control over her motions. On the fifteenth day I removed all the perineal sutures, except the one controlling the sphincter. This, and all of the vaginal, I removed on the eighteenth day after delivery. There was still a small recto-vaginal fistula just in front of the sphincter, through which fecal matter passed, but without any local treatment this healed completely towards the end of the following week. This patient has been examined on two or three occasions since by me, and a more perfect perineum she could not have, and has enjoyed most excellent health ever since.

In strange contrast to the above, I will briefly relate the history of the other case.

About a year ago I was asked to see a lady in consultation with her family physician. I was informed that she had a badly lacerated perineum, which had occurred five years previous, during the birth of her first child. That she had not been able to retain the contents of her bowels at will, that she had to wear a sort of anal pad or truss ever since, and that she led a life of a recluse on account of her blemish. On examination I found all the parts, including the perineum, sphincter ani, and an inch of the recto-vaginal septum, had been torn through; and was impressed with the idea that here was a case which was almost a fac-simile of the one last related. There was this difference, however. In the one the parts were repaired at once, while in the latter they were *let alone*. But the strangest part of all—this woman did not suffer from displacement of the uterus of any kind, nor did she complain of any symptom pointing thereto. Nothing, in fact, but the loss of control over her lower bowel. It is only necessary now to briefly state that I did not succeed in obtaining complete union of the severed parts until I had twice failed, the third operation being successful. The success, however, afterwards, I believe, caused her death indirectly. She was so much delighted with her changed condition that, although she had been a prisoner to her house for almost five years previous to this, she could not now be prevailed upon to remain at home but for a short time each day. One day, some three months after the successful operation, she was caught in a snow-storm, got chilled through as she stated, and became very ill. Her family physician pronounced her illness to be typhoid fever. I saw her in consultation the day before she died, and from a vaginal examination satisfied myself that she had incurred an attack of pelvic cellulitis, which ended in general peritonitis and death, just ten days after the first onset.

These two cases have certainly been to me lessons of great value. They are extreme examples of the two conditions—recent and old lacerations. The same hand was engaged in the repair of both, and the circumstances of nursing, etc., were equal, but how different the result!

The following propositions seem worthy of consideration:

1. That the results obtained by immediate repair justify the operation.

2. That the operation, when performed in a way to be indicated, and when proper antiseptic precautions are observed, is free from danger.

3. That the operation does not involve inconvenience or mental shock to the patient.

4. That the performance of the operation should be within the capability of every well-informed physician.

5. That convalescence is always perfect.

6. That an unrepaired perineum is a blemish to a woman, she is virtually a cripple, and is sure ultimately to find her way to the gynecologist for the relief of a series of uterine troubles, often induced by, and depending upon, this blemish.

Treatment.—We will now turn our attention to means most advantageous in repairing the laceration, and I will mention a few points necessary to the successful issue of the operation *in every respect*.

The operation as performed at present is unnecessarily complicated, and involves the expenditure of too much time in its performance.

It must be reduced to such simplicity, with due efficiency, that most physicians in general practice can perform it without the aid of a skilled assistant, and as I will show, without the knowledge of the patient (two very important conditions). That the patient be seen twice a day, at least, for four or five days following delivery, and attended to by the physician *himself*—not by the nurse.

I will not here enter upon the merits of the various methods of repairing a perineum. Every physician has access to text-books for this information. But what I wish particularly to show in this memoir is that ONE SUTURE ONLY is necessary in cases of the most extensive laceration, with the sphincter remaining intact, and that it can be inserted and adjusted with-

out the knowledge of the patient, and that the results to be obtained from the procedure will be the most perfect which could be expected under any circumstances. I say, at present, "with the sphincter intact," because I have not recently had a case to treat in which that muscle was severed, but believe it will only require one extra one to specially control that muscle.

It occurred to me that the great bug-bear in this operation was the number of stitches recommended as necessary, the time and fuss involved in the procedure, and the all-important fact that the patient and her relatives became aware that something very serious had happened, and that there was an operation about to be performed for its relief. In fact, I have been informed more than once by ladies "that poor Mrs. So-and-So had a dreadful time in her confinement, that she was all torn to pieces, and that the doctors had to put ever so many stitches in her; she is sure there must have been some bad management on the part of the physician, *and that she would not like any of her friends to be attended by him.*"

From this it can be seen that many a worthy physician has suffered unjustly in reputation with the critical public with which we have to deal, and that his more diplomatic brother—who made no examination, but took the chances of his patient "catching the cold" before alluded to, and thereby "cheating the devil in the dark," no matter how the wind blew—often not only escaped scot-free, but became tolerated to a degree of affection by a very intelligent class of society. Reflections of this nature convinced me that there *must* be a way by which honesty should triumph over fraud, in so far that the parts when torn could be repaired without the knowledge of the patient, when it was not good that she should know of it, and that the necessary confinement to bed at this time would aid us in this respect. I continue reasoning as follows: The vaginal canal-walls, when relieved of the distending element, collapse, so to speak. The edges of the tear fall together in close contact, and there being so much of tube calibre to spare between its fully distended condition and that of complete involution that there will really be no side-traction brought to bear upon the edges of the wound for a week or ten days after delivery, and by this time fairly strong union will have taken place. I also reasoned that a recently lacerated peri-

neum, accompanied, as it is, in some instances, with laceration of the posterior wall of the vagina to various degrees of extent, is an open wound with a sliding base (recto-vaginal cellular tissue), and consequently has no fixed point from which union can begin, or the edges can be maintained in apposition.

As a corollary to these truths, I became convinced that *one suture* would prove sufficient, for the following reasons: That it would act by giving a fixed point from which union could begin; that it would be sufficient to support the edges of the wound in apposition during the period of *rest* before described; that by the time involution and side-traction had set in, the union would have become sufficiently strong to resist such influence.

That there could occur no pus pockets between sutures, and that free drainage, both above and below the single suture, is necessary, was evident. Having an unusual number of young brides on my list for attendance during the summer, I knew I would have ample opportunity to put my views regarding the single suture to a practical test. I began thus:

The first case was that of a powerful young Englishwoman. Forceps were used, under chloroform. Laceration extended right down to the sphincter. One silk suture was introduced midway between the beginning of the tear and the sphincter muscles. The patient had no knowledge of the suture being there or of my removing it on the eighth day. Result, perfect.

In the next three cases, I used silver wire in the same way as in the first case, but I was not as well pleased with it as with the silk. It required more care in the after-treatment. If anything touched the end of the wire, the sensation it caused the patient was unpleasant, and it required considerable care in taking the suture out.

In the following four cases, I returned to the use of the silk, and also gradually increased the distance between the sphincter and the suture with complete union, until I had, in the last two cases, passed the needle through the tissues on a level with the *beginning* of the tear.

One of these cases was a little noteworthy in this respect—the forceps were not used. I had left, and returned to the patient's house a few minutes late. The fetus was expelled

before my arrival. On examination, I found the perineum lacerated down to the sphincter, and the laceration extending right up to the posterior fornix. The rent seemed to involve only the vaginal walls proper, and with my left fingers in the rectum, I could, with my right fingers in the vagina, move freely in *every* direction the edges of the lacerated vagina on the cellular tissue and the white glistening surface of the rectum (the sliding base alluded to). This laceration of the vagina, I believe, occurs in very nearly every case of extensive perineal lesion. It is not recognized or known to the physician at the time, because of its not having been looked for. I may also state that in introducing *the single suture* in this case, the patient, who was not under an anesthetic, only complained of being "pinched," and not to do it again. Union was most perfect, and the suture removed on the tenth day without the knowledge of either the patient or her family of its ever having been put there.

I will now allude to a few points necessary to be observed in connection with the single suture :

1. Examine carefully, *with your eyes*, every perineum after removal of placenta. If lacerated to more than a quarter of an inch, apply the suture.

2. Use one of Emmet's long, straight perineum needles, with a silk suture. By the aid of a holder, force the needle through the skin on the left side of the tear, half an inch from its edge, at any point between the beginning and end of tear, but the nearer to the beginning, that is, the higher up, the better will be the result. Now, with the two fingers of left hand in the rectum, press up the rectal wall and recto-vaginal cellular tissue, so that the needle can be rapidly, though steadily, made to glide beneath this tissue and over the rectum, hugging the latter as closely as possible, to make its exit at a corresponding point on the opposite, or right side. In tying the suture, avoid doing so too tight, as it is a good plan to allow for swelling, which generally lasts for some days.

3. Be sure that the needle in no part of its course makes an exit in the vaginal surface ; if so, you will probably have a pus pocket.

4. The operation is very simple, and can be performed by any physician of ordinary experience.

5. The after-treatment consists in washing out the vaginal passage night and morning with any antiseptic solution the physician is accustomed to use. *But he must do it himself*; the nurse would be as likely to pass the tube below as above the suture, *and kill all your joy*. As regards antiseptics, I use in such cases a solution of corrosive sublimate $\frac{1}{2000}$ once in twenty-four hours, administered at night. I find this solution as handy and harmless as carbolic acid. Tell your chemist to make a 3 ij. alcoholic solution of hyd. bichl., each drachm of the solution to contain seven and one-half grain of the salt. One teaspoonful of this mixture added to a pint of water will give, almost to a fraction, one part in one thousand. I have used this solution in cases of metria three times in the twelve hours for two consecutive days without any evidence of toxic effects from absorption. It is probably due to the formation of an insoluble albuminate of mercury which seals up all breaks in the surface for a time.

6. The suture had better be allowed to remain in situ for nine or ten days. I am strongly in favor of the silk; the wire suture is liable to produce a bleeding point or two on removing it. This accident might prove troublesome from absorption, which is so active at this period of convalescence.

7. The nurse is the only assistant you will require, and is, of course, in your confidence.

A REVIEW OF THE OPERATION OF GASTROTOMY FOR MYO-FIBROMATA OF THE UTERUS.
WITH COMPLETE STATISTICAL TABLES.

BY
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(Continued from Vol. XVI., p. 1284.)

Remarks upon the English Table.—Bantock remarks upon his second case that it is bad practice to leave the stump in the cavity, if the uterine cavity is opened. In Baker Brown's case, of March 2d, 1861, the original says: "The tumor appeared to be a degeneration of the ovary into fibrous tissue." In his case of Dec. 11th, 1862, the tumor was pronounced by

Statistics of Gastrotomies arranged in Alphabetical

E N G -

Num. cr.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of operation.	Length of incision.	Character of tumor and complications.
1	Aveling	Enlarged uterus.	Tumor of uterus, weight 26 lbs.
2	G. G. Bantock	Com	Tumor	Adhesions to omentum, etc., fib. cyst of ut., w. 12 lbs.
3	"	...	"	"	Small tumor, weight 12 lbs.
4	"	Bir'ham	Not c	Fibroid of uterus...
5	"	Feb. 2, '82	Com	Left ov. sup. vag. por. of u.	Left ov. attached to tumor; fib. cyst.
6	"	Bir'ham	"	Removed uterus.	Excessive metrorrh. Intramural fibroid
7	"	1882 or 1883, Bir-ming-ham.	"	do.	Myoma of u. Small embryo, partly gangrenous.
8	"	June 20, 1881.	"	Sup. vag. ut. and ovaries.	Fibroid, w. 12 lbs. Large extensive adhesions.
9	"	Prior to Mh. 1, '82	"	Ov. & large part of ut.	Fibroid, weight 6 lbs.
10	"	"	"	Uterus and ovaries.	" " 4½ " fixed to omentum
11	"	"	"	Tumor and ovaries.	Fibroid cystic degen. weight 1 lb., adhesions, pediculated.
12	"	Bet. M'y 3 & Dec. 6, 1882.	"	Ut. and ov.	Vas. fib. of u., w. 3 lb
13	"	"	"	" "	Fibroid, weight 3 lb.
14	"	"	"	" "	12 in.	Intramural fibroid, weight 13½ lbs.
15	"	"	"	" "	Vas. fibroid, 8 lbs. 2 oz., adhesions.
16	"	"	"	Supra - vag. uterus.	Fibroid, w. 3 lbs....
17	"	Bet. Mar. 1 & M'y 3, '82	"	Uterus and ovaries.	Fib. 3½ lbs. Intra-m. pedic. adhesions.
18	"	"	"	do.	Intra-m. & sub-muc. fib. with polypi.
19	"	Bet. Dec 6, '82, & M'y 7, '83	"	Uterus.	Fibroid, 4 lbs.....

Order, according to Nationality of the Operator.

LAND.

Antiseptics, Anesthetics.	Operation.	Age, Married or single.	Previous operation.	Condition of patient.	Result and Cause of Death.	Reporter and where reported.		
....	Died 2d day.	Aveling, Ob. So., London, Dec. 5, 1877. Ob. Journ., London, v., '77-'78, page 682.		
....	Ped. lig. ¹ in 4 div. Cintrat's ser. no ^d	Recovery.	Ob. So., London, April 7, 1880. Trans. Ob. So., Lond., xxii., 1881, p. 125. Ob. Jour., London, viii., 1880, p. 291.		
....	Stump dropped.	Death; septicemia ooz'g of st. Fistula.	Bantock, do.		
....	Recovery.	do. do.		
....	do. Lancet, 1883, I., p. 627.		
Spr'y of wat'r	Ap. 6, 81-dbl. oöphor.	Not good.	do. do.		
....	March '82, oöphor by Dr. S'v'ge	Recovery.	Savage, Br. M. J., '83, I., p. 711.		
....	Stump outside. Tr'n'fix'g Koeberle's ser.n.	45, M.	Recovery.	Trans. Ob. Soc., Lond., March 1, 1882. Ob. Soc., London, xxiv., p. 47, 1883.		
....	" "	40	"	do. do. do.		
....	" "	38	Menorrh. pain.	"	do. do. do.		
....	" "	36	"	do. do. do.		
....	" "	32, S.	"	do. do. do.		
....	" "	46	Menorrh. some ye's previous.	"	do. do. do.		
....	" "	30, S.		"	Trans. Ob. Soc., London, Dec. 6, 1882, and vol. xxiv., 1883, p. 302.		
....	" "	48, S.	Sev. pain.	do. do. do.		
....	" "	47, M.	Sev. pain, menorrh.	Died 6th day, acute enteritis.	do. do. do.		
....	" "	38	Suffered fr'm press.	Recovery.	do. do. do.		
....	51	Suffered from hem.	"	Trans. Ob. So., London, xxiv., 1882, p. 91.		
..	Pain	Recovery.	Med. Times and Gaz., March 31, 1883, p. 368.		

Number.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of operation.	Length of incision, inches.	Character of tumor and complications.
20	Bantock....	Bet. Dec 6, '82, & M'y 7, '83	Com	Uterus.....	Fibroid, 7 lbs.....
21	do.	do.	do.	do.	do. 2 lbs.....
22	Dr. Braxton Hicks, Dr. R. Barnes	1876....	do.
23	Fred. Bird..	1846....	do.	Cyst of ov., and small part of ut.	5	Pelv. adh., cys. tum. of ovaries with cholesterine.
24	Baker Br'wn	M'rch 2, 1861	do.	Remv. tum. of ov. and uterus?	...	3; af- trw'd enl'gd	Fibroid, and mult. cyst of right ov., some ascites.
25	do.	May 15, 1860	Not c	Fibro-cyst of ut., extensive adhesions.
26	do.	do.	Fibroid of uterus, gestation.
27	do.	Uterine tumor.....
28	do.	June 21, 1861	Myoma.....
29	do.	Dec. 11, 1862	Not c	Tap'd cyst.	[See remarks here- after.]
30	do.	Nov. 9, 1864	Com	Ut. at cerv. fib. & ov	Umbilical hernia.— Pediculated.
31	Th's Bryant	May 19, 1871	do.	Uterus and ovaries.	Long.	Cystic myo-fibromas w. 8½ lbs. ov. poly- cystic.
32	Cadge.....	do.	Uterus and ovaries.	Interst'al ut. fibroid.
33	Carter.....	do.	Tumor.....	Fibro-cyst of fundus pediculated.
34	Thos. Cham- bers	F'b 6, '78 Chel's a Hospital fr.w'm'n	do.	Sup.vag. ut. and ov.	Over 12	Myo-fibroma, pedic- ulated.
35	do.	June 2, 1880	do.	Uterus and both ov.	12	Encapsuled uterine fibroid.
36	Chas. Clay.	Manch'r Nov. 17, 1848	do.	Tum. and ut. to cervix.	13	Hard lobular mass, weight 13 lbs.
37	See remarks in this case	Manch'r Jan. 6, 1844	do.	Uterus and ovaries.	Adhesions... ..

Antiseptics, Anesthetics.	Operation.	Age, Married or Single.	Previous operation	Condition of patient.	Result and cause of Death.	Reporter and where reported.
.....	Pain.....	Recovery.	Med. Times and Gaz., March 31, 1883, p. 368.
.....	do.	do.	do. do. do.
.....	Agony fr. pressure	Died 36 h after.	Farncourt Barnes, Ob. So., L., Dec. 5, 1877. Ob. J., Lon., v., 1877-78, p. 680. Trans. O. S., Lon., xix., '78, p. 278.
.....	52	Ut' us prob. normal	Recovery.	Case not published. Lee, "Tumors of Uterus," '47, p. 269.
Chloroform	Tap. cyst cl'mp'd & excis. tu	48, S.	Fair h'lth.	Recovery.	Gibb, Trans. Path. So., Lon., '61, p. 154. Cited incorrectly as Uterine, by Pozzi.
.....	45, S.	Death 16th day. ¹	Boinet, Com. by Brown & Reuth, Gaz. Heb. de Méd. et de Chir., x., 1873, p. 462. C. C. Lee, N. Y. Med. Times, xiv., 1871, p. 469.
.....	Pyemia...	Death 19th day.	do. do. do.
.....	Recovery.	do. do. do.
Chlo. later Ether	Lig. and clamp.	34	Good.	Death 18 d. after. Periton.	Caternault, p. 26. Braithewait's Retrospect, xlv., Jan., 1863; from Lon. Med. Review, Jan., 1862, p. 320.
.....	36, M.	Tap'd no ch Mr. '69	Death 26 d. after.	T. W. Nunn, Trans. Path. So., London, xiv., 1863, p. 198.
Chlo.	Lig. and ecraseur	49, S.	D. next d. from hem.	Trans. Ob. Soc., London, vi., 1865, p. 249.
.....	Lig. to c. in halves Clamp.	26, S.	Good.....	Recovery.	Bryant, Trans. Ob. Soc., Lon., xiv., 1873, p. 79.
.....	D. fr. sh'k 36 h. after	Communicated to Routh. See Caternault. See Boinet, 463.
.....	Recovery.	Carter, Obs. Soc., Lon., May 2, '83; Med. Times and Gaz., 1883, i., p. 595.
.....	Lig. trfix ut. with whip'rd & encir'd whole w. dbl. whip cord.	Bad.....	Death 26 h after. Sm'l piece of bladder in ligature.	Chambers, Ob. S., Lon., Feb. 6, and March 6, '78; Ob. Tr. v., '77-8, p. 814; vi., '78-9, p. 27; xx., '79, p. 32-55.
Anti-sepsis	Chambrs cl'p, cer. tr'nsfix'd and lig.	Chambers, Ob. Soc. of Lon., June 2 and 7, '80, Obs. Jour., viii., '80, p. 418, 474; Tr. Ob. Soc., xxii., '81, p. 159-187.
.....	Lig. ar'd Fal. tube and cerv.	45, M.	Death 1½ h after op	Orig. art by Dr. Clay, in Brit. Rec. Obs. Med. and Surg., i., 1848, p. 281.
.....	49 h a d 10 ch	Death 3 B weeks aft.	Phillips, Med. Chir. Trans., xxvii., 1844, p. 473, 479; S. S. Lee, "Tum. of Ut.," Dub. Jr. M. Sc., xxv., '44, p. 387.

¹ Lee says 24th day.

Number.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of operation.	Length of incision, inches.	Character of tumor and complications.
38	Chas. Clay..	Manch'r Com Jan. 16, 1844		Uterus and both ov.	12	Uterus enlarged to nearly 20 lbs., and l.ov. 4 lbs.; ascitic deposit 8 lbs.; kind of tum. not stated
39	do.	Manch'r do.		Uterus	Long.
40	do.	Manch'r do. Jan. 2, 1863		Ut. to cerv. and ov.	15 m n.	11	Fibroid, w. 11 lbs. no adh. interstitial.
41	F. D. Fletcher	Liv'pool May 14, 1862	do.	9	Adhesions, fibro-cyst Lig. left in cavity
42	A. B. Granville	M'ch 21, 1827 ¹	do.	Tumor	9	Wt. 8 lbs. Lee says fib. tum. [see r'm'ks]
43	Jas. Hakes..	Liv'pool Jan. 29, 1863	do.	Ablation	Long.	Fibro-cystic adhes'ns narrow pedicle.
44	A. M. Heath	Manch'r Nov. 21, 1843	do.	Ut. at cerv.	Long.	Fibrous, weight 6 lbs Submucous.
45	Not given..	Somey'r ago	do.	Tumor	Fibroid, long pedicle ascites.
46	Thos. Keith	Feb. 15, 1875	do.	Ut., sup. v., ov., o'dets	1 h.....	10	Fibro-cyst, 8½ lbs...
47	do.	R'yl Inf. Dec. 12, 1874	do.	Ut., sup. v., left ov.	2 h.....	Long.	Fibro-cyst, adh. ...
48	do.	Nov. 2, 1874	do.	Ut., sup. v., left ov.	Long.	Fibro-cyst, w. 11 lbs.
49	do.	Not c	Emptied 2 lrg. cysts	Tumor fused with uterus and bladder
50	Lane.	London Feb. 15, 1844	Com	Tumor	7	Cystic tum pedicl'd, arising fr. fundus
51	Lane.	London 1847	Not c	Um to pub	Large tumor connected with uterus.

¹ This date is sometimes incorrectly given as 1829.

Antiseptics, Anesthetics.	Operation.	Age, Mar. tel or Single.	Previous operation	Condition of patient.	Result and cause of Death.	Reporter and where reported.
.....	52	D. 15th d. by nurse on fl'r w'le changing bed.	Clay, Brit. Rec. Ob. Med. and Surg., i., '48, p. 391. Trans. Ob. Soc., London, v., '63, p. 66; and also in Clay "Results of Opera'ns," '48, p. 53.
.....	Death....	Com. to the reporter by Dr. C. Clay, "Dis. of Ov.," Lon., 1860, p. 144.
.....	In. hemp lig. Lig. in wound	S.	Recovered	Clay, Trans. Ob. So., Lon., v., '64, p. 67; Am. J. Med. Sci., li., 1866, p. 138.
Chlo.	Cut thro' w'th ecra lig. with iron wire returned to abdom	40 Wid	Healthy..	Recovered	Fletcher. Trans. Liv. Med. So., Oct. 16, '62; Brit. Med. J., 1862, ii., p. 499.
.....	30	Death....	R. Lee, Med. Chir. Tr., xxxiv. '51, p. 15; Med. Gaz., Lon., xxxi., 1843, p. 539.
.....	Silv. wire lig. left in cav.	42, M.	Tap'd Feb'y 1882	Died 33 h. after op.	Hakes, Trans. Liv. Med. Ins., Feb. 5, '63; Br. Med. J., '63, i., p. 225-253. Boinet quotes this name as <i>Stakes</i> .
.....	46 ¹	Anem. fr. loss of blood.	Died 17 h. after op.	Heath, orig. rep., Lon. M. Gaz. '43-4, p. 309; R. Lee, Med. Chir. Trans., xxxiv., '51; T. S. Lee, "Tum. of Ut.," '47, p. 25.
.....	Recovered	Graily Hewitt, Ob. So., Lon., April 7, '80; Obs. J., Lon., viii., 1880, p. 295.
.....	Koerberl's serre noe. steel wire push thr' ut. above vagina.	do.	Keith, Edinb. Med. J., Ob. J., Lon., iv., '76-77, p. 58; Lancet, May 15, 1875.
.....	Tap. cyst Tum. bi-sec'd st'p tr'nsfix'd Koerberlé & fast'nd in incisi'n	44, S.	Sep. 5, 1874, tap'd again 15th.	Feeble....	do.	Keith, Lancet, May 15, 1875. Pozzi, p. 45.
Ether	do.	22, S.	Anemic..	do.	Keith, do. do.
.....	do.	Keith, Lancet, '70, ii., p. 250.
.....	Lig. in ab cav., no drain'ge	43, M.	Tap'd th ree times	Recov. D. 5½ y. af. of dis. of bla.	Com. by Lane to Clay; see "Dis. of Ov.," Lon., '60, p. 166.
.....	22, S.	Recov. D. sud. 5 w. after. See remarks.	R. Lee, Med. Chir. Tr., xxxiv., '51, p. 19; "Dis. of Ov.," p. 166.

¹ The two Lees give her age as 40.

Number.	Name of operator.	Date and place of operation	Complete or not.	What was removed.	Duration of operation.	Length of incision, inches.	Character of tumor and complications.
52	John Lizars.	Ed'burgh, April 24, 1825	Not c	Sternum to umbilicus	Pedicated fibroid attached to fundus
53	William McCormac	Jan. 3, 1882	Com	Uterus	Cancerous ut. tum..
54	Meadows...	A few yrs prior to Dec., '65	2 tumors; one uterine, one omental.
55	D. L. Roberts.	Manchester May 9, 1871	Com	Tum., r. ov. & Fal. t.	3; enlarged	Fibro-cyst, w. 23 lbs. parietal adhesion, and adh. to r. ov. Pediculated 3 in. broad, 1 inch long.
56	C. H. F. Routh	Samar. Hos. N'v	Not c	Umbilicus to Pub	Fibro-cyst of uterus.
57	do.	Cav'dish Home, June 16, 1875	Com	Tumor	12	Pedicated fibroid, Weight 17½ lbs.
58	Thomas Savage	Bir'ham	Uterine fibroid.....
59	do.	do.	Uterine fibroid.....
60	do.	Bir'ham April 22, 1879	Com	Fund. uteri and l. ov.	Multiple fibroids....
61	do.	Bir'ham Nov. 7, 1878	do.	Fund. uteri and tumor	Fibroid of uterus...
62	do.	Bir'ham May 30, 1878	do.	Uterus and ovaries.	Fibroid of uterus...
63	do.	Bir'ham July 5, 1879	do.	Supra - vag. uterus.	Soft Myoma.....
64	do.	Bir'ham Aug. 22, 1879	do.	Fund. uteri & tumors	Myoma l. side fund., senile parov. cyst adherent to uterus
65	do.	do.	do.	Fund. uteri.	Uterine Fibroid.....
66	do.	Bir'ham M'ch, '82	Not c	Ut. appendages.	Myoma of uterus...
67	do.	Bir'ham 1882	Com	Myoma and adh. intes	Myoma of uterus...
68	do.	Bir'ham July 13, 1882	do.	Ut. to cerv. Fetus.	1½ h....	9	Fibro-myoma, 9 lbs. Pregnancy. Interstitial.
69	do.	Bir'ham 1882	Not c	Myoma.....
70	It seems as if Eddison was the op'r here. John Sloane.	Nottingham, Oct., 1854	Com	Tumor	Umbilicus to pub	Myo-fibroma, weight 50 oz. Caternault says 50 pounds!!!

Anti-sep- sis, An- esthetics.	Operation.	Age, Married or Single.	Previous operation	Condition of patient.	Result and cause of Death.	Reporter and where reported.
.....	34, S.	Recovered	Lizars, Obs. in Dis. Ov., Edin., '25, p. 19; Monthly J. Med. Sc., xii., '51, p. 198, 229; Med. Chir. Tr., xxxiv., 1851; Clay, "Dis. of Ov.," p. 198.
.....	Dr'n.tub. thro'wnd and vag.	Recovered	Lancet, Jan. 14, '82; L'Abeille Med., Ap. 23, '83, p. 160.
.....	Parry, "Extra-Uter. Pregn." Phil., 1876, p. 173.
.....	Pedicle outside circul'r clamp	32, S.	Recovered	Roberts, Trans. Obs. S., Lon., xiii., 1872, p. 310.
Chlo.	Tap. cyst	26, M.	D. 2 d. aft Exhaust'n	Routh, Trans. Obs. Soc., Lon., viii., 1867 p. 122.
Chlo.	Clamp, actual caut'ry	32, S.	Recov. aft protracted illness.	Routh, Ob. Soc., Lon., July 7, '75, Jan. 5, '76; Tr. Obs. So., Lon., xvii., '76, p. 216; xviii., '77, p. 5; also Obs. J., Lon., iii., '75-76, p. 823.
.....	48	Death....	Birm. Med. Review, '74; Pozzi, 45.
.....	29	Recovery.	do. do do.
Carb. spray	Dbl.l'g.to cerv. stp. in abd.	53, S.	do.	Savage, "Treatment of Uter. Myoma by Abd. Sec.," Obs. J., Lon., vii., '79-80, p. 606.
do.	Cl'p just above cer	37, S.	Cerv. dil. & incis.	do.	do. do. do.
do.	Do'ble lig caut. cl'p	38, S.	do.	do. do. do.
do.	Stump dropped in.	39, S.	Death10th day.	do. do. p. 607.
do.	Stump in wound Clamp	36, M.	Recovery.	do. do. do.
do.	do.	41, M.	do.	do. do. do.
.....	Savage, Brit. Med. J., '83, i., p. 711.
.....	Death....	do. do. p. 712
Ether	Cl. & lig. Lig. atc'd to ab. w	25, M.	Recovery.	do. '82, ii., p. 423.
.....	do. '83, i., p. 711.
Chlor.	Ligature of whip- cord.	33, S.	Death....	Sloane in Br. Med. J., 1858, p. 159.

Number.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of operation.	Length of incision, inches.	Character of tumor and complications.
71	Heywood Smith	Com	Sup. v. ut., ovar., and oviducts.	1h. 40m	Cyl. epithelioma....
72	Thornley Stoker.	...	do.	Sup. v. ut., and r. ov.	Long & difficult	Enfm to pub	Myoma, 14 lbs., ascites, extensive adh.
73	do.	Dublin? June 6, 1878	do.	Sup. v. ut....	Soft myoma, weight 12½ lbs.
74	Lawson Tait	Jan. 14, 1873	do.	do.	Fibro-myoma, w. 11 lbs.
75	do.	Sept. 11, 1875	do.	do.	8	Soft myoma.
76	do.	May 19, 1876	do.	6	Mul. nod. myoma, 9 lbs.
77	do.	Aug. 4, 1877	do.	Ut., ov. and Fal. tubes	14	Edematous myoma, 20½ lbs. thick ped.
78	J. K. Thornton	London May 22, 1879	do.	Tumor	70m....	Fibroid 7 lbs. pedicle broad.
79	do.	do.	Uterus and ovary.
80	do.	Dec. 4, 1879	do.	Tumor and right ov.	Long & tedious	5½	Fibroid, 4 lbs.
81	do.	do.	Fibroid....
82	do.	Not c	Fibro-cyst.....
83	do.	do.	Large uterine fibroid
84	do.	Com	Tumor	Fibro-cyst ut.....
85	do.	do.	do.	do.
86	do.	do.	do.	do.
87	do.	do.	Uterus and both ov.
88	do.	Nov. 27, 1879	do.	Sup. v. ut., & both ov	2¾h....	4	Fibroid 5 lbs. pedicle soft and vasc.
89	do.	Jan. 10, 1877	do.	Uterus and both ov.	1½h....	Fibroid.....
90	do.	do.	Ut. and appendages.	Soft fibromyoma 11½ lbs.
91	Wallace....	Liv'pool	do.	Sup. vaginal uterus.	Large uterine fibroid

Antiseptics. Anesthetics.	Operation.	Age. Married or Single.	Previous operation.	Condition of patient.	Result and cause of Death.	Reporter and where reported.
.....	Ped. lig. s t u m p drop'd in	Death 4th day.	Smith, O. S., Lon., Dec. 3, '79, Jan. 14, '80; Ob. J. Lon., vii., '79-80, p. 607; viii., '80, p. 91; Trans. Ob. S., Lon., xxi., '80, p. 313; xxii., p. 3.
Th'ml spray	Glass d.t. t'fixed & lig. cerv.	Recovery.	Stoker, Surg. So., Ireland; M. Press and Circ., Jan. 28, '80; O. J., Lon., viii., '80, p. 189.
.....	do.	Kidd, Ob. So., Dub., June 8, '78; Brit. Med. J., '78, ii., p. 342; Lon. Ob. J., vi., '78-9, p. 720.
.....	Clamp...	34, S.	do.	Trans. Obs. S., Lon., meeting Dec. 5, '77, xix.: '78, 274; L. Ob. J., v., 1877-8, p. 679.
.....	St. clmp. extern'ly	45, M.	Death 5th day.	Obs. S., Lon., Dec. 5, '77; Ob. J., Lon., v., '77-78, p. 679; Tr. Ob. S., Lon., xix., '78, p. 275.
.....	Clamp to pedicle	41, S.	Death 3d day.	do. do. do.
.....	Clamp & Mar. cautery	Recovery.	do. do. do.
Bich. mthyl Lister	Enucl'on clamp to ped. Ped tied & st retur'd	39, M.	Pregnant 7 months Feeble.	Death 5th day.	Obs. S., Lon., June 4, '79; Tr. Obs. Soc., Lon., xxi., '80, p. 163; Obs. J., Lon., '79-80, p. 234.
.....	Death....	Ob. S., Lon., Ap. 7, '80; Tr. O. Soc., Lon., xxii., '81, p. 129; Ob. J., Lon., viii., p. 296.
Bich. mthyl	Sec. and enuc. stp transfix. lig. & cut off short.	34, S.	Recovery	do. do. do.
.....	Death, s'p-ticemia.	do. do. do.
.....	Hopeless.	Death....	do. do. do.
.....	Recovered	do. do. do.
.....	do.	do. do. do.
.....	do.	do. do. do.
.....	do.	do. do. do.
.....	Death, s'p-ticemia.	do. do. do.
Ansep bch.m	Stump in wound.	Recovery.	do. do. do.
.....	Dble. lig. c. & br. l. Lig. cut short.	38, M.	do.	Ob. J., Lon., v., '77-78; Tr. O. So., Lon., xxii., '81, p. 129.
.....	do.	Obs. S., Lon., Ap. 4, '83; Lancet, '83, i., p. 687.
Full a'sep'st	Clamped st'p with Koeberl's serre n.	50	do.	Henry Briggs, Brit. Med. Jr., '83, i., p. 958.

Number.	Name of operator.	Date and place of operation	Completed or not.	What was removed.	Duration of operation.	Length of incision, inches.	Character of tumor and complications.
92	J'y Walne.	Oct. 19, 1848	Not c	Left ovary..	. . .	15	2 interst. fib. of ut. cysts of ov. womb enlg'd to full term.
93	T. S. Wells.	Oct. 14, 1861	Com	Sup. v., ut., ovaries and Fal. tubes.	10	Uterine fibroid, 27 lbs. Interstitial.
94	do.	do.	Uterus and ovaries.	Ut. fib., w. 15 kilog.
95	do.	Jan. 12, 1863	do.	Tumor by enucleation	6	Fibrous, wt. 17 lbs. Interstitial
96	do.	do.	Tumor	Interstitial
97	do.	April 7, 1863	Not c	Fibroid, 25 lbs.....
98	do.	April 30, 1863	Com	Tum., r. ov. and 2 sm'l ut. fibr'ds	9	Fib. 16 lbs. 5 oz. with cyst con. 26 pt. fluid & 4 lbs. decomposed matter. Pediculated
99	do.	July 28, 1863	do.	Ov. and ut. tumors.	4	Ov. cyst., ut. fibroid
100	do.	June 20, 1864	Not c	Left tumor tapped.	6	2 fib. cysts at fundus adhesions, ascites
101	do.	April, 1866	do.	Part of tum. of doubtful origin	Uterus and ovaries diseased.
102	do.	August, 1866	do.	19 pints ascitic fluid
103	do.	Decem., 1866	do.	Ascitic fluid	Solid ut. tumor.....
104	do.	August, 1867	do.	Punc. tum..	Ut. fibroid..
105	do.	Novem. 1867	do.	Fluid.....	Fibro-cyst, 7½ lbs. solid and 16½ lbs fl.
106	do.	February, 1868	do.	do.	Ut. fibro-cyst.....
107	do.	April, 1868	Com	Tumor	5	Fibroid.....
108	do.	August, 1868	Not c	Fluid...	Apparently canc. tumor of ut. and ov.
109	do.	January 1869	do.	4	Large uterine fibroid
110	do.	April 7, 1869	Com	Peri-ut. tum Fal. tubes and ovar's	Fibroid of fundus 34 lbs. 10 oz., short pedicle.
111	do.	May, 1869	do.	Uterus	5	Ut. tum. pedicle....
112	do.	Novem. 1869	Not c	Fluid.....	Ut. fibroid.....
113	do.	February, 1870	do.	do.	Papilloma both ov. and uterus.
114	do.	June, 1870	Com	Tumor	9	Solid ut. myoma, w. 22 lbs.

An t isep- sis, Ab- stentures.	Operation.	Age Married or Single.	Previous operation	Condition of patient.	Result and cause of Death.	Reporter and where reported.
.....	45, S.	Feeble...	Death 9 d. after.	Walne, Lon. M. Gaz., xxxiii., 1843-44, p. 723; also in T. S. Lee's "Tumors of Uterus."
Chlor.	Lig. of p. bro't out of wound	Death, shock, 4th day.	Dis. of Ov., i., '65, p. 350.
.....	do.	Gaz. Heb. Med. and Chir., x., '73, p. 463. See Remarks.
Chlor.	Lig.	35, S.	Death....	Dis. of Ov., p. 363; Trs. Path. Soc., London (?)
.....	Death...	See Remarks.
Chlor.	Rec. fr.op. d. Aug. 26 1864.	Dis. of Ov., p. 353.
do.	Stump in wound	53, S.	2 oz. bl. in per. cav. & 3-4 of ser. in pel. D. 3 hours af.	Dis. of Ov., p. 354; Wells says death was by shock and chlo- roform. Autop. as stated.
do.	Cl. to ov. ped. Ecr. to ut. ped	55, S.	Death 44 h. after.	Dis. of Ov., p. 186.
do.	Lig.	45, S.	Never ral- lied.	Dis. of Ov., p. 356.
.....	38, M.	Peritonit's death 32 h. after.	Dis. of Ov., N. Y., '73, p. 466; Path. Trans., 17, p. 203.
.....	38, M.	Relv. D. a few w. af.	Dis. of Ov., N. Y., '73, p. 464.
.....	39, M.	Rel.d.s me mos. aft.	do. do. do.
.....	Drainage	48, M.	Recovery.	do. do. do. Ov. and Ut. Tum., '82, p. 516.
.....	50, S.	Death 3d day.	Dis. of Ov., N. Y., '73, p. 464.
.....	58, W.	Recovery.	da. do. do. Ov. and Ut. Tum., '82, p. 516.
.....	Ligature returned	40, M.	Death 44 h after.	Ov. and Ut. Tum., '82, p. 512.
.....	51, W.	Died 10th day aft.	Dis. of Ov., '73, p. 465.
.....	42, M.	Recovered	Ov. and Ut. Tum., '82, p. 516.
.....	C'mp for- ceps.	36, S.	Per. d. 3d day after.	Tr. Ob. So., Lon., xi., '70, pp. 73, 97; Dis. of Ov., '73, p. 191; Ob. S., Lon., Ap. 7, '69, and May 5.
.....	Ligature.	37, M.	Recov. D. 6 mos. af. can. of cer	Ov. and Ut. Tum., '82, p. 512.
.....	25, M.	Recovery.	do. do. p. 516.
.....	30, M.	Died in 40 hours.	Dis. of Ov., 1873, p. 465.
.....	Extrap'r- itoneal	36, S.	Died 14th day.	Ov. and Ut. Tum., p. 512; Br. Med. J., '78, ii., p. 130.

Number	Name of operator	Date and place of operation	Completed or not	What was removed.	Duration of operation.	Length of incision, inches.	Character of tumor and complications.
115	T. S. Wells.	June, 1870	Not c	Ut. fibroid.....
116	do.	Decem., 1870	do.	5	Ut. fibroid.....
117	do.	June, 1871	Com	Tum. and 59 pts. fluid	Ut. tumor, 11 lbs. 11 oz. solid.
118	do.	August, 1871	Not c	Fluid.....	5	Papilliform masses of ut. ov. blad. & rect
119	do.	January, 1872	Com	Tumor and left ov.	...	8	Fibroid, 20 lbs.....
120	do.	March, 1872	do.	Ut. and both ovaries.	10	Tumor of ut. and ov. 26 lbs.
121	do.	May, 1872	do.	Tumor and right ov.	Ut. fibroid.....
122	do.	June, 1872	Not c	Fluid.....	4	Ut. fib. & cyst. Was it of ov. or ut.?
123	do.	April, 1873	do.	do.	4	Uterine tumor... ..
124	do.	July, 1873	do.	4	Uterine tumor.....
125	do.	April, 1874	Com	Tumor and right ov.	8	Fibro-myoma, 11½ lb
126	do.	Decem., 1874	do.	Tumor.....	8	Fibro-myoma, 9 lbs.
127	do.	May, 1875	do.	Tumor and right ov.	6	Fibroma molluscum cysticum, 29 lbs.
128	do.	do.	do.	do.	6	Fibro-cyst of uterus, 19 lbs.
129	do.	do.	Not c	5	Uterine tumor... ..
130	do.	April, 1876	Com	Tumor.....	7	Subperit. fundus....
131	do.	June, 1876	Not c	Tap'd Cyst..	5	Ut. fibro-cyst... ..
132	do.	June, 1873	do.	5	Tumor of uterus....
133	do.	August, 1876	Com	Tumor and both ov.	9	Ut. fibroid.....
134	do.	Septem., 1876	Not c	Fluid...	5	Uterine fibroid.....
135	do.	October, 1876	Com	Tumor.....	...	4	Subp. fibroid.....
136	do.	October, 1876	Not c	Tap'd cyst..	Ut. fibro-cyst.....
137	do.	Novem., 1876	Com	Tumor.....	6	Ut. fibro-cyst.....
138	do.	Novem., 1876	Not c	5	Uterine tumor.....
139	do.	March, 1877	Com	Tumor and left ov.	6	Fibro-cyst, 20 lbs...
140	do.	April, 1877	do.	Tumor.....	6	2 ut. fibroids.....
141	do.	July, 1877	do.	Tumor and both ov.	8	Ut. fib., 12 lbs.....
142	do.	do.	do.	Tumor.....	6	Solid fibroid uterus, 5 lbs.

Antiseptics, Anesthetics.	Operation.	Age, Married or Single.	Previous operation	Condition of patient.	Result and cause of Death.	Reporter and where reported,
.....	43, M.	Died 2 y. af. recov.	Ov. and Ut. Tum., p. 516. Is this the same case as the one on p. 465 of Dis. of the Ov.?
.....	35 S.	Recovery.	Ov. and Ut. Tum., p. 516.
.....	46, M.	Recovery.	do. do. p. 512; Br. Med. J., 1878, ii., p. 130.
.....	63, M.	Death 15 days after.	Ov. and Ut. Tum., p. 510.
.....	Ligature returned	38, S.	Death 3d day.	do. do. p. 512.
.....	44, S.	Death in 2 hours.	do. do. do.
.....	36, M.	Died. Sup. perit.	Wells, Ov. and Ut. Tum., '82, p. 512.
.....	Recovered	Wells, Dis. of Ov., N. Y., '73, p. 465.
.....	36, M.	do.	Wells, Ov. & Ut. Tum., p. 516.
.....	30, S.	do.	do. do. do.
.....	Clamp Drainage	33, M.	do.	do. do. p. 512.
.....	Lig. ret'd drainage	32, S.	Died in 40 hours.	do. do. do.
.....	Ligature drainage	40, W.	Recovery.	do. do. do.
.....	Clamp Drainage	40, S.	do.	do. do. do.
.....	31, S.	do.	do. do. p. 516.
.....	Clamp Drainage	37, S.	do.	do. do. p. 512.
.....	33, M.	do.	do. do. p. 516.
.....	35, M.	do.	do. do. do.
.....	Clamp Drainage	49, S.	Pneum. D 5th day.	do. do. p. 512.
.....	34, W.	Recovery.	do. do. p. 516.
.....	Ligature returned	36, S.	do.	do. do. p. 512.
.....	46, M.	do.	do. do. p. 516.
.....	Ligature returned	40, S.	Peritonit's died 4th d	do. do. p. 512.
.....	38, M.	Recovery.	do. do. p. 516.
.....	Clamp...	49, S.	Died in 20 hours.	do. do. p. 512.
.....	Ligature returned	52, S.	Recovery.	do. do. do.
.....	Extraper need. &c	50, S.	Died 3d d.	do. do. do.
.....	Ligature returned	56, S.	Sept. per. died 6th d.	do. do. do.

Number.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of operation.	Length of incision, inches.	Character of tumor and complications.
143	T. S. Wells.	Feb'y, 1878.	Not c	Nodule.....	5	Uterine tumor.....
144	do.	March 7, 1878.	Com	Tum., r. ov., and some omentum.	1 h.....	16	Tumor, 68 lbs. 6 oz. senile.
145	do.	May, 1878.	Not c	Ut. tumor.....
146	do.	June, 1878.	Com	Tumor and Fal. tubes	8 or 9	Solid fibroid, 12 lbs.
147	do.	Aug. 12, 1878.	do.	Tum. of ut.	5	Fibro-cyst.....
148	do.	October, 1878.	Not c	5	Tumor of uterus....
149	do.	Novem., 1878.	do.	Tap'd cyst..	5	Cystic ut. tumor....
150	do.	Decem., 1878.	Com	Sup. v. ut.	6	Uterine tumor.....
151	do.	Feb'y, 1879.	do.	Tumor.....	7	Solid fibroid fundus
152	do.	May, 1879.	Not c	5	Uterine tumor.....
153	do.	August, 1879.	Com	Tumor, right ovary.	7	Solid uterine fibroid.
154	do.	October, 1879.	do.	Tumor.....	6	Fibroid outgrowth of uterus.
155	do.	do.	do.	do.	6	Fib. cyst. 26 lbs....
156	do.	do.	Not c	5	Solid myo-fibroma..
157	do.	Dec. 10, 1879.	Com	Tumor....	...	4	Fibroid pediculated.
158	do.	January, 1880.	do.	do.	2 fib. cf fundus....
159	do.	Feb'y, 1880.	Not c	Drain'd bl'd cysts.	4	Uterine tum., blood cysts.
160	do.	July (or June) 21, 1880.	Com	Sup. v. ut..	6	Uterine tumor.....
161	do.	Sept. 27, 1880.	do.	Tumor....	8	Ut. fibro-myoma....
162	do.	October, 1880.	Not c	Uterine fibro-cyst...
163	do.	do.	do.	4	Tumor ut.....
164	do.	do.	do.	4	do.
165	do.	do.	do.	4	do.
166	do.	Oct. 7, 1880.	Com	Tumor and fundus.	8	Fibro-myoma, 25 lb.
167	do.	Feb'y, 1881.	Not c	Tumor.....	5	Uterine fibroma....
168	do.	Feb. 12, 1881.	Com	Tum., fund. left ovary and Fallopian tubes	8	Fibroma, 11½ lbs. Ovary cystic.

Antiseptics, Anesthetics.	Operation.	Age, Married or Single.	Previous operation	Condition of patient.	Result and cause of Death.	Reporter and where reported.
.....	41, M.	Recovery.	Wells, Ov. and Ut. Tum., p. 516.
Th'ml spray	Lig.	36, S.	Recovery.	do. do. pp. 500-512.
Mthyl	42, S.	Recovery.	do. do. p. 516.
.....	36, S.	Peritonit's died 4th d	do. do. p. 514.
Mthyl Spray	Su. to rec cl. to ped	39, M.	Ju'y 25 1878, Tap 'd cyst.	Recovery.	do. do. p. 497.
.....	42, M.	Recovery.	do. do. p. 516.
.....	45, M.	Died in '81 recovery	do. do. do.
.....	Lig.	38, S.	Died same day.	do. do. p. 514.
.....	Trfx. and tied.	M.	Died 3d d.	do. do. do.
.....	41, M.	Recovery.	do. do. p. 516.
.....	S.	do.	do. do. p. 514.
.....	Forceps and lig	52, M.	do.	do. do. do.
.....	Lig.	40, M.	do.	do. do. do.
.....	40, S.	do.	do. do. p. 516.
Spray	C l a m p	30, S.	do.	do. do. p. 514.
.....	Enucl'd .	M.	Died 3d d.	do. do. do.
.....	42, M.	Recovery.	do. do. p. 516.
.....	Ligature.	62, M.	do.	do. do. p. 504, 514.
.....	Ligature.	41, S.	do.	do. do. p. 505.
.....	60, S.	Peritoni's, died 3d d.	do. do. p. 516.
.....	Wound'd ut. vein	52, S.	Peritoni's, died 7th d.	do. do. do.
.....	40, S.	N'ther bet nor worse.	do. do. do.
.....	50, S.	N'ther bet nor worse.	do. do. do.
.....	M.	Recovery	do. do. p. 506, 514.
.....	40, M.	Died 8th day.	do. do. p. 516.
Phenolized spray	Ligature.	43, M.	Recovery.	do. do. p. 507, 514.

Number.	Name of operator.	Date and place of operation	Complete or not.	What was removed.	Duration of operation	Length of incision, inches.	Character of tumor and complications.
169	T. S. Wells.	March, 1881	Not c	4	Uterine tumor..... Bladder wounded
170	do.	June 27, 1881	Com	Sup. va. ut. and ovary <i>which one?</i>	8	Uterine fibroid.....
171	do.	Novem. 1881	do.	Tumor.....	Myo-fibroma.....
172	John Williams	do.	do.	Fib.-cyst of u., 28 lb. Bladder torn.

T. Holmes and F. W. Nunn to be fibro-cystic, not originating in, but afterwards involving the uterus. Clay's case, of Nov. 17th, 1843, does not state whether the ovaries were removed. His case of Jan. 6th, 1844, is probably the same as that of Jan. 16th, 1844, which is the correct date. Dr. Clay nowhere details any case of Jan. 6th. (See Churchill, *infra*, p. 387, foot-note.) Dr. Clay (Trans. Obst. Soc., London, V., 1864, p. 67, foot-note) says that his first case of extirpation of the uterus and ovaries was Jan. 16th, 1844, and second case Jan. 2d, 1863. The case of Dr. A. B. Granville is sometimes incorrectly given as March 21st, 1829. It should be 1827. Spencer Wells ("Dis. Ovaries," N. Y., 1873, p. 184) says this was a uterine fibroid, and so reports it, 1882, p. 480. A reference to the *Med. Gaz.*, London, XXXI., 1843, pp. 437 and 539, will show that this case was, as Dr. Storer states, "distinctly ovarian." The case of James Hakes, Jan. 29th, 1863, was probably a tumor of the broad ligament. Boinet (*Gaz. Hebd. Med. Chir.*, X., 1873, p. 462) incorrectly gives the operator's name as *Stokes*. In Heath's case, the age is given as forty by the two Lees, and as forty-six by Phillips in the *Med. Chir. Trans.*, XXVII., 1844, pp. 474 and 479. Mr. Clay says of the death of Mr. Lane's case (1847) that self-destruction was suspected, a fetus of three months being found in the uterus. In the original article, Dr. Savage says that he removed the appendages in seven cases for myoma of the uterus; but details are not given. The case credited to John Sloane at the Nottingham Gen. Hosp., Oct., 1854, would seem rightly to belong to Eddison, with Sloane as reporter. The second case, accredited to Spencer Wells, is apparently that of Oct., 1861.

Antiseptics, Anesthetics.	Operation.	Age, Married or Single.	Previous operation.	Condition of patient.	Result and cause of Death.	Reporter and where reported.
.....	36, M.	Recovery.	Wells, Ov. and Ut. Tum.. p. 516.
.....	52, W	do.	do. do. p. 509, 514.
.....	35, M.	Death im. after.	do. do. p. 510, 514.
.....	Died.....	Obs. Soc., Lon., April 7, 1880; Lon. Ob. J., viii., p. 294.

It is the only one of removal of the uterus and both ovaries with death on fourth day, prior to Boinet's publication. The fourth case, of Mr. Wells, answers only to that of Jan., 1863. It answers to no other prior to Boinet's or Caternault's publications. In his case of April 30th, 1863, Mr. Wells attributes death to shock and chloroform, though at the autopsy two ounces of clots were found in the back part of the peritoneal cavity, and three or four ounces of reddish serum in the pelvis. Dr. Tait's cases are not completely tabulated, as I could find no reliable reports. In a personal letter, Dr. Tait says he has done thirty gastrotomies, with ten deaths. At the meeting of the London Obst. Society, March 1st, 1882, Bantock said he had operated twelve times. Eight of these were extraperitoneal, and all recovered; four were intraperitoneal, and all died. At the meeting of the same Society, March 7th, 1883, Dr. Bantock said he had performed twenty-two gastrotomies, with twenty recoveries.

(To be continued.)

A CASE OF RETENTION OF A MUMMIFIED THREE MONTHS' FETUS UNTIL THE SEVENTH MONTH AFTER CONCEPTION.

BY

J. HENRY FRUITNIGHT. A.M. M.D.,

New York City.

IN the latter part of April, 1883, Mrs. S. menstruated for the last time. Soon afterwards she developed the usual sympathetic and reflex symptoms of pregnancy, such as "morning sickness," shooting pains in the breast, languor, etc., and at the same time noticed that her abdomen, as well as her breasts, were increasing in size. As she had already borne three children, she naturally concluded that she had become pregnant for the fourth time. I had attended her during the last two confinements, both of which had been normal in every particular. In the middle of August, she was very much frightened by the occurrence of a convulsion in her youngest child. A few days subsequently, on August 17th, she observed a discharge of blood from the vagina, but unaccompanied by pain. The bleeding, which was not profuse, continued for a few days, and recurred later in the month. On September 5th it again appeared, and she presented herself before me for examination. On physical examination, I found a little oozing from a very slightly patulous os. The uterus was enlarged, but it seemed to me not quite so large as it should have been at the supposed period of pregnancy, now about four and one-half months. My diagnosis was a partial separation of the placenta from the uterine wall, and hemorrhage as a result, both into the placenta and externally. I advised quiet and rest, and prescribed gtt. x. ol. erigeron canad. on sugar every two hours, till hemorrhage ceased. I did not see the patient again until November 3d, when she continued the history as follows: She said "that she almost doubted that she was in the family way, because she was now near her seventh month, and had never felt life, though she thought that at one time she had been aware of the movements of the fetus. Moreover, after the last discharge of blood, she said that she had grown smaller around the abdomen, and that her breasts also had become smaller, and she also stated that at about the time of the first bleeding, the reflex symptoms of pregnancy, as vomiting, etc., had disappeared."

I appointed a day for a physical examination, but on the following day, November 4th, I was hastily summoned to my patient's home. She was suffering from uterine contractions, and was bleeding from the uterus. I made a digital examination, and discovered a globular body, about the size of a hen's egg, protruding from the os uteri, and by my manipulation it was

ruptured, and a fluid escaped. Nothing further could be detected, but when I withdrew my finger, I observed a piece of membrane adhering thereto. She was placed upon ergot, fluid ext. Squibb, 3 i. every two hours.

I again examined her on the following day, and detected what I surmised to be the leg of a fetus of about the third month of development. But as it could not be budged, I decided not to interfere, but continue the ergot as before. On the evening of the same day I again visited my patient, and was then enabled to deliver her of a fetus. My surmise had been correct; it was a foot presentation of a fetus of the development of about three and one-half months. Its form, however, was flattened from compression. It was very friable. The viscera of both the thoracic and abdominal cavities were exposed to view, and very small and shrivelled. Its color was of a dark brown, and it was leathery and wrinkled, hence called a mummified fetus. And as Prof. Lusk says, on p. 288 of his admirable treatise: "the subcutaneous areolar tissue had disappeared, and the skin lay in direct contact with the muscles." No trace of putrefaction was discoverable. The umbilical cord seemed normal, though somewhat long. After the fetus had been removed, I attempted to reach the placenta, but I was unable to do so. I continued the ergot as before during the night, hoping that it might be expelled naturally, as it eventually was at some hour in the night.

The placenta was quite small, measuring $3\frac{1}{2}$ inches longitudinally, and $2\frac{3}{4}$ inches transversely, and having an average thickness of $\frac{7}{8}$ of an inch. It weighed about $2\frac{3}{4}$ ounces. Its tissue was very hard, dry, tough, and yellowish. It showed traces of fatty and fibroid degenerations.

On one edge of the circumference of the placenta were the remains of a large decolorized clot, evidence of an earlier hemorrhage, possibly of the one in August. There were also to be seen recently formed clots.

It seems to me that this is a quite uncommon and interesting case. Most probably the first hemorrhage caused the death of the fetus, and induced the changes in the placenta. The integrity of the membranes preventing the access of air, explains the immunity from putrefaction. This arrest of development and mummification of the fetus, and its subsequent retention in utero for many weeks and months, and at times even beyond full term, happens more frequently in twin or multiple pregnancy. In these last cases, quite often one of the fetuses becomes a victim to this perversion of nutrition; one fetus maturing normally, the other mummifying. But in a single pregnancy, as the case just related, it occurs very infrequently. From the whole clinical history of the case, the

fetus must have been blighted at about the third month, and remained innocuous in utero for four months, before nature took it upon herself to expel the intruder from the uterus. Neither parent exhibits any syphilitic taint, nor have they ever suffered from specific constitutional infection.

259 W. 54TH ST.

IN MEMORIAM.

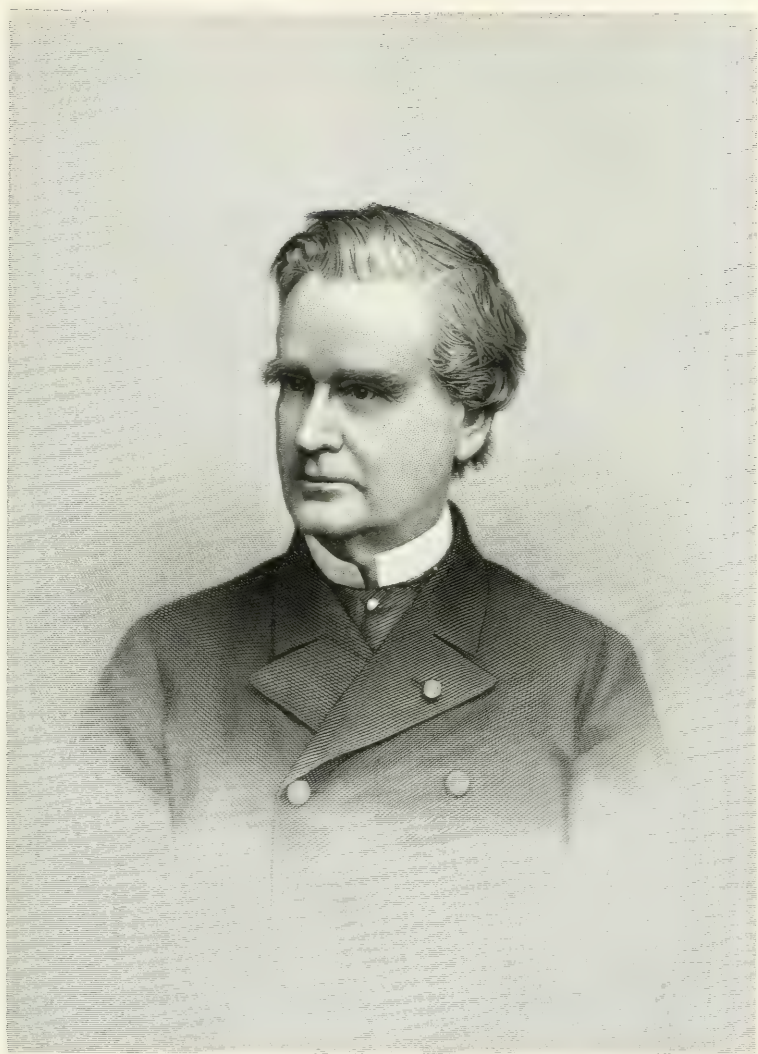
JAMES MARION SIMS.

BORN JANUARY 25TH, 1813; DIED NOVEMBER 13TH, 1883.

No greater shock could have disturbed the medical world than the news of the unexpected death of J. Marion Sims on November 13th, 1883. It seemed as though a cloud had spread over the medical horizon, and as though each one of us had suffered a personal loss. But a few weeks ago we had seen him, the genial host, receiving his guests in honor of Sir William MacCormac; but little more than a week ago the writer had met him in consultation, and on both occasions had taken the opportunity to congratulate him on his ruddy complexion and apparent perfect health, which showed his complete recovery from the severe illness two years ago. No wonder, then, that the news of his sudden death created surprise, consternation, and universal sorrow. It seemed incredible that the name of Sims, which from the beginning of my medical career had been as a watchword to me, and had crossed my lips almost every day, should belong no more to a living man, but have passed to immortality.

To say who Sims was and to recount what Sims has done, seems unnecessary repetition. Who in the medical profession the world over is ignorant of his fame and the foundations on which it rests? A brief review of his life will therefore suffice.

Born in the Lancaster District, South Carolina, on January 25th, 1813, he graduated in 1832 from the College of South Carolina, and after a course of medicine in Charleston, S. C., went to Philadelphia, where he took his degree as doctor in



S. Mason Sims

medicine at the Jefferson Medical College. Settling in Montgomery, Ala., he devoted himself especially to surgery, and soon acquired a large and, for that section, lucrative practice. An accident revealed to him the method of expanding the vagina by the admission of air in the knee-chest position (on the occasion of replacing a uterus retroverted by a fall from horseback), and his peculiar technical genius soon led to the discovery of the perineal retractor with which his name has since been identified, and to which gynecology may fairly be said to owe its renovation and present elevation. By the aid of this speculum he was enabled to thoroughly expose and accurately unite the edges of vesico-vaginal fistulæ, several cases of which had thus far resisted his efforts. By the further substitution of silver-wire sutures for the silk until then used, he succeeded in achieving success in this formidable and up to that period almost incurable lesion. Feeling that he had made a discovery of incalculable benefit in the treatment, not only of fistula, but of all female diseases, and finding the sphere of Montgomery too narrow for his ambition, Dr. Sims, with a well-established reputation in the South for skill and dexterity, removed to a wider field, and in 1853 came to New York. His one great project was to establish a hospital specially devoted to the diseases of women, and for this purpose he delivered addresses, wrote papers, and enlisted the sympathies of such influential and wealthy citizens as he could meet. It was at first a difficult task; specialism was decried by the profession and mistrusted by the laity, and support came slowly. But his enthusiasm, indomitable will, and evident thorough honesty of purpose and conviction finally triumphed, the Woman's Hospital Association was formed by a number of prominent physicians and laymen, with Dr. Sims at its head as Surgeon-in-Chief, and in 1855 the work of the institution was begun in a private dwelling on Madison avenue. The accommodations soon proved insufficient, and steps were taken to secure a more commodious and permanent establishment. The city offered an eligible site, liberal citizens furnished the sum required, and after much hard labor and many delays, at last in 1866 Dr. Sims saw his efforts crowned by the successful opening of the first pavilion of the present New York Woman's Hospital. He was not present on this occasion, for his political

proclivities led him in 1862 to prefer a residence in Europe, whence he did not return until after the close of the Franco-Prussian war in 1871. In his absence, his efficient assistant, Dr. Thomas Addis Emmet, was chosen Surgeon-in-Chief, and under the energetic and able management of this gentleman, the work begun by Dr. Sims was brought to a successful termination. On the return of Dr. Sims in 1871, a new medical board was organized, consisting of Dr. T. A. Emmet (who during these nine years had been the sole surgeon), J. M. Sims, T. G. Thomas, and E. R. Peaslee. Under the medical management of these gentlemen, the Woman's Hospital acquired a world-wide reputation, and the names of its founder and his followers were in the mouth of every physician interested in gynecology. The wards of the hospital were crowded, and soon additional accommodations were called for. Before these were provided, Dr. Sims terminated his connection with the institution, his feeling of humanity not allowing him to coincide in the decision of the Board of Governors that women afflicted with uterine cancer should not be admitted. In this feeling he certainly was right, no matter what may be the opinion as to the wisdom of his course in thinking this sufficient reason for his retirement. His interest in the hospital never abated, and he lived to see a second pavilion of equal size added to the first, and two cottages built for the sole purpose of performing laparotomy operations and allowing subsequent isolation of the patients. The New York Woman's Hospital stands to-day, and always will stand, a glorious monument to the genius, energy, and humanity of J. Marion Sims.

In it are practised and taught the methods which he had devised and introduced, many of them modified, it is true, but still originally *his* ideas; in it, the majority of the instruments used are known as those of Sims; his speculum, without which many operations would be impossible; his depressor, his wire-twister, his shield, his tenacula, his scissors, etc., etc. And from this fountain-head go out all over the land and beyond the sea the precepts which he originated and promulgated, carried away by hundreds of professional visitors to the numerous operations which are there performed, and spread by the house-staff, as year by year they graduate and begin practice for themselves, and by the assistant surgeons, among

which latter the writer was for several years proud to be enrolled.

During his sojourn in Europe, Dr. Sims had abundant opportunity to demonstrate his peculiar methods of operating, and the successes which he obtained both in those operations which first brought his name prominently before the profession—vesicovaginal fistula—and in the minor operations for dysmenorrhea and sterility with which his name was long identified, brought him so prominently forward that honors and riches soon poured in upon him. Numerous decorations were conferred on him, and he had but to show himself in a European city to find himself overwhelmed with professional engagements. This was particularly the case after the appearance of his book on “Uterine Surgery,” in 1866, which was soon translated into German and French, and made an unmistakable sensation. This book carried so plainly on every page the impress of originality; the ideas were so fresh, the suggestions so novel and bold, the style so engaging and eloquent, that every one was carried away with it, and all over the world progressive minds began to put its teachings to the test. It may truly be said that this work of Sims made a revolution in modern gynecological practice. The era of what must properly be called “modern” gynecology, that is, *operative* gynecology, dates, so far, at least, as the world outside of the New York Woman’s Hospital is concerned, from the year 1866. Many of Sims’ views have remained uncontested or unchanged, others have been modified, and others again disproved. But the fact must always be admitted that the *impulse of active surgical interference* given by him to the, before him, largely conservative treatment of the diseases of women, has resulted in the enormous advances which gynecology has made in the last fifteen years. And while this statement applies with more force to this country, where new ideas and bold measures are more readily accepted, it is true also to a large extent of Europe, where (chiefly in England and Germany) many able and dexterous surgeons are now vying with each other in widening the path first traced for them by Sims. In this connection it may not be unfitting to mention one man who aided more than any other in introducing Sims’ ideas into Germany by translating his book into that language, namely, the late Herman Beigel, of London,

afterwards of Vienna, himself subsequently the author of an excellent text-book on gynecology, and a monograph on sterility.

Spending his time chiefly between his favorite Paris and London, and constantly adding to his fame, Sims again came prominently before the world in another capacity in 1870, when, as surgeon-in-chief of the Anglo-American Ambulance, he transferred his skill from the gentler to the stronger sex, and after the battle of Sedan contributed largely to the care of the wounded. It was here that the writer, who, as a boy, had repeatedly met Dr. Sims, and who, while assistant to Scanzoni, had eagerly read Sims' book, again heard his name mentioned by men who had just parted from him; for, on the entry of that portion of the German army with which the writer was serving as surgeon into Sedan, by the merest chance he missed meeting Sims, who only the day before had left on his return to Paris. During his sojourn in Europe, Sims was consulted by many ladies of high degree, and one empress and numerous princesses, I am informed, availed themselves of his skill.

Returned to this country in 1871, for three years Sims carried on a large private practice, to which the severance of his connection with the Woman's Hospital in 1874 allowed him to devote all his time. In 1875, he was elected President of the American Medical Association, and delivered one of his characteristic addresses on the prevention and regulation of syphilis in America, which disease he proposed to limit, and gradually eradicate, by the enforcement of strict sanitary inspection of emigrant vessels and houses of ill-repute.

Dr. Sims was one of the founders of the American Gynecological Society, and in 1880 received the well-merited honor of its presidency. A severe, almost fatal, attack of pleuro-pneumonia in the winter of 1880-81 rendered him unable to attend to his labors for nearly a year; indeed, it was feared at one time that he would never recover sufficiently to resume practice. But his wonderful vitality asserted itself a second time (a first apparently fatal illness having prostrated him thirty years previously), and after a sojourn in the South and a summer and winter in Europe he returned to this country almost entirely restored to health.

During the last two years of his life, he made repeated trips abroad, spending the winter partly in Rome and partly in Paris.

He was about to return to Rome again, having engaged his passage for November 17th, when, apparently in robust health, he was seized with dyspnea at 3 o'clock on the morning of November 13th while in bed working on his autobiography, and when his son, Dr. Harry Sims, reached his bedside, he expired without a word. The autopsy revealed atheromatous degeneration of the coronary arteries and a fatty heart.

To properly estimate the position occupied by James Marion Sims in the department of gynecology (with obstetrics his name can scarcely be said to be identified), we must go back thirty years and compare the science, and, above all, the practice of that specialty, as it was then, with the same practice as it now exists. Vaginal injections, cauterizations of the cervix, and applications to the vagina through the imperfect plurivalve and tubular specula then alone used, constituted pretty much all the active therapeutics of gynecology. Through these specula, an operator might be bold enough occasionally to slit the lips of an external os, and a few successful attempts (out of many failures) were made to close a vesico-vaginal fistula. A kindred spirit to the subject of this memoir, it is true, James Y. Simpson, with originality peculiar to himself, and with boldness foreign to his time, dared to go farther, and divide also the internal os. But this example met with few imitators.

But with Sims came the revolution which upset these conservative, do-little methods, and opened wide the field of active, radical, scientific, and rational treatment by surgical means of the diseases and malformations which formerly were merely trifled with, and left unrelieved. As the promulgator of a new system in gynecological therapeutics, Sims may truly, to use the term of the clergyman who delivered his funeral oration, be looked upon as an "apostle." I will not assert that some of his own doctrines and methods, as well as those of his followers, are not erroneous, exaggerated, or unfounded; that, in fact, as the conservative gynecologists of the present day delight in claiming, there is not now too great a tendency to use the knife, and too little reliance on the "powers of nature," and on the "curative influence of time." But I insist that gynecology, as a specialty, exists only since its chief tendency has become a surgical one, that its greatest triumphs (without disparagement to the patient labors of previous gynecologists)

have been achieved with the scissors, knife, and needle, and that this tendency owes its origin chiefly to the genius of Sims.

In this connection, I may say that it has always seemed to me that his greatest achievement was the perfection and popularization of his duckbill speculum and the methods of examination and operation through it. (He cannot really be said to have invented the principle, although he doubtless discovered it independently, because the idea of a perineal retractor had been advocated by Ulrich, of Vienna, and Simon, then of Rostock, before Sims began his investigations.) The use of the silver-wire suture, by which the suture of deep-seated parts was facilitated, and the long retention of the sutures permitted, seems to me but a minor merit, since silk can be, and has been, successfully employed by many gynecologists for these same operations.

The estimate placed by Sims himself on his work is conveyed in a letter received by the writer on October 10th, 1883, acknowledging the receipt of a reprint of an address on "Specialism in Medicine," delivered before the Medical Class of Dartmouth College, August 2d, 1882. In this address, the writer takes occasion to say that "it is only since the invention and popularization of Marion Sims' peculiar methods of examination and operation that gynecology, as a distinct specialty by itself, can be said to exist. Certainly, within the quarter of a century which has elapsed since that discovery of Sims, the specialty of gynecology has made more rapid strides, and attained greater results than during all the preceding ages. . . . Récamier, Simpson, and Sims . . . may properly be called the fathers of modern gynecology. And to the men who, while not forgetting the precepts of the older teachers, have accepted and improved upon the new methods devised by Sims, may the recent great advances and present high position of the specialty be attributed." Dr. Sims writes as follows:

267 MADISON AVE., OCTOBER 10TH, 1883.

Thanks, my dear Dr. Mundé, for copy of your specialism lecture. I had read it in the *Atlantic Medical Journal*.

Your estimate of the value of my labors and of their place in the growth of gynecology will, I am sure, be the verdict of your followers.

It is given to but few men to live to see themselves understood and their labors appreciated.

Again thanking you, believe me,

Yours most truly,

J. MARION SIMS.

Sims was not a voluminous writer, and did not always confine himself to his specialty. His only book was the "Uterine Surgery," which he was engaged in re-writing when ill-health concluded his labor on it, unfortunately, as it proved, forever. It is a great pity this book could not have received the corrections and amendments of an increased experience and a more matured judgment, for doubtless much would have been altered and improved which now still stands as "Sims' practice."

Of his pamphlets, one of the first was on "Silver Sutures in Surgery;" then "Trismus Nascentium;" "The Microscope in the Sterile Condition;" "The Discovery of Anesthesia;" "Septicemia in its Relations to Ovariectomy;" "Intrauterine Fibroids;" and more recently, "The Treatment of Epithelioma of the Cervix Uteri" (*AM. JOURN. OBST.*, July, 1879); and "Treatment of Stenosis of the Cervix Uteri" (*Am. Gyn. Trans.*, vol. iii., 1878); "Chole-cystotomy;" "Treatment of Gunshot Wounds of the Abdomen" (called forth by the discussion on President Garfield's wound), and some other minor papers.

His style of writing, even more than his delivery in speaking, was fluent, eloquent, simple, and incisive, bearing in every sentence the impress of enthusiasm and honest conviction. It was this one characteristic of the man, his evident honesty of purpose, which attracted and convinced many who otherwise would have doubted the wisdom of his bold and original methods.

As an operator, Sims was bold, quick, dexterous, and graceful, and few can surpass him, even now, in the ease and certainty with which he performed the operations for which his peculiar instruments were devised. He performed many capital operations, both ovariectomy, and laparotomy for large fibroid tumors; but he never aspired to the name of a voluminous ovariectomist. But he kept himself fully informed on the modern improvements in these operations, and only three years ago spent some time in Edinburgh studying Keith's method,

an interesting sketch of which he wrote for the *AM. JOUR. OF OBSTET.*, vol. xiii., 1880.

Personally, Dr. Sims was one of the most amiable and lovable of men. A genial, hearty manner, so common to the sons of the "sunny South," a certain sympathetic charm of voice and action, which few could resist, and a frankness and kindness, especially pleasing to the young men who called to ask his counsel; a warm-hearted, impulsive nature, easy to arouse to anger, and ready to melt in tears; forgiving, gentle, playful, even, at times; modest and unassuming; in fact, a nature of which even the faults seemed lovable—these were the characteristics of J. Marion Sims.

That these attributes not only exercised a powerful influence over his professional brethren, but also endeared him to his patients, by whom he was adored, is evident.

He was essentially a family man, cared little for social triumphs, was sparing and frugal in his habits, using neither wine nor tobacco, and devoted what leisure his professional duties gave him to study and literary work. Of late years, his large practice and, at times, failing health, prevented his competing with his contemporaries in the literary contest which is the aim of most of our present gynecologists. I fear many an unfinished article will be found among his papers.

Although not a "society man," Sims delighted in extending the hospitality of his home to all who presented themselves favorably to his notice, and the entertainments which he gave to distinguished visitors were lavish in their profuseness. He was generous to the poor, and his name is missed on but few of the many subscription lists for charitable purposes which circulate continually in New York.

While demanding liberal reward for his services to the wealthy, many a poor woman (often, no doubt, poor only by her own statements) benefited by his kindness of heart, and marvelled at the smallness of his fee. That this kindness was often abused, goes without saying.

Rather above medium height, slender, always cleanly shaven, with thick hair and bushy eyebrows; clad in black broadcloth, and scrupulously neat in his attire; always polite, courteous, and quietly dignified, Sims never failed to attract attention. The portrait accompanying this memoir is from a photograph

taken in England three months before his death, and is an excellent likeness. I am indebted for it to his son, Dr. Harry Marion-Sims, of New York.

It certainly has not been my intention to indulge in a fulsome eulogy of the great man whose loss we all deplore. While knowing him well, although meeting him but occasionally, the writer has not felt compelled by personal obligations to exceed the bounds of respect due the eminence of the man or his memory in preparing this sketch. It is simply a modest tribute paid to one of the most genial men whom the writer has had the fortune to meet, and the greatest gynecologist whom America has produced.

The name and deeds of Sims are immortal!

PAUL F. MUNDE.

QUARTERLY REPORT ON THE PROGRESS OF OBSTETRICS AND GYNECOLOGY IN FRANCE.

BY

A. AUVARD, M.D.,

Interne at the Maternité of Paris.

I.—LITERATURE.

AMONGST the most important works which have appeared in France during the past quarter, I would mention the second edition of DE SINETY's Treatise on Gynecology and the second volume of M. HENRI ROGER's Clinical Researches in the Diseases of Infancy.

M. de Sinety is a careful and conscientious writer. His Treatise on Gynecology bears the imprint of learning. Skilled in anato-pathological research, a disciple of Professor Ranvier, M. de Sinety is an accomplished histologist who has, with his microscope, elucidated many facts bearing on the pathology of women, and, thanks to this method of investigation, made many interesting discoveries. The original side of this book, on which its value chiefly depends, is the careful manner in which the pathological anatomy of the diseases of women is handled. Let no one

expect to find in it a detailed description of the manifold operative procedures belonging to the surgery of women. M. de Sinety is a histologist and a physician, in no sense a surgeon. He speaks only of what he knows. His object has not been to make a compilation.

Although clinical researches into the diseases of infancy are not closely related to our specialty, still, owing to the two subjects being in a measure related, I have thought it worth while to refer briefly to this work. This volume is complementary to one which appeared about ten years ago, and had not as yet been supplemented. M. HENRI ROGER is already advanced in life, being a near contemporary of the late esteemed PROFESSOR PARROT, well known for his writings on marasmus. The author's work, hence, is of a ripe kind, and M. ROGER writes with the weight of authority and clinical experience. It affords genuine pleasure to read the pages of this volume devoted to the consideration of infantile syphilis and the placenta.

Other French publications are of far less importance, and I will simply mention them.

DR. MARTINEAU, physician to the Lourcine Hospital—a hospital reserved for venereal affections in women, even as the hospital *du Midi* is intended for venereal affections in the male—has collected in pamphlet form his lectures on the malformations of the vulva and anus produced by masturbation, saphistry, rape, and sodomy. It is a malodorous subject to write about, and, though M. Martineau has covered it with a coating of scientific varnish, it will be read probably with more interest by the layman eager for scandal than by the man of science. Of what use the book, then? it will be asked. It is of practical value as elucidating certain knotty points for the medico-legalist, is the answer. I will simply question this. It is of value theoretically, is another answer, since it teaches the physician how to read that diagnostic story from the genital organs, which the woman would carefully conceal. I would question this also, though with more reservation.

Two interesting articles have recently appeared in the *Annales de Gynécologie*. The one, by DR. L. H. PETIT, on ileo-vaginal artificial anus and intestino-uterine fistulæ. It constitutes a careful study of the subject, and, though it contains no new facts, reproduces the cases of the kind which hitherto have remained scattered throughout literature.¹ The other, from the pen of DR. BROUARDEL, Professor of Medical Jurisprudence at the Faculty of Medicine, Paris. This article sets forth the causes

¹ Vide A.M. JOURN. OBST., September, 1883, p. 989.

which may lead to an erroneous diagnosis of attempted rape on children. The author, with his recognized ability, emphasizes the numerous precautions which the medico-legalist ought to use in forming his opinion and affirming it in such cases.

II.—CLINICAL OBSERVATIONS.

1st. From the 1st of January, current year, to the 5th of November, a peculiarly interesting number of cases of eclampsia have occurred at the Maternité. Of the ten cases there have been four deaths. The treatment instituted has been chloroform, administered particularly during the intervals in the attacks, combined with enemata of chloral, from six to eight grammes in the twenty-four hours. In a few cases venesection was performed. A study of the fatal cases shows that they differed absolutely in nature from those which recovered, to such a degree that one is tempted to ask if we had not to deal with two different diseases united simply by the fact that in both there were convulsions. In the simple cases, the classic albuminuria existed, always abundant, the convulsions, as a rule, appearing before labor and ceasing after delivery. In these cases any treatment succeeded, and each accoucheur could declare that to the therapeutic method he had employed was due the success. Very different were the fatal cases. Higher range of temperature, coma earlier, as a rule, and more complete. The onset of labor wrought no change. It seemed to be simply an epiphenomenon in the disease, which continued its course without deviation. An icterode tint of the tissue soon appeared, followed by a corresponding change in color of the urine. Of the four cases mentioned above, in one a moderate hemoptysis was present, in two it was very marked. One of these women died suddenly, as the result of a profuse hematemesis. In a word, we find here many symptoms belonging to an infectious disease. The cases were grave, and all treatment unavailing, probably on account of the predominance of this infectious element. At least this is the impression left by a careful study of this series of cases of eclampsia. Perhaps a study of the microbes belonging to albuminuria and its congener, eclampsia, might throw a little light on this subject. The researches of M. DOLERIS, chief of the obstetrical clinic at Paris, on this point are of peculiar interest. This author has always found a minute germ present in the urine of pregnant women affected with albuminuria. The albuminuria, then, of pregnancy becomes a germ disease, and according to the kind of germ present we will have (a) simple albuminuria, (b) albuminuria and slight eclampsia, (c) albuminuria and protracted convulsions, usually fatal.

2d. Corrosive sublimate, under the form of Van Swieten's solution (sol. hydrarg. bichlor. $\frac{1}{1000}$) is still the antiseptic to which preference is accorded in the various obstetrical services of Paris. Its trifling cost, its powerful and certain action, its almost entire harmlessness, gives it supremacy over other antiseptics in use at the present.

3d. Ergot, in its crude form, or as ergotine and ergotinine, which has for so long been employed in obstetrics, has had its prestige considerably shaken at the Maternité, thanks to the use of intravaginal and even intrauterine injections of hot water. During a recent trip to Germany, having seen the happy results obtained from their use by Carl Richter and Runge, I mentioned the fact to M. Tarnier and to the head midwife at the Maternité. The method was at first tried with considerable doubt, but so good were the results that at the end of several days hot injections was the only means used, to the exclusion of ergotinine, and for the last three months neither ergot nor any of its derivatives have been employed at the Maternité. At the outset the injections were simply vaginal, and only in cases of hemorrhages, a solution of the sublimate ($\frac{1}{2000}$) being used. With increasing familiarity with the method, however, they were given intrauterine and after every labor, the strength of the solution being $\frac{1}{1000}$. The three advantages belonging to this method are that it is hemostatic, antiseptic, and seems to favor involution. For the purpose of injecting, a glass irrigator with a canula of the same material is used, and the temperature of the solution should be at 50° C., or a little above—a degree, in fact, just short of burning the accoucheur.

4th. I would mention two interesting cases which occurred in M. Budin's service at *la Charité*. In the one there was simultaneous rupture of the membranes and the vessels of the cord which had a velamentous insertion. There was a sudden gush of blood from one of the ruptured vessels. It was a twin pregnancy; the placenta were distinct; there was a marginal insertion of the cord of the second placenta.¹ In the second case we witnessed an occurrence the explanation of which is but hypothetical, not proved. I refer to the obliteration of varicose veins of the lower limbs in a woman at the eighth month of pregnancy who carried her child to term, a fact in favor of the physiological theory of the pathogeny of varicose veins, and against the mechanical theory.²

5th. Dr. Olivier has recently communicated to the Academy of Medicine the case of a gravid woman who, at the beginning of

¹ Archives the Tocologie, September, 1883.

² Progrès Médical, November 3d, 1883.

pregnancy, aborted during an attack of diphtheria. The patient concealed the fact of miscarriage, but at the autopsy the uterus presented the signs of a recent delivery. Crouzat,¹ after a search through medical literature for an analogous case, found but another reported by Underhill, of Edinburgh, where the patient aborted at the eighth month immediately after tracheotomy for a diphtheria, which eventually proved fatal.

6. Dr. Terrillon has reported to the Surgical Society of Paris three interesting cases of multilocular cysts of the ovary with implication of the broad ligaments, rendering ovariectomy very difficult. In one case enucleation was possible, and the patient recovered. In the two others the operation was incomplete, drainage was established, with the result of one success and one death. The author advises enucleation where it is practicable; if not, incomplete removal followed by drainage.

7th. A case of pelvic malformation due to osteomalacia has recently entered the Maternité. The patient was in her fourth labor, the three others having been normal and at term. At the time of her third labor she had been attacked by a disease which, according to her account, had been pronounced paraplegia. At her last labor the affection of the bones seemed cured; the pelvic bones were no longer soft; nevertheless, recourse to craniotomy was necessary to complete the delivery. The cephalotribe was at first tried, but the cranioclast was necessary for extraction. (The report of this case will probably appear in the December number of the *Annales de Gynécologie*.)

8th. At the meeting of August 27th, 1883, Professor Sappey reported the case of a fetus which had been retained in the abdomen for fifty-six years. It was contained in a calcified pouch to the side of the uterus. The woman became pregnant at the age of twenty-eight and died at eighty-four at Quimperlé, in Brittany. This case is remarkable for the wonderful state of preservation of the fetus, whilst in parallel cases in medical literature the product of conception was more or less mummified or calcified.

III.—ITEMS.

M. T. N. Depaul, Professor at the Obstetrical Clinic of the Faculty at Paris, died, on the 21st of October, 1883, at his country residence in the Pyrenees. The renowned Professor was on the point of returning to the direction of his clinic, when a pneumonia cut him off in his prime.

Professor Pajot, who hitherto has filled the chair of the Theory of Obstetrics at the Medical School, has been appointed to succeed

¹ Progrès Médical, October 6th, 1883.

M. Depaul in the Chair of Practice, and will in a few days take possession of it.

This promotion of Professor Pajot leaves a vacancy at the School of Medicine in the chair of the Theory of Obstetrics. The appointment will not be made for nearly two months, but the opinion is nearly unanimous as to the appointee, and so, without fear of being contradicted by the result, we may announce the nomination of M. Tarnier.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, November 1st, 1883.

The President, R. A. CLEEMANN, M.D., in the Chair.

DR. B. F. BAER related the following cases, the clinical histories of which present some points which he thinks are instructive and worthy of discussion. They are somewhat unusual in character, and remarkable that they all occurred within a period of thirteen days.

FORCEPS LABOR, FIFTH POSITION.

On October 17th I was requested by Dr. ——— to visit his patient, Mrs. H., who had been in labor thirty-six hours, prepared to perform craniotomy. She was a primipara, forty-three years of age.

I found the patient nervous and exhausted, the soft parts dry and rigid, the os only partially dilated, and the membranes ruptured many hours. The head, a large one, was in the cavity of the pelvis, and, whilst not impacted, it was nearly so. The larger portion of the head was posterior and to the left, the smaller portion, anterior and to the right. The fetal heart-sounds were heard in the left lumbar region, and nowhere else. I therefore diagnosed a left occipito-posterior, or fifth position of Baudelocque.

I placed a vectis, and endeavored to assist rotation forwards, but failed to make any impression. I next adjusted, with some difficulty, Simpson's forceps, and by traction during uterine action, with gentle efforts at rotation, allowing the forceps to turn as the occiput rotated anteriorly, that process was finally accomplished in about two hours of hard work. I now removed the blades, and after finding that the head could not be delivered without it, I re-adjusted the instrument and assisted in extension of the head, delivering a living child said to have weighed twelve pounds. There was no laceration of the perineum. Both mother and child have

done well. The case is interesting because of the age of the primiparous patient, and in the position of the occiput, which is rare.

ARM PRESENTATION; PODALIC VERSION.

On October 23d Dr. ——— requested me to see a patient with him, a girl sixteen years of age—a primipara at full term—in labor about twenty-four hours, and trunk presenting.

In general appearance she resembled more a child of twelve than a girl of sixteen. The external genitals and vagina were small and undeveloped. The abdomen was greatly distended, globular, and fluctuating. Palpation was of only negative diagnostic value, probably on account of the large quantity of amniotic fluid. But I thought I detected the head in one iliac fossa, and the breech in the other. Auscultation revealed the fetal heart-sounds, feebly heard in the right iliac region. The upper portion of the vagina was distended by a large, protruding “bag-of-waters,” and the os uteri was fully dilated. I could only slightly touch the presenting part, which was entirely above the superior strait. I detected what I thought to be a limb, and from what I had learned previously by inspection, palpation, and auscultation of the abdomen, I believed it to be an arm. I then dilated the orifice of the vagina preparatory to passing my hand, should that be found necessary, after rupture of the membranes, which I now did, and found a shoulder presenting and an arm on the verge of passing the os. This I arrested, and made version by the feet. I preferred this to version by the vertex, because I deemed it easier and less dangerous to both mother and child to effect delivery in that manner than to apply the forceps in this special case. The child was alive, but feeble. The body passed through the narrow vagina very slowly, and only after pressure on the fundus of the uterus, until the head reached the floor of the pelvis. Here, by assistance, the occiput rotated forwards and the head was arrested. Flexion of the head could not be made to occur by supra-pubic pressure, and by pressure upon the nape of the neck, whilst a finger or two acted upon the anterior surface of the head through the rectum. I then quickly adjusted the forceps, and carrying the handles forward with the body of the fetus, made flexion, and delivered a living child. There was not the slightest laceration of the perineum.

The uterus did not contract well; and although ergot was administered, and time given for the organ to recover its tonicity (thorough kneading being used meanwhile), when the placentia was expelled a smart post-partum hemorrhage followed. This was easily controlled by the application of pure vinegar to the cavity of the uterus, injected by means of the long-nozzled uterine syringe, which holds about half an ounce. I prefer this method of introducing the vinegar to any other, for the reason that it is more easily and thoroughly applied. I carry the nozzle, guided by the index-finger, as in the introduction of the sound, into the uterine

cavity, and project the vinegar, without force, over the surface. This can be repeated if necessary, which is seldom. Too much praise cannot be afforded Prof. Penrose for his earnest advocacy of the use of vinegar in the treatment of post-partum hemorrhage, the result of uterine inertia. In my experience, it has never failed to secure firm and continuous contraction, when properly applied. It is simple, antiseptic, and harmless.

ARM PRESENTATION; PODALIC VERSION.

October 29th Dr. ——— sent for me, stating that he had a case of shoulder presentation, that the membranes were ruptured, and the os only partially dilated. He had attempted to make version by the feet, and had brought down an arm in mistake for a foot. I found a primipara, æt. twenty-two years, illegitimately pregnant, at full term, feverish and excited. A large, fat right arm occupied the vagina, and the shoulder was jammed into and projecting through the os, which was firmly contracted around it. It was a dorso-posterior position, and the head was in the right iliac fossa. All the liquor amnii had been drained away, and the uterus was closed tightly around the child, which was apparently dead.

We administered ether, and I at once began an effort to bring down a foot, deciding that version by the vertex here could not be made, because the arm could not be returned to the uterine cavity, and, even if the arm had not been down, I feel sure that the bipolar force would not have been great enough to have brought that head to the superior strait. But to get through the narrow vagina and rigid os, which were filled already by the arm and shoulder, was one problem, and another, apparently greater one, was the turning of the large child in a contracted uterus. An attempt was, however, not only justifiable, but obligatory, for the sake of the child, of whose death we were not sure. Then, embryotomy, in a case of this character, would, I believe, have been attended with greater danger to the mother than version. I gradually inserted my hand and carried it into the uterine cavity, and with it I tried again to replace the arm, but failed. My hand was now so benumbed that almost all sensibility was lost. However, I finally reached the feet, selected the uppermost or left one, and began my efforts at version, assisting all my internal manipulations, of course, by placing the external hand on the abdomen, and acting with it on the opposite pole of the child. When I made traction on the leg, the arm advanced further into the vagina, and it now seemed that I should certainly be compelled to give it up, the difficulties appeared so great. But patience and perseverance are cardinal virtues here, and by exercising them to my utmost capacity, I succeeded in getting the foot and leg into the vagina, where I secured them with a fillet. I now gave this to Dr. ———, and whilst he made traction upon it, I pushed upon the shoulder, and succeeded finally in revolving the child on its long axis, causing the arm to ascend and the leg to occupy its place in the vagina. The re-

mainder of the delivery was that of a difficult breech case, where traction on the child and pressure on the fundus of the uterus are imperative. The child was dead. The mother reacted well, and has not presented an untoward symptom. There was slight laceration in the sulci on either side of the vagina, not through the cutaneous surface and not enough to require suturing.

BREECH PRESENTATION.

Twenty-four hours later, on October 30th, my friend Dr. Wm. L. Taylor requested me to see with him Mrs. X., a primipara, thirty-five years of age, who had been in labor twenty-four hours, the breech presenting in the left sacro-posterior position. The membranes had ruptured twelve hours previously; the os was rigid and only slightly dilated, and the breech was impacted in the superior strait, which seemed to be narrow. The patient was short of stature, fat, and had a small vagina. It was thought that the child was dead; but of this we were not sure.

Was there any use in waiting longer for nature to effect delivery? We decided that there was not, and, I believe, correctly. An attempt at traction was made, by acting on the thigh; but it was futile. I passed my hand with great difficulty into the cavity of the uterus, which closely surrounded the child, and endeavored to reach a foot, but found that the legs were extended; and it was only after I had advanced my hand absolutely to the fundus of the uterus that I secured the desired member. The uterine cavity was now so rigid and full that it appeared impossible to flex the leg and extend the thigh. But here perseverance again succeeded, and the leg was brought into the vagina. Delivery was finally consummated by the greatest effort. The child was dead and, from appearances, had been so for some hours, as Dr. Taylor had suspected. The mother recovered, as after an ordinary labor.

PUERPERAL CONVULSIONS.

A few days before, October 21st, there entered my service, at the Maternity Hospital, a girl, eighteen years of age, illegitimately pregnant, and near term. She presented a depressed appearance, and was pale and puffy from edema. Her urine was examined at once and found to contain a large quantity of albumen, and some casts. Her labia minora were so edematous that she walked with difficulty.

She was placed upon a treatment consisting of Basham's mixture, digitalis, laxatives, and warm baths, with good food. On the 20th, the nymphæ were so greatly distended that I feared obstruction to delivery, which was about to take place; I therefore made about a dozen small punctures over their surfaces. This was followed by a very free discharge of serum, so that in the evening the labia were reduced more than one-half. During the night, labor occurred, and she was delivered naturally, at seven A.M. on the 21st, having been attended by my assistant, Dr. J. P. Pyle. There were no

symptoms during the labor, nor immediately after it, to attract attention; but, before leaving her, he administered thirty grains of the bromide of potassium as a safeguard.

At nine o'clock, he was hurriedly called and found her just recovering from a convulsion. He at once sent for me, and began the administration of chloroform. But before I reached her, at ten o'clock, she had had two more seizures, and just as I entered the room, she went into another, which was one of the most terrific convulsions I have ever witnessed. I immediately opened a vein and allowed about sixteen ounces of blood to flow. I will confess that I did not want to take blood from this patient, because she was in such an apparently low condition. The bleeding did not seem to have the slightest effect, for very soon after it she had another convulsion, fully as severe as the one preceding. Since the first attack, there had been given, per rectum, twenty grains of the hydrate of chloral, and forty grains of the bromide of potassium; and, per os, one-fourth of a grain of elaterium. But the convulsions continued to recur, unless the patient was kept constantly under chloroform, and coma was deepening with each attack. I now injected, hypodermatically, three-fourths of a grain of the sulphate of morphia. This was at 11.30 A.M. She did not have another convulsion, although no more chloroform was administered until 2 P.M. At this time, she had a slight one, and at 2.30 another much more severe, when I repeated the dose of three-fourths of a grain of morphia. After this, she had no more convulsions. The dose of elaterium was now repeated, and the kidneys stimulated by large doses of saline diuretics, administered by the rectum. The bowels moved freely and repeatedly soon after the last dose of elaterium was given, and the kidneys responded promptly; but the urine became nearly solid, when the test for albumen was applied, and casts were so numerous, and of such a character, that an unfavorable prognosis was pronounced by the competent microscopist who made the examination. The patient, however, came gradually out of the profound coma, but did not recover consciousness until nearly three days had elapsed, becoming at times wildly delirious and maniacal. As soon as she could swallow, I resumed the administration of Basham's mixture and digitalis, and on the next day added quinine and ergot, the latter especially to restore tone to the capillaries, and thus assist in improving the condition of the brain. Milk and beef tea were given largely. The patient will leave the hospital to-morrow, although her urine still contains albumen in considerable quantity.

If uremia is ever the cause of eclampsia (which is not settled), this case presented the kidney state which is usually found in cases said to be of that origin.

Since it is *apropos*, I will relate a case which was probably not of uremic origin, because the urine did not indicate the slightest disease of the kidneys.

PUERPERAL CONVULSIONS.

My friend, Dr. J. B. Deaver, asked me to assist him in the delivery and treatment of a case of convulsions. The patient was eighteen years of age, a primipara, and unmarried. The occiput was posterior and in the hollow of the sacrum. The first convulsion occurred after the head had passed the superior strait, and it was a very severe one. Dr. Deaver immediately bled, and very freely. Another convulsion occurred soon after the bleeding, although chloroform was administered and chloral given by the mouth. When I reached her, she had had three attacks and was profoundly under the influence of the anesthetic, and, of course, could not convulse in that state. I adjusted the forceps and delivered, with the occiput posterior, being unable to rotate it anteriorly. The anesthetic was now removed, and, not long after, another violent convulsion occurred. I now injected two-thirds of a grain of morphia under the skin. She did not have another seizure, and made an uninterrupted recovery. As stated above, there was not the slightest evidence of disease of the kidneys, either before or after labor. The cause here was reflex, the patient being predisposed by a depressed mental condition, etc.

The first indication to be met in the treatment of puerperal eclampsia should be to control the convulsions. I do not think it will be gainsaid that the prognosis becomes less favorable with each recurrence. I believe that morphia, administered hypodermatically in a large dose, and repeated, if necessary, is one of the most efficient, if not the most efficient, means which we possess for that purpose. In the next case which I am called to treat, I shall give one grain. I will bleed, if I think that it is indicated, and shall use chloroform; but I will certainly give the morphia. I will then attend to elimination through the bowels, kidneys, and skin. Dr. Clark, of Oswego, N. Y., first brought the morphia treatment before the profession, in a fearless and excellent paper, published in the *AMERICAN JOURNAL OF OBSTETRICS* for January, 1880, which is worthy of study.

DR. ELLIOTT RICHARDSON thinks the extent of dilatation of the cervix a very important point in considering the advisability of version in shoulder presentations. When the fetus is in a transverse position it cannot descend, and as the cervix dilates it slips upward on the neck and chest of the child, and thus puts the vagina in a condition of longitudinal tension and consequently of narrowing. Any sudden or extreme attempt at dilatation of the vagina, when in this condition, involves a great risk of laceration. In Dr. Baer's case, the narrowness of the os uteri was a favorable circumstance for podalic version.

There is a wide difference in the treatment of puerperal convulsion between this country and Germany. Carl Braun strongly discountenances bleeding and recommends chloroform with the administration of benzoic and citric acids to assist the action of the kidneys. He considers that the prime object is to put the body at rest.

DR. W. T. TAYLOR thought that in Case II., if the method of Dr. Wright, of Cincinnati, for the correction of the shoulder presentation by converting it into a vertex had been tried, the difficulties and dangers of a version by the feet might have been avoided.

He does not think bleeding should ever be omitted in the treatment of puerperal convulsions in plethoric patients. Bleed freely and give chloral in large doses by the rectum. He thinks the use of opium should be preceded by bleeding.

DR. ALFRED WHELAN has tried one-quarter grain nitrate of pilocarpine hypodermatically after bleeding, the result being successful. The use of the pilocarpine did not seem to be followed by serous effusion. In one case in which no treatment of any kind had been used, an autopsy showed all the serous cavities filled with effusion. He thought the arterial tension consequent on the convulsion was the cause of the exudation.

DR. R. A. CLEEMANN had tried all plans and none of them were certainly successful, every method would fail at times and any method will be followed by recovery. He thinks bleeding should be tried in every case, to remove the vascular tension which is the great source of danger.

DR. B. TRAUTMANN had under his care a primipara, plethoric, who was suffering from puerperal convulsions; she was bled, a large dose of calomel was given, chloral was administered and pilocarpine was injected, but all without effect; the patient died. In another case the urine contained fifty per cent of albumen with casts and no convulsion occurred. What is the relation between albuminuria and convulsions? Is the origin of the convulsion in the nervous system, and the albuminuria a result?

DR. H. F. BEATES.—The presence of urea in the blood being generally considered a prime factor, most of the forms of treatment have reference to its elimination. Bleeding should be very free to act in that manner, and if it is prompt and free it will be followed by improvement; pilocarpine acts as an eliminator of urea by the skin, thus relieving the kidneys and the system. He had treated two cases by this method and both had recovered.

DR. PHILIP M. SCHIEDT had recently under his care a primipara aged twenty-five years; she had convulsions for four hours, chloral and bromide of potassium had been given freely, but with no effect; a hypodermatic injection of three-quarter grain of morphia sulphate was followed by quick relief; she was very plethoric, but there was no need of bleeding after the use of the morphia.

DR. BAER, in Case II. had considered version by the vertex, but thought that he could deliver more quickly, and with less danger to both child and mother, by means of podalic version. He considered elimination a false principle in the treatment of puerperal convulsions. First stop the convulsions, eliminate afterwards if there be any necessity for it. How much elimination can be effected by drawing twenty or even forty ounces of blood? The majority of these patients need all of the blood they have; they have none to spare. There is a neurasthenia at the bottom of these attacks. The patients are generally nervous and depressed from circumstances connected with their physical and social condition. Dr. Penrose at his lectures at the University taught bleed—bleed—every case that was bled sufficiently got well; every case that was not bled died. Dr. Carson's lecture followed immediately after that of Dr. Penrose, and he was as bitterly opposed to bleeding as Dr. P. was enthusiastic in its advocacy. He has

been afraid of pilocarpine because, its action once established, cannot be controlled; he thinks, however, the effusions observed have been caused by the convulsions and not by the remedy. Morphia, used hypodermatically, is the remedy upon which he puts dependence, it will control the convulsion. Any medicine administered by the mouth or rectum must be of slow and uncertain action, because of the slowness of absorption from the alimentary tract.

TRANSACTIONS OF THE WASHINGTON OBSTETRICAL AND GYNECOLOGICAL SOCIETY.

Stated Meeting, held May 25th, 1882,

DR. C. S. BUSEY, *President, in the Chair.*

Dr G. N. ACKER read a paper on

RETAINED PLACENTA AFTER ABORTION.

After referring to the evils following abortion, and the necessity of having a clear and definite idea of the treatment in such cases, Dr. Acker gave the history of a case occurring in his practice. The patient miscarried three months previously, since which time she has had frequently recurring hemorrhages which were only temporarily checked by remedies. After dilatation with a sponge tent, the finger was passed into the uterus, and a so-called "fibrinous polypus" found tightly adhering near the internal os. This was removed with difficulty and the hemorrhage checked by the application of persulphate of iron. The patient's health gradually returned. Another abortion was followed by the birth of twins, and after two years another abortion, but there has been no return of the metrorrhagia. Commenting on the case, Dr. Acker referred to the fact that, although the secundines may, in some cases, be retained for lengthy periods, without detriment, yet in others, patients die from recurring hemorrhages, or are left with chronic inflammations of the pelvic organs; he therefore concludes, "that the treatment which gives the best results in such cases is to empty the uterus, as soon as possible, of all retained secundines."

DR. FRY said Dr. Acker had written a paper in a good cause. Not one of the modern text-books gave the bold advice to remove the placenta immediately after abortion. Lusk came the nearest to advising that practice, while Leishman and others, who fully recognized the danger of retention, did not directly advocate this procedure. Nearly all recent writers in the journals recommend the immediate removal of the secundines. The dangers recognized by authorities are hemorrhage, either immediate or remote; septicemia, which may appear in from twelve to forty-eight hours; and inflammation, local or diffused. Phlegmasia dolens and chorea had also been traced back to retention of the secundines.

According to Barker, more women die from hemorrhage due to abortion than to the same cause occurring at term. The recommendation of some practitioners is to let the secundines alone. In a discussion in the New York Obstetrical Society, the majority were in favor of immediate removal from the uterus of membranes and placenta, and in answer to a circular letter addressed to the members of the Maine Medical Association, a large majority advised the same course of treatment. As to the details of treatment, Barker recommended vaginal injections of hot water, while Harrison used intrauterine injections of the same agents. Dr. Fry recommended the use of the curette followed by intrauterine applications of iodine. He has also frequently resorted to the use of placental forceps without any injurious effects. Lusk advises that no instrument be introduced into the uterus; he presses down the uterus from above and passes the index finger into the uterus, and is thus enabled to remove the offending material. Simpson, as early as 1876, advised the removal of the placenta by the fingers; he seized the anterior lip of the cervix with vulsellum forceps and dragged the uterus down, thus facilitating extraction. Barker uses a wire curette.

DR. C. E. HAGNER referred to that class of cases in which hemorrhage occurs long after it was supposed that the placenta had escaped. This form of hemorrhage, he thought, proceeded from the former site of the placenta which had acquired a fungoid condition somewhat resembling a granulating wound. Inasmuch as slight causes would induce bleeding, sexual intercourse should be interdicted until the uterus had regained a normal state. Local applications of nitric acid, nitrate of silver, perchloride of iron, etc., were useful in repressing the granulations. He was in favor of the early removal of the membranes, etc.

DR. SMITH said the principles on which cases referred to by the author of the paper should be treated ought to be clearly defined, for in scarcely any other affection could so much be accomplished by prompt and energetic treatment. To temporize is to jeopardize the health, if not the life of the patient. There are two classes of cases which call for treatment: one in which hemorrhage occurs at the time of abortion; the other where the physician is required to afford relief for recurring hemorrhages, coming on at variable periods after the escape of the ovum. In the first class of cases, Dr. Smith's practice is to pass the finger into the uterus and, by depressing the organ with the other hand, remove the secundines. If the procedure gives much pain, he administers ether. While the uterine cavity is accessible to the finger, the opportunity should not be lost of thoroughly exploring it. In the second class of cases, if the uterus will admit the finger, he follows the same course of treatment. But as this can seldom be done, he dilates the cervix with a sponge-tent and administers ether, so that he is able to proceed without giving pain. He protested against the introduction of instruments into the uterine cavity, believing that practice fraught with danger. He had no reason to regret having followed the treatment outlined above.

DR. MAGRUDER said Dr. Smith's experience with the sponge-tent was certainly very favorable. Dr. Magruder had seen a case in which the introduction of a tent led to tetanus and death. He believed with Dr. Smith in the use of the finger, which he always used successfully in extracting the placenta.

At the meeting held June 1st, 1883, the discussion was continued.

DR. J. T. JOHNSON was glad that so practical a subject had been presented for discussion, and commended the practice advocated by Dr. Acker. He did not agree with Dr. Smith, who had, in sweeping terms, condemned the use of instruments for the removal of retained secundines, but held that proper instruments, properly used, were of less danger than Dr. Smith's method of sponge-tents and ether. We could not always get into the uterus with the finger, and the use of tents was of special danger at this particular time. True, Dr. Smith had facts in his favor, but others had reported unfavorable results from the use of tents. He thought a dull curette a safe instrument to use; it could be passed into the uterus without harm, and he knew of scores of cases where the woman had derived benefit from such practice. He was opposed to the use of forceps in these cases.

DR. SMITH said that since the last meeting he had had occasion to practise his plan of treatment. He was called at night to attend an unmarried woman who had miscarried. She had been bleeding three weeks and had had several chills. There was ante flexion of the uterus, which was painful on pressure. Tamponed the vagina and gave ergot. On the following morning he introduced a sponge-tent, and in the evening was able to pass the finger into the uterus and remove the afterbirth. Patient is now doing well. He was glad to observe that his method was substantially sustained by Lusk.

DR. ACKER, in closing the discussion, remarked that most practitioners were disposed to temporize, for he had been informed by several that they left the placenta to come away in its own time, and one physician stated that he had left it in utero for six months. Johnson uses the curette; Mundé does the same, but he just loosens the placenta and then removes it with forceps.¹ He commended the careful use of instruments.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF LONDON.

Meeting, Wednesday, October 3d, 1883.

The President, DR. GERVIS, in the Chair.

HYPERTROPHY OF MAMMÆ.

DR. I. A. MANSELL-MOULLIN showed a patient, aged eighteen, unmarried, the subject of hypertrophy of both mammæ. The patient's health was otherwise good. The nipples were small, the areolæ large.

[¹ In an article published in the February, 1883, number of this JOURNAL I expressed in strong and concise language my advocacy and usual practice of *immediately* removing the secundines after abortion, by the finger if feasible, or by long blunt curette and flat forceps, if this can be done without injury to the mother. I am glad to see more and more followers of this active practice. The sponge tent I do not like in a pregnant uterus. The tupelo is much safer, and equally efficient.—PAUL F. MUNDÉ.]

DR. CHAMPNEYS had seen a case in Professor Billroth's clinic treated by rest in bed, the breasts being suspended from above.

TUMOR OF CLITORIS.

DR. W. A. DUNCAN showed a large fibro-cellular tumor involving the clitoris and both nymphæ, which he had removed.

RUPTURED OVARIAN CYST.

DR. W. A. DUNCAN also showed a multilocular ovarian tumor, one of the cysts of which had ruptured during an ordinary examination, leading to fatal peritonitis.

A committee was appointed to examine and report upon the specimen.

ARRESTED DEVELOPMENT OF ONE FETUS OF TWINS.

DR. EDIS exhibited a specimen of this kind. One fetus was born alive in the seventh month of pregnancy; the other, expelled seven hours before it, was shrivelled, and its placenta atrophied, apparently having died in utero about two months previously.

DR. EDIS also showed twin fetuses at about the fifth month of development.

DR. MALINS had seen a case in which one fetus had been born living at the eighth month; the growth of the other, expelled at the same time, having been arrested at the fourth month. Cruveilhier had illustrated the same condition. Such cases showed the power of toleration possessed by the uterus, and were also interesting in their medico-legal aspect.

PLACENTA SUCCENTURIATA.

DR. CHAMPNEYS showed a placenta succenturiata. It was impossible, from an examination of the afterbirth, to diagnose the retention in utero of such a body.

DR. DALY said these cases were of medico-legal interest. He had been called to a case in which a woman died from hemorrhage caused by a piece of placenta left in utero. An unqualified practitioner had attended the patient, and at the coroner's inquest pleaded that the retained piece was a supernumerary placenta: a view which was accepted by the jury.

GANGRENE DURING PREGNANCY.

DR. SWAYNE related a case of gangrene occurring during the seventh month of pregnancy. The disease came on after a long journey, and attacked the integuments and muscles over a space the size of a man's fist on the upper and inner third of the right thigh. The symptoms had existed about four days before the occurrence of premature labor, but were not very severe until after delivery, when they became much intensified, and proved fatal early on the third day. There was no injury, wound, or erysipelatous inflammation to account for the occurrence.

DR. BURCHELL said that although the case was not clear, he could not help believing it to have been one of strangulated femoral hernia.

DR. HERMAN asked if the case were not one of gangrenous carbuncular inflammation?

DR. SWAYNE thought the most probable explanation was that the gangrene was due to undue pressure on the iliac vessels, possibly from tight lacing, to which the patient was accustomed. The situation of the tumor precluded the idea of strangulated hernia; the vesication and absence of any head, that of malignant carbuncle; and the absence of diffused redness at the onset, that of erysipelas.

ON THE ANATOMY, PHYSIOLOGY AND PATHOLOGY OF THE OS UTERI INTERNUM.

By DR. HENRY BENNET.—The author had in 1849 drawn attention to the existence of a muscular sphincter at the os uteri internum, and this, like all sphincters, was closed when at rest. This fact was accepted by many at the time, but now seemed to have passed out of mind. It had a most important bearing on uterine therapeutics. The ordinary physiological closed state of this sphincter offered resistance to the passage of the metallic sound; but a small wax bougie could be passed through it. By the use of such bougies he had in 1846 discovered that the cavity of the uterus was not straight, but had an anterior concavity. This sphincter was no doubt greatly developed by pregnancy. It opened slightly before, during, and after menstruation, and probably during sexual congress. It was relaxed by disease, such as fibroids, chronic uterine inflammation, endometritis. The easy passage of the sound was therefore an indication of a morbid rather than of a healthy condition of uterus. This fact had an important bearing on the theory and treatment of sterility. If a closed os uteri were presumed to be a morbid condition, then nearly all healthy young women who were examined would be erroneously considered to require surgical treatment.

DR. GALABIN had no doubt of the existence of a sphincter at the os internum: this was shown by the constriction often seen at this point in a laminaria lent, and by the rapid contraction of the os after dilatation. But he could not agree with Dr. Bennet that it was normally completely closed. Where the passage of the ordinary sound was resisted, a smaller one, without a bulbous end, would often pass, if the direction of the canal were hit upon, although a hitch was sometimes caused by flexion of the canal. He thought further evidence was much to be desired as to the cure of sterility by incision or dilatation of the cervix. His impression was that he had seen a larger proportion of pregnancies follow dilatation by bougies than incision. He thought it would be of great value if some of those who performed the operation would give the number of pregnancies following in a complete series of consecutive cases. The only such series he remembered did not show a greater number than might be accounted for by coincidence.

THE PRESIDENT remarked on the interest and value of Dr. Bennet's papers. In former years he (the President) had rarely incised the os internum. But lately, where there was evident constriction (a fact of which he had no doubt) he had done so, and his results had been distinctly better. Where the os internum was fairly patulous, and the constriction affected the os externum alone, he was satisfied with its division.

DR. HEYWOOD SMITH protested against the use of scissors to divide the os externum, for too extensive an incision was thus made, and the power of imbibition possessed by the external os destroyed. The most scientific method of doing the operation was with Sims' narrow-bladed knife.

DR. PLAYFAIR believed very little in stenosis of the os internum, and not at all in its incision for the cure of sterility. Incision of the os externum in well-selected cases was occasionally followed by pregnancy, but he believed it was done far too often and too indiscriminately. He believed it acted not only by enlarging the os, but by remedying the conical condition of the cervix, which was more often than stenosis the cause of sterility.

DR. CHAMPNEYS pointed out that difficulty in the passage of the sound was not proof of stenosis of the os internum. Difficulty might arise even when the canal was larger than usual, from the instrument being passed in the wrong axis, or from its point catching in a fold of mucous membrane. It was only when the bulb of the sound was gripped during withdrawal that stenosis could be inferred.

DR. AVELING was sure that contraction of the os internum was a cause of dysmenorrhea and sterility, and believed that incision gave more permanent relief than dilatation. After incision he did not use a stem pessary, but passed the sound daily for a week, and then less often, till healing had taken place.

DR. EDIS thought there were instances in which division of the internal as well as the external os was needed. Each case must be treated on its own merits, it being impossible to lay down any general rule. After incision he used a stem pessary, the patient being carefully watched.

DR. MURRAY thought division of the os uteri for sterility alone of doubtful utility. He had seen many cases in which it had been done without good results. It was not free from risk to life, and ought not to be done simply at the request of the patient.

DR. HENRY BENNET gathered that his views were generally accepted, although some might not go as far as he did. Deep division of the cervix had been formerly, and he believed was still too frequently performed by some practitioners. The abuse of surgical treatment might be on the wane in England, but certainly was not elsewhere. It was therefore desirable to establish the anatomy, physiology, and pathology of the os internum on a sound basis. Other waves of opinion were setting in, equally exaggerated in their character; as for instance, in America the unjustifiable sewing up of the lacerated cervix uteri for insignificant lesions easily cured by the simplest local treatment, and with us the abuse of pessaries.

Meeting, Wednesday, November 7th, 1883.

DR. GERVIS, President, in the Chair.

FIBROIDS REMOVED BY ABDOMINAL SECTION.

DR. MEADOWS exhibited two specimens of subperitoneal fibroids, one weighing half a pound, the other five pounds, successfully removed by abdominal section.

VENTRAL PREGNANCY TREATED BY ABDOMINAL SECTION.

DR. MEADOWS also exhibited a fetus successfully removed by abdominal section from a suppurating extrauterine cyst. The

pregnancy had occurred about fifteen months previously. The cyst was stitched to the abdominal wall, and its cavity washed out.

DEFORMED PELVIS.

MR. W. S. A. GRIFFITH showed an oblique rachitic pelvis, occurring without spinal curvature, and due, he believed, to unequal length of the legs.

DR. ROBERT BARNES said that in an early volume of the Transactions he had described a similar specimen.

THE PRESIDENT called attention to the fact that the half of the sacrum, corresponding to the shorter leg, was smaller than the other, although no synostoses of the sacro-iliac joint existed.

THREE CASES OF PYOSALPINX.

By MR. LAWSON TAIT.—The author related three cases of acute peritonitis due to pyosalpinx, cured by abdominal section, removal of the diseased appendages, cleansing and draining of the peritoneum. The *first* case was one of chronic pyosalpinx, made acute by a stem pessary. The tube burst, and acute peritonitis followed. Abdominal section was promptly performed, and the patient saved. Mr. Tait quoted the remarks of the gentleman who sent the case to him as to the effect of the mechanical treatment in causing the disease, the difficulty in discriminating the cases suitable for treatment by stem pessaries, and the dangers of these instruments. The *second* case had already been published in the *British Medical Journal* of February 17th, 1882, and was brought forward here for the purpose of recording the subsequent history, which was that all the symptoms had vanished, and the patient was now perfectly well. The *third* case was one of purulent peritonitis arising from rupture of a suppurating Fallopian tube. The pyosalpinx was due to gonorrheal infection. The left tube only was removed. The patient recovered completely. Mr. Tait had now operated on sixty-five cases of occlusion and distention of the Fallopian tube without a death. In only one had there been failure to completely relieve the patient's sufferings. Six cases had been lost sight of, and two had died since the operation from causes independent of it. The author remarked that cases such as these could not be relieved by anything short of removal of the diseased organs, and that they existed in large numbers, forming a large proportion of the cases which wander about from one practitioner to another seeking relief. He also complained of some unjust and ungenerous criticisms which were frequently repeated to him, and he asked those who expressed such views to come and see his work.

DR. WYNN WILLIAMS protested against the use of a stem pessary in such a case as the one related. If harm followed the use of a stem in such a case, the blame should be laid on the practitioner, not on the instrument. He presumed the pyosalpinx was not attributed to the stem, as it must have been there previous to the insertion of the instrument.

MR. DORAN believed that suppuration of the Fallopian tube was sometimes caused by the introduction of a dirty sound in the uterine cavity, conveying septic matter therein, and setting up a low form of inflammation.

DR. ROBERT BARNES said that Mr. Lawson Tait had opened out a new field in abdominal surgery. It was to be expected that there would be opposition to his views. Every one must have seen cases like those described by Mr. Tait, and his statistics proved that they were amenable to surgical treatment.

DR. W. A. DUNCAN asked Mr. Tait whether in many of his cases the tubes were fixed by adhesions, and, if so, whether the operation was made much more difficult? He had recently seen two cases of pyosalpinx: in one the left tube ruptured into the vagina: in the other, a very characteristic left pyosalpinx entirely disappeared.

THE PRESIDENT thought Mr. Tait took somewhat too gloomy a view of the prognosis in cases of tubal distention. Some cases, possibly of hydrosalpinx, certainly got better without operation. He thought that whatever induced endometritis might lead to tubal inflammation. He asked for further information as to the diagnosis of these cases. He thought that this operation, though the latest, was not the least important of recent advances in abdominal surgery.

DR. HONOLDS asked how it was that these cases were not more often seen on the post-mortem tables of large hospitals? If they were as common as Mr. Tait thought, was it not probable that most of them got well without operative interference?

DR. FANCOURT BARNES congratulated Mr. Tait. He now recognized, by the light thrown by Mr. Tait, several cases of pyosalpinx. He believed he had such a case now under care.

DR. GRAILY HEWITT thought the affection described by Mr. Tait was not a common one. Another cause not mentioned was occlusion of the canal of the cervix uteri. He mentioned a case in illustration.

MR. KNOWSLEY THORNTON asked for the respective numbers of the cases of hydrosalpinx and pyosalpinx. He could not admit that hydrosalpinx was a grave condition: he had met with it often in performing ovariectomy, and believed that its bursting caused little or no disturbance, and was a common mode of its natural cure. Pyosalpinx was more serious, but he believed it was often cured by discharging into the uterus. The distinction between these two conditions was therefore of much importance, and he asked for information as to the diagnosis between them. He had twice operated for pyosalpinx, and in neither case were there more than slight adhesions.

DR. MATTHEWS OWENS had seen some fifteen of these operations, and could vouch for the great good done by them. He believed that many cases of so-called hysteria would now prove to be due to disease of the Fallopian tube. He mentioned a case in illustration. Such cases were not recorded in post-mortem records, because they were put down as peritonitis. The difficulty of diagnosis of these cases was a drawback. But the risk of an exploratory incision was *nil*, and the result, if pyosalpinx were found, brilliant.

DR. GALABIN inquired as to the tube and the fluid used for drainage and washing out the abdomen.

DR. MURRAY remarked on the importance of diagnosis, and congratulated Mr. Tait. He thought the Lock Hospital might afford opportunity for verifying the supposed influence of gonorrhoea.

DR. HEYWOOD SMITH asked whether in cases of hydrosalpinx aspiration should not be preferred to the major operation?

MR. LAWSON TAIT said the words condemning the stem pessary were not his own. He had known gonorrhea given by a dirty speculum, and thought it might be given by a dirty sound. If he were called to a case of puerperal peritonitis sufficiently early to promise a good result, he would open the abdomen, wash out and drain the cavity. But, as yet, he had not had a chance of doing this. He had no doubt that many cases of hydrosalpinx, and some of pyosalpinx, were cured by natural processes. In diagnosis, he depended largely on the history, which started from an inflammatory attack. There was more or less constant pain, aggravated by movement and by intercourse, and menorrhagia, and there were physical signs of pelvic changes. Errors in diagnosis occurred in his practice about once in ten times, and were always instructive. He mentioned cases in which he had taken for pyosalpinx a small dermoid cyst. In these, the initial point in the history was the only thing wanting. Cases of pyosalpinx were not seen in hospital post-mortem rooms, because they commonly died from peritonitis too quickly to come into hospital. They were, however, exceptionally seen in hospitals. Hydrosalpinx and pyosalpinx occurred in his practice, he thought, in the proportion of about three to two. Hydrosalpinx was not dangerous to life, but often caused intense suffering, and therefore he did not hesitate to remove it. He did not think its rupture ever likely to prove fatal. The differential diagnosis between the two could not be made. He used a glass drainage tube, and washed out the abdomen with plain water. He expressed his gratification at the reception of his paper.

A CASE OF IDIOPATHIC GANGRENE OF THE UTERUS.

By MR. LAWSON TAIT.—The patient, aged thirty-four, was admitted into the hospital on account of vague pelvic pain and offensive watery discharge. The uterus felt soft and flabby. The abdomen was swollen, and there were feverish symptoms. The patient died forty days afterwards, and on post-mortem the uterus was found a black, sloughing, stinking mass, having only about a square inch of normal tissue. No reason could be discovered for the gangrene. No operative treatment was permitted, or else it would probably have been quite easy to remove the dead uterus by abdominal section.

AN UNDESCRIBED DISEASE OF THE FALLOPIAN TUBES.

By MR. LAWSON TAIT.—The patient, aged thirty-six, suffered from constant pelvic pain, aggravated during menstruation and after marital intercourse, and was much emaciated and haggard. She had had much fruitless medical treatment. There were no physical signs of pelvic disease, except great tenderness. Mr. Tait made an exploratory incision, and found the fimbriæ of the tubes adherent by curious little nodules, like millet seeds. He therefore removed the uterine appendages, with the result of completely restoring the patient to health. The nodules had been examined by Mr. F. S. Eve, who reported that he could offer no opinion as to their origin or nature, but that they were neither cartilage nor bone.

TRANSACTIONS OF THE GERMAN GYNECOLOGICAL SOCIETY.

SECTION XVIII. OF THE FIFTY-SIXTH ANNUAL MEETING OF
GERMAN NATURALISTS AND PHYSICIANS.

HELD IN FREIBURG, SEPT. 18TH, 19TH, AND 20TH, 1883.

First Session.

PROF. HEGAR *in the Chair.*

THE EMPLOYMENT OF BROMIDE OF ETHYL IN OBSTETRICS.

MUELLER (Berne).—Following in the wake of the experiments repeatedly made of late (especially by Heckermann, of Berlin, 50 cases), M. has employed ethyl bromide in 22 cases of labor. The results were not as favorable as those obtained by H. The action is uncertain. M. employed it 13 times in the second stage. In 5 of these cases the labor pains ceased altogether; in 3, they ceased in part; in 5, no effect was produced. The drug was used 8 times during the whole course of labor: in 2, the effect was very good; in 4, the effect was temporary; in 2, the pains were obtunded only in the first stage. On the average, 50 to 60 grams of the drug were employed. During the narcosis, the pupils dilated and the face became congested. Little effect was produced on the pulse and respiration. M. believes that the retardation in the activity of the contractions is merely apparent. He has witnessed no post-partum hemorrhages and no mal-involution of the uterus.

Quite noteworthy is the fact that a very severe bronchitis set in during the puerperium in two of the cases. The purity of the drug was established by chemical examination. Perhaps the quantity employed (80 grams) was too large.

SCHATZ (Rostock) said he was glad that the narcosis of the normal parturient had not thus far become the rule in Germany. The kymographion shows that chloroform narcosis considerably reduces the force of the contractions. He ascribes the large number of post-partum hemorrhages in England to the frequent employment of chloroform. He fears that similar drawbacks attend the use of bromide of ethyl.

NEUGEBAUER (Warschau) called attention to some points in Heckermann's lecture which he had heard in Berlin: he stated that Rose likewise had experimented with the drug. The latter objected to it because the odor of the expired air of persons anesthetized with bromide of ethyl was disagreeable to patients in the same ward.

MUELLER stated that he, too, had noticed a garlicky odor in his patients, but it was never strong enough to be really objectionable. He added that the drug seemed to affect the fetus rapidly, the expired air of which likewise diffuses that odor.

HEGAR had tried various anesthetics, but had always returned to chloroform. Ether may also be the cause of violent bronchitis which may be fatal to debilitated persons. Bichloride of methylene has the advantage that it has no depressant effect on the heart. Breisky has had one case of death from this drug. Chemical examination showed that the preparation was a mixture of chloroform and alcohol. H. likewise had his sample tested. The result was the same. He thinks it advisable to put the patient under chloroform toward the end of the second, but particularly of the first stage.

MUELLER had also experimented with chloral (5 grams). The pains were moderated in the first, but not in the second stage.

ON LESIONS OF THE PELVIC FLOOR DURING LABOR.

By SCHATZ (Rostock).—Lacerations of the muscles of the pelvic floor have not hitherto received the attention they deserve on account of their frequency and clinical importance. As regards the dislocation of the pelvic viscera, especially prolapsus, they are equal, if not superior, in importance to lacerations of the perineum. Cases may often be seen where the perineum is ruptured beyond the sphincter ani and still the integrity of the pelvic floor has not suffered, neither prolapsus nor descensus having occurred. For the perineum does not appertain to the muscular pelvic floor; it lies entirely below it.

In cases in which the muscular pelvic floor is torn, in which consequently it fulfils its purpose only in part, the intergluteal furrow is found flattened or obliterated even in the dorsal decubitus of the woman: the anus is not lifted forward, the pelvic outlet is insufficiently closed. Where the functional disturbance of the pelvic diaphragm is not excessive, the perineum, indeed, acts supportingly, often with considerable effect.

Median sections furnish no clear picture of the function of the muscular pelvic floor. We should imagine the pelvic strait to be an almost unyielding, circular ring, corresponding to the course of the arcus tendineus pelvis. This ring is filled in funnel shape by the pelvic diaphragm. Slightly below it, the osseous parturient canal ends. In order to continue the course of the latter, the diaphragm as far as its terminal opening behind must be as broad as possible; in front, as narrow as possible. The terminal opening, too, lies so completely eccentrically from the front that there is no muscle at that point, but the margin of the ring remains free for about four centimetres.

The muscles are inserted into the ring in a semilunar form in three systems, thus: the posterior system, the two spino-coccygei muscles, ending on either side at the spine of the ischium or at the end of the posterior third of the ring; the middle system, the two ischio-coccygei muscles joined by the coccygo-anal ligament, being attached on both sides at the median third of the lateral ring; finally the anterior, the two purely muscular halves of the levator ani joined together, ending at the anterior third of the ring, ex-

cepting where it remains altogether free on account of the eccentricity of the terminal opening. The greater dilatability required by this anterior segment is effected by the absence of a tendinous median portion.

When during labor the pelvic diaphragm is unfolded by the advancing head, it is done first with the posterior system, and usually most easily, because the opening it presents, with the portion of the bony pelvis appertaining to it, is nearly sufficient. Hence lacerations here are very rare. Sch. saw them but once. But it is otherwise with the second system. This must be dilated far more. Here lacerations are more frequent. Their usual seat is postero-laterally. They are most evident when the anterior system has remained intact or, having been closed by sutures, has united well. We then feel behind the levator ani rather large, often hernia-like defects, through which the vaginal wall, in persons devoid of fat, can often be pressed almost as far as the external skin. Most important and frequent are the lacerations of the anterior system, *i. e.*, of the levator ani. While the width of the opening is the least, it is subjected to the greatest dilatation: often, too, to injuries during operations, especially with the forceps. Here lacerations occur most frequently in old primiparæ. They are by no means always continuations of ruptures of the vagina and perineum, although this is often the case; for instance, when a vagino-perineal laceration extends around the sphincter ani. Should the levator ani tear laterally, the laceration is not rarely subcutaneous or rather supravaginal, therefore does not communicate with the vagina. This is most frequently the case in lacerations of the levator immediately adjoining its attachment to the descending ramus of the pubis. To these lesions, hitherto but little known, the speaker called special attention. Their cause usually seems to be a contusion of the muscle at its point of attachment, particularly where the forceps were applied obliquely and remained so during extraction. In this way one of the blades is pressed far forward into the pubic arch. But these lacerations occur also without operative interference, and are most frequent unilaterally. It is these latter which are most readily diagnosed. We find, while on the one side the muscle is evidently inserted at the normal place, the attachment on the other side is a finger's breadth or more backward, and the edge of the muscle projects less. Often, especially when the condition has long given rise to prolapsus, barely any muscular projection is to be found. Even if the attachment is severed on one side only, usually the whole muscle, or perhaps the entire pelvic floor, loses its normal tension. The levator pulling merely on one side, does not lift the anus sufficiently forward. Thus the anal furrow becomes shallower, the vulva long and wide, even if the perineum is well preserved. Even if the women remain long in the lying-in, descensus or prolapsus easily takes place sooner or later.

The prophylaxis consists in heightening the succulence and dilatability by baths, irrigations, etc., in gravid women with tense, unyielding musculature of the pelvis, especially in older primiparæ. Furthermore, in preparatory stretching by means of two fingers introduced into the vagina which draw down the whole of the pelvic floor. Particularly to be avoided are the anterior ruptures of the levator ani, because they do not heal at all or with great difficulty. With this view, we must prevent great pressure of the head into the pubic arch; also the extraction of the head with the forceps applied obliquely (comp. above).

The diagnosis of muscular laceration is not easy immediately after labor, because the muscles are relaxed.

In reference to the treatment, in lacerations situated posteriorly or laterally, deep sutures must be inserted, not merely uniting vaginal lacerations possibly present. It is necessary to employ only non-absorbable sutures, as the stitches must remain for one week. Accurate coaptation of the severed surfaces is very desirable. Care must be had that the rectum, which easily slips into these ruptures, is not included in the suture. In lacerations of the levator at its anterior attachment, the suture is placed with difficulty, because the muscular remnant inserted into the bone is often too short. Usually no result is obtained by joining the vaginal membrane attached in front and laterally, because it is not rarely torn, and generally loosened. Very troublesome are the subcutaneous lacerations. The author, thus far, has not ventured to open the vagina so as to stitch the ends of the muscle directly. The evil consequences of laceration of the pelvic floor manifest themselves often at a later period by descensus or retroversion and are all the more disagreeable because Hodge's or similar pessaries resting on the levator do not hold.

Restoration of the pelvic floor by a bloody operation is to be recommended in lateral or posterior, but not in anterior lacerations. For the latter the author recommends his "pan-pessaries" (*Schalenpessarien*, comp. Prochownick's essay in Volkmann's "Samm-lung klin. Vorträge," No. 225).

HEGAR inquired how an accurate diagnosis is to be formed in these lacerations of the pelvic floor.

SCHATZ.—The diagnosis can be made most readily about two weeks post partum, with difficulty soon after labor. The above-mentioned diagnostic points were then briefly repeated.

HEGAR.—Lacerations of the pelvic floor may give rise to very disagreeable sequels. H. had termed them insufficiencies of the closing apparatus. They may occur where the perineum is quite intact. He called attention to a variety of lacerations which had not been mentioned by Schatz—lacerations found on one side of the rugous column or extending around it in horse-shoe form; even complete ruptures occur. They exert a great influence on the direction of the vaginal walls; even prolapse of the uterus may ensue. Nor will lateral incisions during the emergence of the head

guard against these lacerations. They often start precisely from these incisions.

H. distinguishes a posterior and an anterior perineum. The latter likewise becomes elongated, and here lacerations may occur between the urethra and clitoris. The hemorrhage resulting from them is arrested with difficulty. If circumligation be performed around the bleeding point, the suture tracks bleed. In order to avoid this, the circumligation must be at considerable distance. These anterior perineal lacerations occur especially with forceps extractions in which the posterior perineum was meant to be protected, and in this way the anterior perineum was excessively stretched.

SCHATZ had intended to discuss only the lacerations of the pelvic floor. Those mentioned by the preceding speaker did not come within the scope of his paper.

KALTENBACH had never observed ruptures of the anterior edge of the levator ani. He wondered how, in cases such as Schatz had mentioned, the pelvic floor was to rupture high up, while the more exposed perineum was to remain intact.

FREUND thought that the lacerations of the rugous column were particularly to the point among those described by Sch., because every laceration involving its top and penetrating deeply injures the pelvic floor. He was of opinion that ruptures of the pelvic floor are not as frequent as Sch. assumes. He had made numerous autopsies of women with prolapse, but had never encountered the alterations of the pelvic diaphragm described by the speaker. Finally he inquired when the ruptures in question were to occur.

SCHATZ.—During the emergence of the head.

FREUND thought otherwise. The muscle can only be bruised off much earlier. During the emergence it is no longer exposed to traction.

SCHATZ thought that F.'s post-mortem results did not prove anything, for during the dissection the muscles are drawn out and thus a correct appreciation of the relations is rendered difficult. He admitted, however, that the majority of lacerations in the pelvic floor take place in the posterior portion, though the anterior ones are not rare.

PROCHOWNICK (Hamburg) recommended, in order to make an accurate diagnosis of lacerations of the pelvic floor, to give vaginal injections of hot water while two fingers are present in the vagina. Thus the ruptures are felt often with remarkable distinctness as compared with an ordinary vaginal examination.

HEGAR added, in reference to the anatomical relations, that the fibres of the levator ani extend into the recto-vaginal septum. Between the median and inferior third of the vagina the levator ani can be felt as a loop. This may tear, and the laceration can be felt. It is difficult to diagnosticate whether the portion of the muscle has been torn off close to the bone; nor can he conceive why it should tear just there.

SCHATZ.—The point of rupture is not always directly at the bone; but the portion of the muscle remaining there may be so short as to render the suture impossible.

PROCHOWNICK read a paper entitled:

STUDIES ON COTTON-ROOT BARK (RAD. GOSSEYII) AS A SUBSTITUTE FOR ERGOT.

He had experimented with cotton-root bark because he thinks it desirable to find a substitute for ergot. The action of the drug,

when employed during the last stages of the expulsive period, is inferior to that of ergot; but it produces no tetanic contractions. When given during the lying-in, the effect was very satisfactory. But the author was particularly pleased with it in gynecological cases, in hemorrhages continuing after the removal of remnants of abortion. In myomata, the metrorrhagias often diminished as early as after two months, but usually after three, and a reduction in the size of the tumor could also be demonstrated. Infusions of the fresh drug produce the best effects. The American fluid extract is likewise to be recommended. The drug is considerably cheaper than ergot. In reply to a question by Schatz, the author added that the agent can by no means fully supplant ergot.

JUNGBLUTH (Aix-la-Chapelle), in speaking of

THE APPLICATION OF ASEPTIC SPONGE TENTS IN PLACENTA PREVIA, referred to the details of his paper published in No. 235 of "*Volkmann's Sammlung klin. Vortr.*" At present he desired merely to demonstrate the mechanical qualities, the dilatability of his tents. He mentioned that they swell most rapidly in hot water. If immersed in a fluid at blood-heat, they swell more rapidly if their points have been previously dipped in a hot liquid. This fact he demonstrated with sponge tents of different sizes.

SCHATZ thought that the tents, after introduction into the cervix, do not dilate as much as in the test with water.

JUNGBLUTH replied that so great a degree of expansion was not necessary.

MUELLER (Berne) called to mind the latest publications from Berlin respecting the treatment of placenta previa. As regards the mothers, the results attained there are brilliant, not so with the children. Jungbluth's method, therefore, might be tried.

SCHATZ said he cannot look upon sponge tents as practical. Every pain expresses the blood absorbed by them; they then refill. Hence theirs might almost be called a pumping action. He believed a firm vaginal tamponade to be more practical.

FRANKEL (Breslau) thought it advisable to combine with J.'s his own method of preparing sponge tents (dipping in vaseline, rolling in iodoform, powdering the vagina with iodoform, tamponing the vagina with iodoform gauze after the introduction of the sponge tents). Thus the epithelial defects caused by the tents can be rendered innocuous. In several cases of placenta previa he had successfully employed the largest sizes of Hegar's hard-rubber bougies. In general he advised to follow the plan of the Berlin school.

SCHATZ does not believe that the pressure exerted by sponge tents is as great as is usually assumed. He admitted that tents can be made antiseptic, but not so the cervix, the disinfection of which is always difficult. For this reason this method will remain a precarious one.

MUELLER thought that the previous speaker rather underestimated the dilating power of sponge tents. Their force is proved by the dilatation of cicatricial strictures.

SCHATZ denied this. In such cases much less is demanded from sponge tents than in placenta previa, in which the dilatation must be far greater.

JUNGBLUTH briefly summarized his above-mentioned paper which did not seem to be known to every one present. He does not demand rapid dilatation; on the contrary. He ascribes the advantage of his method to the fact that with it the obstetrician can await the complete dilatation of the cervix.

Second Day—Morning Session.

SCHATZ (Rostock) in the Chair.

SIMPSON (Edinburgh) exhibited a new

PERFORATOR—THE BASILYST.

The instrument is merely a perforator, not an extractor. The screw thread cut on its upper extremity can easily be bored into the roof of the skull. After the latter is perforated, pressure on the strong spring attached to the lower end separates the blades. In this way the original, not very large opening can be widened *ad libitum*. Then the cranial cavity is washed out. Should the labor fail to progress after opening the roof of the skull, the base must likewise be diminished with the instrument. The author discussed the various methods of demolishing the fetal skull and the instruments devised for them. For his own he claimed simplicity of construction and application. He had used it in several cases with good results.

MUELLER (Berne) expressed himself as opposed to cephalotripsy and extolled the advantages of the cranioclast.

BANDL (Vienna) called attention to the difficulties which may be presented by the base of the skull where there is great disproportion in size, after perforation of the roof of the skull. Then the former must likewise be diminished, after which the skull can be very much compressed. Usually perforation of the roof suffices.

SIMPSON added that, as a rule, he would leave the expulsion of the perforated child to nature. If necessary, extraction may be tried by the fingers hooked into the cranial cavity. In reply to a question by Freund, he explained that he did not propose to perforate the base of the skull in every case. After operating on the roof of the skull, he waits from one-quarter to half an hour. If then the labor does not progress, he diminishes the base.

SCHATZ (Rostock) thought the time stated to be too short to enable us to judge of the effect of the perforation; besides, he believed that, in very many cases in which perforation is called for, rapid extraction is desirable, especially for the dilatation of the cervix.

KALTENBACH (Giessen) concurred in this view.

BANDL (Vienna) called attention also to those cases in which extraction fails even after perforating the base of the skull. It then becomes advisable to perforate the thorax likewise and apply the cranioclast to the vertebral column.

FREUND (Strassburg) read a paper on

EXTRAUTERINE PREGNANCY.

The material on which the essay was based consisted of sixteen cases under F.'s observation, the detailed histories of which will soon appear in the *Edinburgh Journal*.¹

¹ The paper is essentially a repetition of one presented by Prof. Freund to the Obst. Soc. of Edinburgh, the discussion of which, however, will take place later in the course of the present month.

F. points out that but slight progress has been made in the elucidation of the pathogenesis of extrauterine pregnancy. Many of the former attempts at explanation are without the basis of actual observation; they are mere hypotheses, as for instance the explanation of ovarian pregnancy, which assumes that fecundation and nidation of the ovum may take place in the Graafian follicle when the ovum is retained in the latter because of insufficient size of the rupture and slow escape of the liquor or because the cumulus proligerus is not always situated exactly behind the point of rupture of the follicle. In a case of ovarian pregnancy under F.'s observation, he sought its cause in a functional disturbance of the tube—produced by a sudden very violent fright—whereby the reception of the ovule into the tube was prevented. After a brief review of the causes of abdominal and tubal pregnancy, the author proceeds to the discussion of the formation of the placenta on tissues situated outside of the uterus. He laid stress on the lately manifested endeavor to represent the process of the implantation of the ovule outside of the uterus as altogether easily comprehensible and compatible with the most recent histogenetic views. In connection herewith he calls to mind Waldeyer's interpretation of the inner peritoneal investiture as epithelium; also the fact that, in some animals, the entire peritoneal cavity participates in the formation of the ovum, by means of its epithelium.

F. does not admit any analogy of the placenta formation of an extrauterine implanted ovum with the adhesions by means of which foreign bodies are incapsulated in the peritoneal cavity, or tumors of the abdominal or pelvic cavity become adherent, as had been maintained. He denies that adhesions are capable of starting into renewed growth tumors which, being no longer sufficiently nourished by the source from whence they sprang, have remained stationary or have retrogressed. The adhesions of such tumors are either very deficient in vessels or very vascular as regards veins, while the arteries are sparse and of small calibre. Any blood stasis in the tumor, of whatever origin, then becomes the cause of the dilatation of the veins of the adhesions. It is different in malignant tumors, where the adhesions are really rich in arteries and contribute to the growth of the neoplasm. On the other hand, the growth of non-malignant tumors partially or wholly separated from their source is not an actual increase of their proper substance, but is caused by a sudden augmentation of the volume of blood, by larger effusions of blood into the substance, or by cyst formation with sudden addition to the cyst contents. So also the ovum, developing outside of the uterus, is not nourished by its capsule formed of peritoneal adhesions, but only through the placenta. It is not the point of implantation of the fecundated ovum which possesses anything specific, but the place where the placenta is forming is acted upon by something specific—namely, the allantois with its vessels. Therein lies the formative irritation of the maternal structures for the development of the maternal placenta.

In the diagnosis of extrauterine pregnancy, progress has been made through the perfection of the bimanual exploration of the pelvic organs. Repeated examination will demonstrate a remarkably rapid growth of the tumor. In the beginning of pregnancy, its extrauterine position is often easily ascertained; toward the end, frequently only with difficulty. Inversely, in the beginning the nature of the tumor as ovisac is often recognized with difficulty. However, subjective and objective signs of pregnancy, but especially the rapid growth of the tumor as compared with that of the uterus, will clear up the case. As to the recognition of the site of an almost or quite mature, dead, extrauterine fetus, exploration with a needle may furnish a good landmark. But when employing this means, we must be prepared for instant laparotomy.

Of great importance for the prognosis, diagnosis, and treatment is the seat of the placenta at some point of the intestinal caual.

In such cases there are often present intense intestinal catarrh and violent colic, even in the beginning of pregnancy. In two instances the ovum died in the first half of gestation; the symptoms of chronic septic infection developed.

Basing on his material, F. erects four groups of extrauterine pregnancy: first, tubal pregnancy, characterized by intermittent pains resembling those of dysmenorrhea, and by sudden attacks of peritonitis with symptoms of internal hemorrhage. F. never saw a tubal pregnancy develop beyond the fifth month. This group would include interstitial pregnancy and that in a rudimentary cornu of a uterus bicornis. Second, ovarian pregnancies. The pains here are absent or at most moderate. They often go to term. Third group, abdominal pregnancy (exclusive of placental insertion on the intestine). In the beginning there are often no morbid symptoms; later, the fetal movements are painfully felt. Different from the first two groups, the uterus is often recognizable with difficulty, owing to its dislocation and adhesion to the ovisac. At the end of pregnancy, *molimina ad partum*, death of the fetus. The symptoms of the fourth group, abdominal pregnancies with placental insertion on the intestine, are given above. Cases of this kind offer an unfavorable prognosis; but as regards that of the other groups, especially the first, F. concurs in Schroeder's view that it is not as bad as had been formerly believed.

F. does not think his experience sufficient to lay down general rules for the treatment. In concrete cases he would proceed as follows: In the first stage (first to third month) of all groups, killing the ovum by puncture of the ovisac followed by injection of morphine. In spontaneous rupture, therapeutic treatment of the threatening anemia and peritonitis. No operation. Beyond the third month, expectancy. Prescribing of absolute rest. Application of an elastic wadding pressure bandage to the abdomen; chiefly milk diet. After death of the fetus before it is viable, expectancy; in localized peritonitis, eventually opening,

evacuation, and drainage of the sac. In eliminative efforts of the living, viable fetus, laparotomy; after death of the fetus, expectancy; in local and general signs of reaction, especially symptoms of general infection, laparotomy. In performing this operation, the abdomen is opened by a long incision, the ovisac carefully stitched to the margins of the wound, then incised and if necessary a piece is excised; extraction of the fetus. The placenta is not detached. The ovisac is disinfected, dried, powdered with equal parts of salicylic and tannic acids, and the deepest part drained with a glass tube; finally the entire cavity is stuffed with disinfectant gauze or cotton. The expulsion of the placenta and membranes is left to nature. The powdering is repeated. The drainage tube is removed when the secretion ceases.

HEGAR (Freiburg) is of opinion that an atrophy of the musculature of the tube may lead to tubal pregnancy. He had observed a case which could be thus explained. The patient died. An operation would have been quite feasible, as the entire sac could have been extirpated. H. contradicted F.'s opinion that non-malignant tumors cannot be nourished by adhesions. He had observed a large fibroma which grew very rapidly. Cyst-formation, effusions of blood, etc., were not found. The adhesions contained arteries the size of the radial.

FRÄNKEL (Breslau) spoke a warning word, in suspected extra-uterine fetation not to supplement the bimanual examination by sounding. Not only the uterus, but also the ovisac can thereby be excited to contract and the latter caused to rupture. For the same reason, exploration with the needle appears to him fraught with danger.

His experience differed from that of F., inasmuch as he had observed a case of tubal pregnancy in the eighth month. He inquired if any of the members present had experimented with galvanism for killing the fetus, as had been recommended in America. He himself had tried currents of fifteen to twenty elements: once in a gravida in the fourth month. But abortion did not follow.

FREUND (Strassburg) thought that the case mentioned by Hegar, of growth of a myoma, could not be cited as against his views, because, according to H.'s statements, the tumor was not separated from its source, nor the uterus twisted. His remarks referred merely to such tumors the nutrition of which from their source was either quite stopped or was only minimal.

KALTENBACH (Giessen) confirmed Hegar's observation that more rapid growth of tumors may follow in consequence of the formation of adhesions. When symptoms of rupture of the ovisac have set in, he expects favorable results from laparotomy only when the operation is performed in the first few hours after the occurrence of the rupture. Later, peritonitis has generally set in, with incipient sepsis. A thorough disinfection of the abdominal cavity can then no longer be expected.

BADLEHNER had observed the regular occurrence of the menses in a patient castrated by himself. After two and a half years she became pregnant and went to term. From this fact he argued that there might be other carriers of germs than the ovaries.

HOFMEIER (Berlin) called to mind a case of completed tubal

pregnancy reported by Werth in the *Archiv f. Gynäk.* The danger of incising the ovisac when the fetus is living, in his opinion, lies not merely in the likelihood of encountering the placenta, but also in the possibility of profuse hemorrhages occurring from the ovisac.

SCHATZ (Rostock) holds examinations with the needle to be dangerous because, when no adhesions are present, liquor amnii may escape into the abdominal cavity. He inquired of Freund whether there were any points of predilection for implantation of the ovum on the peritoneum, to which the latter replied that the pouch of Douglas seems to be one.

KALTENBACH (Giessen) read a paper on

RUPTURES OF THE PUERPERAL UTERUS.

Spontaneous ruptures can occur only in consequence of serious impediments to parturition. In some cases there seems to be a certain predisposition of the tissues. Former lesions, as also the retention of tissue elements in fatty degeneration due to malinvolution, may act in this way.

In such cases the picture is different from those where there are impediments to labor and lead to rupture. The contractions are not violent; on the contrary, they are weak. Often the rupture is not discovered until after the escape of the child, or even during the removal of the placenta.

As to the treatment in impending rupture, it is usually recommended, if the head presents, to reduce it by operation. In transverse positions, however, many obstetricians perform version even when the child is dead, though great dangers attend that procedure in particular. Here, too, reducing operations are more appropriate. Should the head lie in the dilated portion below the contracting points, the neck can always be made accessible and decapitation performed. Should this not be the case, exenteration will have to be done.

After rupture has occurred, K. recommends laparotomy in cases of disproportion. He sees the greatest danger of rupture less in the hemorrhage than in the infection of the peritoneal cavity. The prognosis is most unfavorable, of course, when the uterine contents are beginning to decompose or are putrid. The idea of thoroughly cleansing and disinfecting the abdominal cavity and then to suture the rent is rational; but usually it fails because the cases come under treatment too late.

K. is of opinion that too great importance has been attached to drainage in the treatment and cure of ruptures of the uterus. Many of the cases thus far reported would perhaps have recovered without drainage; for former cases are on record where recovery ensued, though neither drainage nor disinfection was employed, and even the intestine was prolapsed. Here, possibly, the intra-abdominal pressure, driving the contained fluids downward and out of the wound, took the place of drainage.

A decidedly more favorable prognosis is presented by ruptures situated posteriorly, for in them the conditions for draining the

peritoneal cavity are advantageous. It is different with ruptures opening the vesico-uterine excavation. One case of spontaneous recovery is on record. But here, likewise, the occurrence of intestinal prolapse would lead us to conclude that the intra-abdominal pressure was great. For such cases K. advises laparotomy. He performed it once, introduced drainage tubes, and closed the abdominal cavity but partially. The patient remained free from fever until the fifth day; but died in consequence of secondary infection contracted on that day during the change of the tubes. When it is probable that septic matters have found ingress, laparotomy can be of use only when the decomposition has not progressed too far. Where the uterus is infected, Halbertsma's proposition to extirpate the entire organ might be looked upon with favor.

SIMPSON (Edinburgh) has observed cases in which friability of the tissues was really to be looked upon as the cause of the rupture. He sees one material difficulty as regards delivery in the resistance or inflexibility of the vertebral column. Judging from experiments made by him on the cadaver, his basilyst is applicable for the breaking up of that structure.

KALTENBACH (Giessen) wished to add to his previous remarks that exenteration is to be merely a preparatory operation, to be followed by extraction with the hook, version, or extraction simulating spontaneous development.

BADLEHNER mentioned a case in which rupture of the uterus with escape of the child recurred three times in the same woman. He coincided with Kaltenbach in the view that posterior ruptures are more liable to recover than the anterior ones.

BANDL (Vienna) stated that he had likewise had in view a disposition of the tissues, in a certain sense, as the cause of easy rupture. After difficult labors, the mechanically dilated cervix, as well as the vagina, may involute insufficiently and thus become again thinner in a subsequent labor. Finally, he had also noticed that former lacerations which have healed may be the cause of an early occurrence of rupture. As to the relations in a neglected shoulder presentation, he distinguishes two forms: *a.* Shoulder crowded through the internal os; head above it. Child firmly embraced by the greatly thickened body of the uterus. Cervix usually shorter rather than longer. To bring down the feet and turn the child is hardly possible. In these cases B. had never noticed the uterus to retract over the head. *b.* Head and shoulder below the internal os. In these instances B. assumes a primary head presentation; secondary occurrence of the shoulder presentation by a yielding of the head. Such are chiefly witnessed in multiparæ in whom the soft parts have remained abnormal. The external os, when dilated, soon stands rather high at the level of the pelvic inlet. Should the head give way from any cause, the originally abnormal cervix is rapidly dilated in an unnatural manner, unless a rupture occurs either suddenly or gradually even with the yielding of the head. As to treatment, B. favors drainage.

MUELLER (Berne).—Ruptures early in labor occur not only after previous deliveries with excessive cervical dilatation or with lesions, but also in primiparæ. M. had observed three such cases, in one of which he performed laparotomy and stitched up the

rent which was very difficult. The patient recovered. He inquired what should be done if hemorrhage sets in after the drainage tube has been inserted. He himself had removed the tube and tamponed.

SCHATZ (Rostock) thought that in explaining the occurrence of transverse or oblique positions, the form of the uterus is also to be considered. As to the treatment after rupture has occurred and how the labor should be terminated, he believed it was best to abstain from all interference. He advises merely to bring the patient into a half-sitting posture. Thus the outflow from the abdominal cavity is facilitated.

KALTENBACH (Giessen) holds the suture to be the best means for arresting hemorrhage after the abdominal cavity has been opened. He inserts two rows of sutures—one muscular and one peritoneal. As to the eventual extirpation of the ruptured uterus, he believes it to be not very difficult where the organ has been torn extensively.

HEGAR (Freiburg) called attention to the fact that Wiegand had formerly pointed out the distention of the lower uterine segment. He considers this to be the cause of the transverse position, and not the reverse.

(To be continued.)

ABSTRACTS.

1. Gonner (Basle): **Foot-measurements in new-born Children. A Contribution to the Treatment of Pelvic Presentations** (*Ztsch. f. Geb. u. Gyn.*, IX., 2).—The writer complains that most systematic authors in obstetrics say too little in regard to the difficulties which accompany extraction in breech presentations, whether these difficulties be primary, or secondary to turning. The importance of knowledge upon this subject can be appreciated when we remember its bearing upon prognosis in respect to the life of the child. We have methods sufficiently accurate for ascertaining the dimensions of the mother's pelvis, as those of the child's head, but nothing has heretofore been offered which would enable us to get a good general idea of the size of the fetus in utero. Ahlfeld's method with the instrument (Tasterzirkel) devised by him is inaccurate, and gives useless results. In order to obtain an approximate idea of the size of the child, we must get an accurate measurement of some part which is accessible, and this part must bear a tolerably constant relation to the size or proportions of the entire body. Such a part in breech presentations is the foot. In order to ascertain whether the length of the foot stood in any particular relation to the weight of the child, measurements were taken in one hundred consecutive births, during the year 1881, at the Basle obstetric clinic. The measurements were made immediately post partum, with a wooden instrument similar to that which is used as a measuring-stick by shoemakers, and in a similar manner. In a procedure which is apparently so simple, great pains are necessary, for the differences in the length of the feet of

new-born infants are slight. As a result of the author's labors in the cases mentioned, the conclusion reached was that for a length of foot of eight centimetres, a total weight of three thousand grams might be expected as a rule; also that a length exceeding eight centimetres made it probable that the child's weight exceeded that of the average mature fetus, and that extraordinary difficulties in extraction might be expected. By considering, in addition, the measurements of the maternal pelvis, data are afforded, says the author, which will enable us to prognosticate the birth of a living child, or the necessity for a destructive operation. It must be borne in mind always that these calculations have no reference to hydrocephalic children, and those who are the subjects of other deformities. A foot which measures seven and six-tenths centimetres in length presupposes a child of moderate size; a length of less than seven and three-tenth centimetres presupposes an immature fetus. In the tables which are given by the author, a tolerably constant difference in weight is observed, between male and female children, in cases in which the feet of both sexes are of the same length, the females being the heavier, but this is not considered of any especial importance in an obstetric point of view. The occupation of the parents, also, has something to do with the size of the hands and feet of the offspring, and as the measurements in the tables referred to were taken upon children in the poorer, or working classes, the relative weights are, probably, larger than would obtain among the children of those who are not habitually engaged in severe manual labor. Race and national peculiarities may also require consideration, and it is to be hoped that the subject will be worked in other fields, for the perfection of the desired statistics. A. F. C.

2. Liebermeister (Tübingen): Concerning Hysteria and its Treatment (*Volkmann's Sammlung*, No. 236).—The results of the treatment of severe cases of hysteria in the Tübingen clinic have been very favorable. They are largely due to the almost faultless management, in the author's opinion, of the hospital there. In answer to the question: "What is hysteria?" the author replies: "Hysteria is a psychical disease," the associated disturbances in other portions of the nervous system being of a secondary nature. In comparison with diseases of the mind, purely, hysteria concerns mainly disturbances of the lower psychical functions; the others, those of the higher psychical functions, that is, in relation to their nearness to or distance from conscience. The author then makes an excursion into the domain of practical psychology, for the purpose, as he says, of extending his definition. The higher human functions pertain to the faculty of intelligence; the lower ones, to the faculties which, collectively, are known as instinct. With most hysterical patients, the disturbance is chiefly in the region of the feelings, all sorts of pains being experienced without corresponding local lesions. The hyperæsthesiæ, paresthesiæ, and idiosyncrasies must all be referred to a disturbance of the central organ. Among the hysterical paralyses, the functional ones are especially common, and only those muscles are paralyzed which perform a particular function, neighboring ones, even though supplied by the same nerve, being unaffected. More curious still, this paralysis of certain muscles limits itself to certain conditions, *e. g.*, muscles of the leg may show sufficient contractibility in a bed-ridden hysterical patient, while the patient is in bed, but will refuse to do their

work, if the patient is required to stand or walk. Undoubtedly, certain forms of hysteria develop into positive mental disease. The etiology of hysteria also indicates that it is of psychical origin; witness, as factors in such cases, the original tendency of the mind, the influence of education, the result of depressing forces, and the propensity for imitation. As other etiological factors, the feelings, moods, and impulses may be referred to, also centripetal irritation on the part of the sexual organs, the intestinal canal, and other internal organs. The treatment must be, to a great extent, psychical, but this does not imply a disregard of other means, especially when there is evident disease of any of the organs or parts. The etymology of the word hysteria (*ὑστέρα*, the uterus), of course, implies disease of a particular organ, but it is often the case that no disease of the uterus can be made out. On the other hand, hysteria may be cured, and yet disease of the uterus or the other genital organs remain. Castration, and removal of the clitoris, are quite disapproved of by the author, when they are to be performed in order to cure hysteria; and rather faint praise is given to other gynecological treatment for this purpose. As the general condition of the patient improves, the hysteria is likely to improve also. Change of residence, certain water cures, baths, exercise, etc., are recommended, and directions are given which are suited to particular types of the disease. Among psychical instrumentalities, the author recommends the provocation of joy and hope. Hypnotism, which is advocated by some, is calculated, in the author's opinion, rather to excite than to cure hysteria. Inherited tendency, and education, are always to be regarded in a consideration of the prophylaxis of the disease, and if self-control and a sense of responsibility in regard to duties to be performed can be inculcated, the result will be a satisfactory one. The more striking phenomena will call for special treatment. If the attacks take the form of spasms, ecstasy, or somnambulism, consciousness will be only partially abolished, and in this fact will lie the differentiation from actual disease, though hysteria and the disease which it simulates may co-exist. For the treatment of such attacks, cold water poured over the surface of the body is recommended. The induction current is also a useful instrumentality. Aborting a first attack may result in cutting short the disease, or, at least, in delaying it. Metallo-therapy has been tried by the author in the treatment of hysterical anesthesia, but its effect, if any, is thought to be only upon the imagination. One of the main elements in successful treatment consists in gaining the entire confidence of the patient, and another in causing her to abandon the habit of brooding over herself and her condition. Medication is usually superfluous.

A. F. C.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

GLIOMA OF PONS VAROLII AND MEDULLA OBLONGATA OCCURRING IN A CHILD OF EIGHT YEARS.

BY
G. H. WHITCOMB, M.D.,
Greenwich, N. Y.

THE following very unusual case occurred in my practice, and is worthy of record.

Henry S., aged eight years, son of a well-to-do farmer, of light complexion, spare build, with large head and full nervous temperament, was brought to my office in July last. Family history unexceptionable on father's side, but mother's father's people have insanity among them.

Previous health good. Never had any serious sickness, nor symptoms of nervous disorder.

Parents stated that during the latter part of the preceding May the left eye became crossed, without evident reason, or other symptoms of any kind. Health and spirits usually good; was ahead of his classmates at schools, and studied industriously at all times; to which fact his parents attributed the trouble.

Examination revealed internal strabismus of left eye, from a partial paralysis of the external rectus. Vision very poor for short, and not very good for long distances, owing seemingly to paralysis of accommodation. Right eye good. Pupils equal and normal, ophthalmoscope showed a possible hyperemia of retina; no other symptoms of any character could be elicited.

Advised parents to consult an oculist which they failed to do.

Saw case again September 6th and found strabismus in *statu quo*, with the addition of difficult speech and deglutition, together with a weakness of all the extremities. On examination found complete symmetrical paralysis of soft palate, with absolute anesthesia; partial paralysis of vocal cords, tongue and lips; voice low and indistinct; respiration entirely oral. Face slightly drawn to right when laughing or crying. Was very emotional. Extremities of

left side most paralyzed; arms less so than legs; tongue clean, pulse and temperature normal; no headache, vertigo, spinal tenderness, pain, hyperesthesia nor anesthesia, except of soft palate, which was so anesthetic that no reflex could be excited.

At this time, after careful study of the case and finding no organic lesion that might produce any such *ensemble* of symptoms mentioned by authors, as occurring in either adult or child life; and reading frequently in various medical periodicals of diverse forms of reflex paralysis from nervous irritation, my veneration for authority constrained a diagnosis of reflex trouble. This was made with many misgivings and soon abandoned.

The irritations to which the trouble might be referred on this hypothesis, were a small abscess above right lateral incisor which contained the root of a deciduous tooth, two decayed teeth, and a long, tight but not adherent or tender prepuce.

Sept. 8th.—Saw patient again. All symptoms were slightly worse. Parents said that child fell in the yard while at play, and was carried to the house, but did not lose consciousness. Nothing else new was reported.

On the evening of the 10th was hastily summoned to see patient at his home, and found him with high fever, rapid pulse, and the usual indications of gastro-intestinal disturbance. A sudden increase of paralysis was feared; but found patient much better next morning, with no other harm than a slight elevation of temperature and pulse, which was 94. Tongue covered with thin white fur. About this time salivation occurred, but was not profuse.

This condition continued, with child growing weaker, and paralysis gradually increasing in degree. Patient chokes while eating, and regurgitates fluids through nostrils. Bowels and bladder act sluggishly, several attempts at evacuation being necessary before succeeding. Sleeps well, intelligence unimpaired, laughs heartily, but speaks mostly in a very indistinct tone; occasionally articulates quite well, but then only such words as no, oh, etc.

Sept. 15th.—Patient could not stand nor help himself; was able to draw up legs, but could not put left over right. About the same relation was sustained by the arms, though not as weak.

Cutaneous sensibility normal throughout. Tendon reflexes exaggerated. Electro-muscular reaction very slight to an eighteen cell galvanic, and only good to a strong faradic current.

Sept. 20th.—Has attacks of rapid, harsh, irregular breathing, with rapid and irregular pulse.

Temperature 98½; face flushed; chokes occasionally with saliva when lying on back. Can close mouth, but cannot purse lips for whistling.

Patient can articulate nothing, but seems in good spirits most of the time.

After excluding glosso-labio-laryngeal paralysis, a diagnosis of disease of the medulla oblongata was made as the only safe one, though the extent of nervous involvement implied a more extensive lesion.

A prognosis of an early death from involvement of the respiratory centre, and paralysis of the pneumogastrics was rendered.

From this time respiratory trouble increased and became constant five or six days before dissolution. Pulse varied from normal to 120, but did not intermit. Temperature slightly increased, though not taken.

The double hemiplegia never involved sensation to any great extent and did not completely abolish motion.

The respiratory muscles acted well.

During the last week, the pupils were slightly dilated, the left most, yet reacted to light. They were noticed to oscillate the day before death.

The morning of dissolution, Oct. 10th, respiration became intensely labored, and strabismus disappeared.

The breathing as described by the mother resembled that known as the Cheyne-Stokes respiration.

No rheumatism or other disease pre-existed, or complicated the case. The child had no pains, formication, spasm, inco-ordination, subsultus, tremor, nausea, vomiting, facial distortion or loss of special senses to the last, so far as could be learned.

Duration of illness from beginning of strabismus, three and one half months; from outset labio-glosso-laryngeal symptoms, about one month and ten days.

Autopsy, 48 hours after death, by Dr.H. Gray and myself.

Cadaver very much emaciated. Rigor mortis moderate. Brain only examined.

Calvarium removed and brain taken out. Dura mater normally adherent, and of healthy appearance. Sinuses and arteries were engorged with dark fluid blood. Membranes throughout presented nothing abnormal, except the engorged condition of the blood-vessels, which was due directly to the manner of death. No hemorrhage, embolism, thrombus, or organic vascular change was found.

Cerebrum and cerebellum presented nothing worthy of note, except that the ventricles contained an unusual amount of serum.

Pons Varolii and medulla oblongata were very soft in their interior. They were removed entire, and placed in strong alcohol to harden sufficiently to allow more careful dissection.

Alcohol changed daily for two or three days, when a longitudinal section was made in the median line.

In central portion of pons, just above its union with medulla, was found a softened and broken down *foyer* which involved the right side to a greater extent than the left. It was semi-fluid or creamy in consistence, of an ash white color, with its surface somewhat depressed. A pink zone surrounded the softened portion, and extended throughout central portion of medulla, which was very soft, but not broken down anywhere.

One of the lateral portions was divided by cross section into three parts, and the whole replaced in alcohol for further hardening.

The softened centre of pons became shrunken and fissured as the water was abstracted.

Sections from cord immediately below medulla oblongata, from crura cerebri, and from outer surface of pons and medulla, were in healthy condition. The disease is then restricted to the pons Varolii and medulla oblongata.

Microscopic examination of the diseased portions kindly made by Dr. W. H. Welch, of New York, showed the structure of glioma. Dr. Welch writes, "I have examined the microscopic slides which you kindly referred to me, and find that they present the structure of glioma. . . . There are many cells of various sizes and shapes imbedded in a finely granular and fibrillated matrix resembling neuroglia. Some of the cells are 'spider cells.' Large and small cells are abundant, and in some places fusiform cells are seen. Gliomata sometimes resemble closely in their gross appearances necrotic softening of the brain, and it may take a microscopic examination to distinguish between the two."

Without attempting to discuss the pathology to any extent, I shall briefly recall the salient features of the case.

Glioma is the term applied by Virchow to morbid processes or tumors occurring in the brain, and consisting of a proliferation of the neuroglia. Their etiology is obscure, though traumatism is assigned in many instances. Their close resemblance to sarcoma, in some instances, suggests hereditary influences. I could not learn that my patient ever suffered any injury, nor was I able to establish any inherited taint. There seems but one thing that may reasonably be presumed to have exercised a causative relation, and that is an excessive mental application. Friends and neighbors vied in lauding the child's mental capacity and constant industry. He was intelligent beyond his years, and the praise received seemed well merited. The rapid development of fatal symptoms, three and one-half months, is not in harmony with other cases of glioma, but this discrepancy is no doubt due to the vital parts involved.

The gradual and successive implication of the cranial nerves is very interesting, and plainly suggests the seat of origin and subsequent spread of the morbid process. In fact, it amounted to an automatic physiological experiment. That is to say, it began at the nucleus of the sixth and travelled backwards, or down, gradually involving the seventh, ninth, tenth, eleventh and twelfth nerves. The growth in the pons was probably an extension, and did not involve the medulla posteriorly above the origin of the sixth nerve. All the nerves implicated have their

deep origin in the medulla oblongata. Only two escaped, the fifth and eighth. Perhaps these were not wholly unaffected, for my scrutiny on this point was not searching.

The paralysis of accommodation without dilatation of pupil is unusual, though it occasionally occurs. The paralysis may be explained by the fact that the abducens gives a branch to the cavernous plexus of the sympathetic, which in its turn sends one to the ophthalmic ganglion, which supplies the ciliary muscle. The paralysis was therefore "sympathetic" or round about and coincident with that of the nerve from which it was derived.

The facial originates from the same nucleus as the abducens, yet for some reason was not involved for over two months, and then never completely, since its paralysis was not complete, either in degree or extent, for the orbicularis oculi and corrugator supercilii were not involved, and the temporal incompletely so.

The glosso-pharyngeal was the only nerve that lost both its motor (by anastomosis with facial and pneumogastric) and sensory function.

The vagus, together with the accessory portion of spinal accessory, which becomes anatomically a portion of itself, were implicated coincidently with the facial and glosso-pharyngeal, and suffered first in the branches to larynx, pharynx, etc., next in its gastro-intestinal distribution, which accounted for gastric symptoms; and, finally, the destruction of the respiratory centre finished life. The spinal portion of the spinal accessory failed to perform its duties about one week before death.

Hypoglossus lost its function about same time as facial and vagus.

The absence of headache, pain, spasm, vomiting, and a more extensive involvement of sensation, together with some other symptoms already mentioned, are worthy of more elaborate consideration than the writer can give.

The double hemiplegia was due, of course, to the bilateral lesion. The greater degree of paralysis of left side, corresponded with a more extensive destruction of nerve-fibres on right side of pons Varolii.

TWO FATAL CASES OF DIABETES MELLITUS IN CHILDREN.

BY
LEONARD WEBER, M.D.,
New York.

DIABETES MELLITUS is not often met with in infantile life. The following cases, interesting also from an etiological point of view, deserve to be placed on record.

Louisa W., seven years old, comparatively well developed, with no hereditary taint, as far as could be ascertained, but of nervous temperament, was ordered to take of a mixture containing 3 i. of bromide of potassium to aqu. \bar{z} vij., a tablespoonful every two hours. Through some mistake, \bar{z} i. of the salt, instead of 3 i., was put in the above quantity of water, and the child took the whole of it, in the course of two days.

She got over the acute symptoms of bromism, but, about a month later, her parents noticed that she was unusually thirsty and passed large quantities of urine.

When I saw the patient first, three months after the overdose of bromide of potassium, she presented all the clinical symptoms of diabetes mellitus, the urine showing a specific gravity of 1.030, and containing sugar. She voided about a gallon per day. In the course of the next three months, she grew quite thin and pale, the diuresis increased to six quarts daily, and albumen began to appear in the urine, its specific gravity remaining about the same. Sugar and albumen continued to be present together, up to the time of the patient's death, which occurred twelve months after the poisoning.

Autopsy: Body emaciated; abdomen moderately tympanitic.

Thorax: In right pleural cavity, about two ounces of bloody serum, no adhesions; right lung normal, except some hypostasis in lower lobe, posteriorly; some serous exudation in left pleural cavity, adhesions between pulmonary and diaphragmal pleura and upper and lower lobes; edema of pulmonary tissue; two enlarged and calcified bronchial glands.

Heart: Left ventricle hypertrophied; fibrinous clot in right chamber; valves normal.

Abdomen: Liver almost double the normal size; its peritoneal surface smooth, yellowish-red, and moderately injected; parenchyma in a state of fatty degeneration. Ileum tympanitic, hyperemic; colon normal. Bladder large; its walls thin.

Left kidney a little larger than normal, with hard cicatricial retractions in its upper and lower circumference; the cortical substance of yellowish color, and in several places greatly reduced in diameter. The pelvis considerably widened, with cystoid enlargement of its upper portions. Right kidney reduced to the size of a large chestnut, with many cicatricial retractions. There is but

a very thin layer of cortical substance left, and that in an advanced state of degeneration.

Clara P., fourteen years old, an intelligent and physically well-developed girl, with no hereditary taint, had scarlatina simplex when five years old. She was again taken with the disease when thirteen and one-half years old. The case was well marked, accompanied by high fever, but otherwise uncomplicated, until the third week, when she had a mild attack of nephritis, which yielded to diaphoretic treatment. Two months later, I was informed that Clara was not doing well. I found her afflicted with a grave form of diabetes mellitus. Her urine had a specific gravity from 1.035 to 1.038, and was passed in abundant quantities. The disease ran a very acute course, unaffected by any treatment; the patient became rapidly emaciated, developed the clinical signs of phthisis pulmonalis later on, and died completely exhausted, six months after the attack of scarlatina.

CORRESPONDENCE.

A POINT IN PROGNOSIS.

TO THE EDITOR:

THERE is a phenomenon presenting itself in some diseases of children, which seems to me to be of more importance in connection with prognosis than generally known.

In exhaustive diseases, such as diarrhea, typhoid fever, and others, after having for days persistently refused nourishment, the child suddenly swallows with avidity whatever is offered, food or medicine indiscriminately. Even quinine will be taken as readily as sugar. Such an occurrence is generally hailed with delight by the interested bystanders, but in reality it is a very untoward symptom. In my experience it frequently warrants an unfavorable prognosis.

An explanation of this sudden change may perhaps be found in the cessation of cerebral function, through the want of nutrition or of stimulation.

Combined with this behavior is often found the Cheyne-Stokes breathing, and this coincidence goes far to support the above explanation, as this respiratory disorder has been traced also to the want of stimulation of the respiratory centres.

Having been impressed with the above observation, and finding no mention of such in the books, is my excuse for these few lines.

Very respectfully,

B. E. HADRA, M.D.

SAN ANTONIO, TEXAS.

TRANSACTIONS OF THE WASHINGTON OBSTETRICAL AND GYNECOLOGICAL SOCIETY.

Stated meeting, October 20th, 1883.

DR. S. C. BUSEY, *President, in the chair.*

DR. THOMAS E. MCARDLE read a paper on

CHOLERA INFANTUM.

Referring to the reports of the health officer of Washington, he found that 168 deaths from cholera infantum had been reported between May 1st and October 1st, 1883, and hoped that the discussion would bring out such facts as would enable the profession to treat this disease with more success or point out prophylactic means to be resorted to to prevent its development. He gave the various theories relating to the causes of the disease, and gives his preference for that which attributes the disease to the disturbance of the nervous system occasioned by sudden and excessive rise of temperature, rather than to the persistence of hot weather for a lengthy period. He illustrated his views by reference to the fact that persons passing from a cold to a hot climate are more susceptible to diarrheal disease than those who have lived in the warm countries throughout the year. In addition to the cause mentioned, it was only necessary to call in the aid of improper diet, and the disease was likely to make its appearance. Dentition was probably the most active predisposing cause. Dr. McARDLE briefly recapitulated the symptoms and pathology of the disease, and in the treatment gave his adhesion to the bismuth and anodyne method, with stimulants, especially Cognac brandy in cold water. Cold baths might be resorted to in suitable cases. The importance of suitable dieting, and of hygienic measures was insisted upon. The necessity of sending children to salubrious localities in the country, when practicable, or on short excursions by land or water, was emphasized.

DR. S. S. ADAMS said that, in opening the discussion, he was confronted by a diversity of opinion as to cholera infantum. The disease has always been confounded with other diarrheal diseases,

while it was entitled to a distinct nosological position. It is the analogue of cholera morbus in the adult. At one time it was believed to be purely an American disease, but now it is recognized in Europe as a distinct type. Mortality statistics are incorrect because physicians fail to make the diagnosis between it and other diarrheal diseases, and reference to mortality reports sustains the assertion. He did not think we were as familiar with the symptoms of the disease as Dr. McArdle claims we are, and thought if these were better known there would be fewer cases reported under the name cholera infantum. He pictured the disease as it most frequently presents itself, and declared the termination to be generally fatal. In the treatment he would particularly urge the early and free use of stimulants.

DR. SMITH said he had not seen a single case of cholera infantum during the past season, and inquired of others what their experience had been in that respect. Many cases terminate in enterocolitis, and if death occurs it might be proper to give cholera infantum as the primary cause of death, although enterocolitis might be its immediate cause. This would help to explain the apparent errors in mortality reports.

At the following meeting, the discussion was resumed on Dr. McArdle's paper on cholera infantum.

DR. W. W. JOHNSTON, in re-opening the discussion, said he had a few words to say as to nomenclature, and as to the position which cholera infantum, and inflammatory diseases of the intestine in general, should occupy in nosology. By referring to mortality statistics, deaths were found recorded from diarrhea, enterocolitis, enteritis, intestinal catarrh and ulceration of intestines, as from distinct diseases. All of these terms are included under, or represent different stages of one pathological state—intestinal catarrh, and the subdivision of slightly varying forms, originating in one morbid process, into separate diseases is unreasonable and destroys all the value of statistical data. It depends entirely upon the bias or fancy of the physician who makes the return, as to whether he shall give as a cause of death enteritis, intestinal catarrh, ulceration of the bowels, or diarrhea. The symptoms in the case in question might justify any one of these names, as we understand their meaning from descriptions in our text-books. The books persist in propagating confused and erroneous ideas on this subject. In Reynolds' System of Medicine, the description of diarrhea and catarrhal enteritis is essentially the same. Diarrhea has enteritis for its lesion; enteritis has diarrhea for its symptom. It is only conservatism which dissociates the symptom and the lesion. The books, some of them, continue to speak of "serous enteritis," which is peritonitis and not enteritis proper; of enteritis involving all the coats, which is also essentially peritonitis, or which, from the symptoms given, is no doubt only colic, in many instances. Enteritis primarily involving the muscular coat has no existence. We are thus thrown back upon the mucous lining as being the only membrane of the bowel which has a distinct inflammation and this is catarrhal, and the disease properly called "*intestinal catarrh*." *Enteritis*, *enterocolitis*, and *diarrhea*, are one and the same with *intestinal catarrh*; *ulceration* is an advanced stage of the same process. It is irrational to differentiate the *symptom* diarrhea into "*diarrhea crapulosa*, *mucosa*, *biliosa*," etc., these being only modifications of the symptoms due to difference in the localization

or in the intensity of the catarrhal process. Some held that diarrhea could exist without catarrh, but he did not think we could say so. There was, first, hyperemia, and second, hypersecretion, and these were only stages of catarrh. True, it might not pass through all the stages, but when there was hyperemia and swelling of the mucous layer, there was catarrh. Hence, we could not call diarrhea a disease. It bore the same relation to intestinal catarrh that the mucous or other discharge from the air passages bore to bronchitis; and surely no one would think of dividing bronchitis in that way; we only spoke of bronchitis according to the pathological condition causing it. The intestinal canal was very long, and the products of different glands were discharged into it which modified the character of the alvine evacuations. Thus, if the catarrh was high up, say in the duodenum, there was excess of bile; in the ileum there were the ordinary stools of diarrhea with undigested food; while lower in the colon, we had admixture of mucus, and still lower down of blood, but they all represented catarrh, differing only in locality. In our classification we should make our ideas as simply as they could be made, by referring everything to the pathological basis underlying the symptoms.

DR. KING agreed that we should make our pathology as simple as possible, but while our patients were alive, we were obliged to classify by symptoms, inasmuch as at that time we could not examine the pathological condition.

DR. JOHNSTON.—Our effort should be to locate the disease, judging of the locality by the character of the discharges. This was the method adopted by Nothnagel, and the only correct one.

DR. HAGNER.—We use the terms, criticised by Dr. Johnston, very often not so much for the purpose of defining the pathological condition as for the sake of convenience. We know that bronchitis means catarrh of the respiratory passages, but we speak of mucous, purulent, muco-purulent bronchitis, etc., and a physician would associate certain conditions with these terms; thus the purulent discharge would suggest catarrh of long standing, but if we simply say bronchitis, we give no clue as to how far the case had advanced. The use of these terms facilitates speaking of the cases, although we do not mean to use them as defining pathological conditions.

DR. MAGRUDER said this looseness of expression had crept in because the public wanted a name for the disease, and physicians, unfortunately, had fallen into the habit of catering to them. Health boards could not remedy this error, because they were obliged to accept the certificate of a registered physician. He thought the colleges ought to exert an influence to correct the evil. Many cases were called cholera infantum through carelessness in diagnosis. He had seen many cases that some might call by that name, but very few of true cholera infantum. The name "intestinal catarrh" might be adopted to include the cases grouped by Dr. Johnston. He could generally trace the cause of intestinal ailments in infants to error in food, hygiene, and surroundings. If these cases were due to germs, we should find more patients in the same house, but the trouble never spread unless the children had been exposed to the same causative conditions. In the treatment of profuse diarrheal discharges, he had successfully used *ext. pancreatis* with bicarbonate of soda, or soda mint.

DR. ACKER added his testimony to the infrequency of cholera infantum in this city. A pathological basis for diagnosis and no-

menclature should be insisted upon. Most cases could be controlled by lime-water and bismuth.

DR. MAGRUDER agreed to the efficacy of bismuth and lime-water. In dispensary practice he had used a mixture of chalk-mixture, bismuth, and chlorate of potassium, with success. He rarely used astringents. More care should be observed in the use of opiates. Thus we found patients in a stupor, due perhaps to cerebral anemia, the so-called spurious hydrocephalus, and if opiates had been given, we could not say whether the brain symptoms were due to opium or to the disease.

DR. HAGNER would refer to the use of opium, spoken of by Dr. Magruder. It was the very thing in anæmia of the brain. The coma from the disease and that from opium ought not to be mistaken for each other by any careful observer. It was also beneficial in relieving the tenesmus due to dysentery. It acted better than any other agent when given by injection, but was more serviceable when the lower bowel was affected than in cases where the small intestine was the seat of disease.

DR. KING said that all agreed that cholera infantum is a summer disease, and it was necessary to determine what element existing at that time called it forth. Was it heat? If so, how did it act? Theoretically this had been explained in many ways. The child lost water by the skin and might there not be in consequence irritation from acridity of partially inspissated secretions? And this suggested the utility of water in the treatment of the disease. Again, if the woman was nursing her child, she at the same time losing water by reason of the high temperature, would not her milk contain less of water and more of the solids, and would not such milk be difficult of digestion? Dr. King had for years advocated the theory that disease was designed to preserve life, and in accordance with this would say that the intestinal symptoms in this complaint pointed to a condition in the nervous system (congestion, etc.), and that the intestinal organs represented skirmishers of the organism, at the periphery, whose action tended to preserve the citadel. Instead of the bowels, we should address our investigation to the nerve centres. As to drinking water as an element in summer diseases, he would point to the possible influence of the mosquito. He had often seen a mother go to a vessel to get water for her child, and previous to dipping up the water from the bucket, brush away the mosquitoes from the surface. These insects, infecting our houses, are all females; they deposited from 250 to 300 eggs in the water; and which in the digestive organs of a feeble infant might develop into larvæ and so cause intestinal disease. Means should be adopted to prevent the mosquito from depositing her eggs in drinking water. One more point he thought worth mentioning. In England he had seen the disgusting practice of the mother first chewing the food and then giving it to her infant. Repulsive as it appeared, it still prepared the food for digestion. It might be advisable in some cases to advise the practice, or pure saliva might be placed in the child's mouth. Above all, however, he thought that the treatment by an abundant supply of water was the best means of relief, and that the effect of drugs was more or less a matter of chance.

DR. HAGNER agreed with Dr. King in the use of water in these cases. Early in his practice Dr. K. had impressed him with the importance of this practice and results had not caused him to regret its adoption.

DR. MAGRUDER was aware of the physiological action of opium

referred to by Dr. Hagner, and only desired to impress upon practitioners the necessity of carefulness in its use.

ABSTRACTS.

Prepared by J. FEWSMITH, JR., Newark, N. J.

1. Silbermann: Pavor Nocturnus of Children (*Jahrbch. f. Kinderheilkde.*, XX. B., 3 H.).--A rapid glance through the literature of children's diseases shows that though the earliest writers have described the "night terrors" of early years, yet no definite conclusions have been reached as to the etiology of the trouble, the views varying from the opinion of those who believe it to be a disease of the brain, with genuine pathological changes, to the idea that it is simply and always due to a disturbance of digestion. Dr. Oscar Silbermann, taking hold of the subject in his analytical manner, says the name, Pavor Nocturnus, is perhaps the best which could be found, but in defining it, it is necessary first to separate between an idiopathic and a symptomatic pavor. By idiopathic pavor nocturnus, we understand a disease *sui generis*, originating primarily in the brain, and running its course in the central organ of imagination (cortical substance). The symptomatic pavor nocturnus is a reflex neurosis characterized by dyspnea and anxious crying, and especially caused by irritation of the vagus. The first is much the more frequent, attacks children between 2 and 6 years, and especially those who are physically delicate and mentally bright. After supper the child goes quietly to bed and to sleep; rests quietly two or three hours, and then suddenly wakes with a frightened cry, sits up in bed with wide open eyes, wrings the hands or wards something off with them, and utters short sentences of fear--such as "the dog!" "the black dog bites!" "the black man is coming!"--and the face gets red and covered with perspiration. The pupils are sometimes dilated and sometimes contracted and fixed on some one point, respiration and pulse a little hurried, temperature normal. The child is unconscious, does not know any one or hear the quieting voices. After 10 to 20 minutes he begins to cry, recognizes his friends, generally asks for a light, and then goes quietly to sleep again, and in the morning has no recollection of what has occurred. No cause for this can be found in any organ. It is rather to be sought in the sphere of the child's thoughts during the day. The pathological conditions of dread may depend on a certain object or be entirely without object. There are, therefore, two kinds of dread. Objective fear, caused by reproduction in the mind of the individual of objects of the outer world, and subjective fear, which is entirely independent of the outer world and objectless. There is no doubt that in idiopathic pavor nocturnus we always have to do with objective fear. There is always a picture before the child of something which has taken place in its sphere of thought in the outer world. This is the causal moment. Another point of interest is that there is no recollection of the attack. There is, therefore, first an increased excitability of the brain (cortical portion), and second, a defect of memory (amnesia).

The symptomatic pavor nocturnus differs from this. It may occur in strong, hearty children. The child after supper sleeps uneasily, sometimes cries, grits its teeth, breathes either slowly or very fast, and after two or three hours' sleep wakes up always with dyspnea, wrings its hands and says, ah, ah, oh, oh, and interjections of pain. Then it generally wakes from the attack, which never lasts as long as in the other form. The pulse is apt to be very irregular. After a time the child sleeps quietly, and on the next day has no recollection of the occurrence. In almost every case of this kind we are able to find as a cause an error of diet, generally shown by unmistakable symptoms. In analyzing the attack, we see that it comes on slowly, that is, gives premonitory signs, that it lasts a short time—two to six minutes, while the idiopathic form lasts ten to forty minutes; that the children never utter exclamations denoting fear of an external object or gaze fixedly as if in dread. It is evident that the fear is subjective. There is little doubt that it is occasioned by the dyspnea, and as little that this is occasioned by reflex irritation of the fibres of the vagus, sometimes the respiration being hastened and sometimes slowed, as the different fibres become irritated. This also explains the irregularity of the pulse.

The following conclusions may therefore be drawn :

1. A strict separation must be made between the idiopathic and symptomatic pavor nocturnus.
2. The dread or fear, common to both, is, in the idiopathic form, objective; in the symptomatic form, subjective.
3. Idiopathic pavor is a disease sui generis, consisting of a transitory hallucination as the expression of objective fear, which depends on an abnormal excitability of the brain (cortical portion), and is always accompanied by defect of memory (amnesia).
4. Symptomatic pavor nocturnus is a reflex neurosis from the gastric fibres of the vagus through the pulmonary terminal fibres, consisting of dyspnea and the expressions of subjective fear caused by this. There is amnesia in this case also.

2. Biedert: The Development of Infants on the Minimum Amount of Nourishment (*Jahrbch. f. Kindhklde.*, XIX. B., 3 H.).—A large part of this number of the *Jahrbuch* is taken up with a very exhaustive and interesting article entitled "Weight Studies." The first part treats of the conclusions reached by the systematic weighing of infants; the second is devoted to the important and practical point of determining exactly how much food a child should have. Important as this point is, we have but few reliable data upon it, and some of the directions given in the popular works, written by physicians (unfortunately) for the laity, are simply horrible. Biedert mentions one which directed for a new-born infant 600 gm. milk with twice the quantity of water daily. One-third of this is more than enough. Criticisms have been made upon Biedert's former articles on this subject, but his present tables of weight and carefully conducted experiments seem to establish his points beyond doubt. He reckons for the first month 2.1–2.6 gm. albumen, and for the second month 3.0 gm. albumen for each kilo (2 lbs.) of weight. That makes, reckoning a child in the first month at 6–7½ lbs., and in the second at 8 lbs., from 6.5–9.0 gm. albumen in the first, and about 12.5 gm. albumen in the second month; or reckoning the amount of albumen in cow's milk at 4–4.5%, not more than 160–200 gm. cow's milk for the first

month, and about 300 gm. for the second. A list of cases given shows an average daily gain in weight of the child of 24 gm. in the first month, and 31 gm. in the second. In the third month the average amount of albumen is 3.7 gm. (2.8-4.2 gm.) per kilo body weight. In the seventh month about 4.2-4.6 gm. per kilo is sufficient. Biedert's results certainly prove him right on these points.

The second important point is that by the addition of more fat than is contained in the ordinary milk mixture, we can spare albumen and add to the nutrition of the child. In this position, Biedert has also met with much opposition, but has again demonstrated the correctness of his opinions. By very careful controlling analyses of the feces, he finds that the child can absorb much more fat than is generally supposed; for instance, in the first month twenty-one grammes more than the ordinary, above measured, diluted milk contains. He has shown in other works that at certain ages this increased amount of fat is of the greatest importance. Now, following out his schedule for the minimum amount of albumen, he makes a practical schedule also for the amount of fat and the combination of the two. He first gives the mixtures as formed from his well-known "artificial cream conserve," then the natural cream mixtures (*Rahmgemenge*), also well known, and then the diluted cow's milk. All the experiments made to test these forms of nourishment have been controlled, not only by weighing, but by systematic analysis of the feces.

To understand what follows, we must remember that the artificial cream mixture consists of 1 (table) spoonful conserve with 13 spoonfuls water. This contains 1% albumen and 2% fat, etc., and is called mixture I. One spoonful (always understand tablespoon) milk added makes mixture II., with 1.24% albumen and 2.12% fat. Two spoonfuls milk, No. III., with 1.43% alb. and 2.22% fat. Three spoonfuls milk, No. IV., with 1.6% alb. and 2.32% fat. Four spoonfuls milk, No. V., with 1.78% alb. and 2.4% fat. Five spoonfuls milk, No. VI., 1.92% alb. and 2.47% fat. Six spoonfuls milk, No. VII., 2.05% alb. and 2.5% fat. Seven spoonfuls milk, No. VIII., 2.17% alb. and 2.57% fat. Eight spoonfuls milk, No. IX., 2.26% alb. and 2.65% fat. Nine spoonfuls milk, No. X., 2.37% alb. and 2.7% fat. Ten spoonfuls milk, No. XI., 2.46% alb. and 2.8% fat. Eleven spoonfuls milk, No. XII., 2.54% alb. and 2.8% fat. Twelve spoonfuls milk, No. XIII., 2.6% alb. and 2.83% fat. Thirteen spoonfuls milk, No. XIV., 2.68% alb. and 2.9% fat. The milk is reckoned as containing 4.5% albumen and 3.5%-3.8% fat. In all the mixtures, the sugar represents about 4%. Enough was given the children to correspond with the amount of albumen needed, according to the preceding schedule, at a certain age. In the first month, therefore, beginning with mixture I., we should give from 175-200 gms., according to the child's weight, advancing rapidly to the other mixtures. For simplicity, we may reckon on 200 gms. pro kilo weight. In the first half of the first month, then, we would give 200 gms. of Nos. I. and II., containing 2.0-2.5 gm. albumen; in the second half, the same amount of Nos. II. and III., containing 2.5-2.8 gm. albumen pro kilo weight. Thus we advance one mixture each half month, always in the amount of 200 gms. pro kilo. (2 lbs.) At length, in the second half of the seventh month, we come to 200 gms. of mixture No. XIV. pro kilo, then to the same amount of a mixture of 2 pts. cow's milk and 1 pt. sugar water; in first half of eighth month the same quantity pro kilo of a mixture of 3 milk to 1 sugar water; in the

second half of the eighth month, 4 milk to 1 water, then pure milk and other food. It is easy to calculate from this that the child will receive in the third month 3.5 and 3.8 gm.; in the fourth month, 4.1 and 4.3 gm.; in the fifth month, 4.5 and 4.7 gm.; in the sixth month, 4.9 and 5.0 gm.; in the seventh month, 5.1 and 5.3-6.0 gm.; and in the eighth month, 6.7 and 7.2 gm. albumen pro kilo body weight. The quantity of the mixtures given daily can also be easily calculated, and will be in the first month 600-700 cm., and in the second, with a weight of 4.5 kilos, 900 cm., thus corresponding closely with the average quantity of mother's milk taken (Ahlfeld). A little less must be given to delicate children, about 175 gms. pro kilo, instead of 200 gms. If digestive disturbances set in, we must go back to weaker mixtures, thus from No. VIII. in the fourth month to No. IV. or No. III.

What has been said applies also to the "natural cream mixtures," so far as the daily quantity given and the amount of albumen contained therein is concerned. As to the different mixtures, we may regard mixture I. of natural cream ($\frac{1}{8}$ litre—quart—cream, $\frac{3}{8}$ litre water, 10.0 gms. sugar) equal to Nos. I. and II. of artificial cream, containing 1-1.2% albumen; mixture II. (the first with $\frac{1}{16}$ litre milk added = 1.4-1.5% alb.) equal to Nos. III. and IV. artificial; mixture III. (No. I. with $\frac{1}{8}$ litre milk = 1.8-1.9% alb.) equal to Nos. V. and VI. artificial; mixture IV. (I. with $\frac{1}{4}$ litre milk = 2.3-2.4% alb.) equal to Nos. IX. and X. artificial; mixture V. (I. with $\frac{3}{8}$ litre milk = 2.6-2.8% alb.) equal to No. XIV. artificial. There No. I. must be given in the first month, II. in the second, III. in the third and part of the fourth, IV. in the fifth and sixth, and V. in the seventh. The fat percentage thus advances from 2.5% to 3%. Sugar remains at 4%.

The diluted cow's milk mixtures are not so exactly graded. No. I. (1 of milk to 3 of sugar water) = the thinnest cream mixtures, and is given in the first month; No. II. (2 pts. water = 1.3-1.5% alb.) = the cream mixtures used in the second month; No. III. (equal pts.) = cream mixtures given in the fourth and fifth months; No. IV. (2 of milk to 1 of water) is fitted for the end of the seventh month, as already stated. The fat percentage is here a little lower, 0.8% to 1.9%, and the sugar is about 4%-4 gm. to each 100 gm. water. The quantity given is about the same as for the other mixtures, 200 gms. per kilo body weight. As a rule, however, on account of the lesser quantity of fat in the diluted milk, a little larger quantity may have to be given, or the stronger mixtures used earlier, always supposing the digestion is undisturbed. The milk, as well as the water, is boiled, and the daily quantity prepared in the morning, and kept on ice. In calculating the weight, the quantity given is always for $\frac{1}{2}$ kilo in advance. That is, if the child weighs 3.25 kilo, the quantity given for 3.50 kilo, and, as soon as that is reached, for 3.75 kilo. Then follows a series of cases in which the application of these rules is given in detail and the excellent results shown, and the author closes his whole article with these important conclusions:

1. Remarkably small amounts of nourishment, especially in the first months (such as have been above given), are sufficient for good development of infants. Our most important task is the precise and most available preparation of these substances.

2. An increase of fat above what is contained in dilute cow's milk spares albumen, makes digestion easier, and increases the growth of the child.

3. In using the milk mixtures, therefore, we must give a slightly larger quantity or a stronger mixture, always watching the digestion.

4. The need of albumen is less in the early months, but increases rapidly about the seventh and eighth month.

5. Sickly children must use mixtures suitable to younger ages, sometimes in slightly greater quantities, because they have been accustomed to drink more, but as soon as possible the stronger mixtures must be given.

6. Careful regulation is necessary, as the child has no instinct which guards against indigestible and over-feeding.

7. In the first months, this regulation is above all of importance. Even breast children drink too much then, and it is only the harmlessness of mother's milk which saves them from digestive troubles.

3. Hirschsprung: Catheterization of Infants (*Jahrbch. f. Kind heilkde.*, XIX. B., 4 H.).—Most of the books state that kidney diseases are rare in childhood, speak of the difficulty of procuring urine for examination, and apparently express great fear of catheterization in infancy. It is to combat these views that Prof. Hirschsprung writes. He claims that it is simply because the catheter has not been used and the urine tested, that we have not found more kidney troubles in infancy. He explains away the various objections and fears concerning catheterization, describes his method and the thousands of cases in which it has been used without the slightest bad effect. He prefers a metallic catheter with a rather short end and the point solid to the eye. There is nothing new in his method of introducing it. He quotes from Kjellberg to show (from autopsies) the frequency of kidney troubles in childhood, independent of the acute infectious diseases, and hints that in the latter perhaps the kidney trouble existed first. He claims that in every case of infantile sickness in which there is the slightest doubt of diagnosis, the urine should be examined, and gives several cases in detail to show how the regular examination of the urine in the children's hospital at Copenhagen often showed totally unsuspected affections of the kidneys. From his list he finds examples of cases of nephritis complicating such diseases as Kjellberg has shown to have a tendency in that direction—especially acute intestinal catarrhs, less often inflammation of pleura and lungs, meningitis, erysipelas, pyemia, syphilis, etc. To these he now adds tetanus neonatorum. Beside these cases, which may end fatally or get well, according to the course of the main disease, and without much reference to the kidney complication, there are others in which the latter is undoubtedly the main disease, but yet in which it is not betrayed by the slightest symptom, and is only to be discovered by examination of the urine. He details several cases in point. Some of the most interesting were cases of skin diseases, and especially cases of eczema of head and face, in which his experience has led him to believe there is an unexplained but marked tendency to nephritis. In these cases, when albumen is found, the prognosis is bad. In one case of convulsions he found pus in the urine, a rare thing in childhood, and the case yielded to treatment for pyelitis. Such words from such a man as the author should certainly impress us all with the importance of examination of the urine of children, especially when catheterization is so easy and so safe as he states.

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ORIGINAL COMMUNICATIONS.

AN INQUIRY CONCERNING THE RELATIVE INFLUENCE OF THE SEX OF THE FETUS IN UTERO, ON THE MENTAL, PHYSICAL, PHYSIOLOGICAL, PATHOLOGICAL, AND DEVELOPMENTAL CONDITION OF THE MOTHER DURING GESTATION, LACTATION, AND SUBSEQUENTLY.

BY

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For convenience of classification we shall treat the subject of our essay under the following different propositions, taking each in chronological order from conception to weaning.

Influence of child without regard to sex.

Relative influence of each sex.

1. On producing extrauterine gestation.
2. On the time of quickening.
3. On the temperature of the mother.
4. On the pulse of the mother.
5. On the composition of the blood of the mother.
6. On the digestive system.
7. On the composition and quantity of the urine.
8. On producing edema of the vulva and limbs.
9. On inoculating the mother with the constitutional peculiarities and maladies of the father.
10. On the duration of gestation.
11. On the position and presentation of the fetus.

12. On the duration and production of difficult labor.

13. On determining the site, weight and conformation of the placenta.

14. On the quantity, quality, and duration of the lochia.

15. On the quantity, quality, and duration of the lacteal secretion.

16. On arresting the growth, development, and nutrition of the mother.

17. On the determining the sex and weight of the next succeeding fetus.

18. On the length of the inter-gestation period.

19. On the mental condition of the mother.

20. Influence of co-twins of different sexes on each other.

A pardonable curiosity has been manifested in all ages concerning sexual differentiation in the human species, particularly with regard to the causes which determine the production of the sexes, and the influences reflected by the peculiarity of each sexual organization on the mother during the time she carries the fetus in utero, and subsequently on the product of such conception itself, thereafter throughout life.

Among the ancients, medical men, philosophers, divines, and astrologers, such as Aristotle, Hippocrates, St. Thomas Aquinas, Lactantius, Pliny, occupied themselves much with these questions, as have innumerable others down to the present day.

Innumerable then are the authors and their opinions on the influence of the fetus in utero on the mother, and many of these have occupied themselves with a differentiation of these influences by sexes.

To arrive at a clear understanding of these influences, it will be necessary to review the influences in general of the fetus in utero on the mother without regard to sex, and in the second place, to study the influence of the predetermined developmental powers peculiar to each sex conferred on the fetus at the time of conception, and thirdly, to return to our theme to study and compare the different influences of children of each sex on the mother during gestation and lactation.

By a careful study and analysis of these effects of a difference of sex, we hope to discover the causes producing them, and thus add to our knowledge of those influences which are instrumental in determining the sex of the child.

Concerning the effect of the sex of the fetus on the "color" of the mother, Sebizi¹ (p. 495) very properly remarks that these aphoristic propositions are for the most part true, but for obvious reasons they are to be taken *cum grano salis*, as all women who bear males are not necessarily in good health at the time, and consequently may not have a good color, but given two women in good health, the one who carries a boy will usually have a better color, better appetite, be lighter in her movements, and experience less malaise in general, than one who carries a female. Hippocrates, Galen, Aristotle, and Avicenna are all of the same opinion.

The following are the signs of a male conception as given by Avicenna, *L. 3, Can. Sen. 21 tract. 1. C. 13*: Her color is better, her action is more agile, that is, more prompt and expeditious in her movements, the burden or weight of the uterus is less sensibly felt, her appetite is better; she is lighter in going about; should the gravid uterus incline to the right side, as usual with males, the right breast will be more enlarged, contain more milk than the left, the milk will be thicker, less aqueous, the nipples will be redder, the right pulse will be fuller and stronger. In rising to walk she will move the right foot first, her right arm will be strengthened, the right eye will be brighter and move more quickly. The contrary signs prevail with a female conception.

Dr. Carlile,² of Palmyra, Ill., claims to have discovered that in pregnancies with male children the areola of the nipple is "very dark in color," while with females it is "not very dark in color." He says that he "found almost invariably that when the sex is male the color of the areola was much darker than when the sex was female." In this manner nineteen out of twenty males were determined (predicted), and eleven out of eighteen females.

I have not met with any opinions concerning the rust-colored spots on the skin of pregnant women, whether they prevail more with male conceptions than with female.

According to Aetius, *L. ferm. 4, c. 9*, if a woman conceives with a boy, the right nipple is larger, the veins and arteries on

¹ Melchior Sebizii, *Excitationes medicæ. De discrimine corporis virilis et muliebris. Item de notes Virginitatis*, 4to, Argent., 1672, pp. 94.

² *Med. Record*, N. Y., May 15th, 1880, p. 554.

the right side are perceptibly tumefied, as one may see under the tongue; the pulse of the right arm is healthier, swifter, greater, and harder. The pupil of the right eye is larger and more brilliant than the left. The arteries of the right temple are more agitated and swollen than on the left side. Her spirits are gay, mature milk is generated, and the mammæ are more solid. If the woman conceives of a girl, she is sad, the mammæ are flaccid and more pendulous, the temporal artery of the left side is tumefied, and the milk arrives later. If the milk is dropped into the urine of a gravid woman it sinks and remains at the bottom if a male, but rises if a female. If a drop be allowed to fall on a mirror and it adheres together and rolls about like quicksilver, it is a male; but if it separates, it is a female.

Signs of a conception with a male. [Out of Culpeper.]

“1. The woman, when she riseth up from a chair or the like, doth sooner stay herself upon the right hand than on the left.

2. The belly lies rounder and higher than when it is a female.

3. The child is first felt to stir on the right side, because, according to Hippocrates, the male children lie on the right side of the womb.

4. The womb breeds boys easier and with less pain than girls, and [the woman] carries her burden not so heavily, but is more nimble in moving.

5. The right breast is more plump and harder than the left, and the right nipple redder.

6. The color of the woman is more clear and not so swarthy as when she conceives a girl.

The contrary to these are signs of conception of a female; it were but loss of time and blotting of paper to quote them.

These are some of the vulgar rules, and the best of them. I never knew these fail:

1. If the circle under the woman's eye, which is of a wan-blue color, be more apparent under the right eye, and the veins most apparent in her right eye, and that most discolored, she is with child of a boy; if the said marks be most apparent in the left eye, judge her to be with child of a girl.

2. Let her milk a drop of her milk into a basin of fair water,

if it sinks to the bottom as it drops in, round in a drop, it is a girl she goes withal; for if it be a boy, it will spread and swim at the top. This I never knew to fail, though it be contrary to all authors that I ever read."¹

Dr. McDonal¹, of Liverpool, claims to have predicted unerringly the sex of the fetus during the last month of gestation in every case (eight multiparæ and one primipara) by the external form of the abdomen. He alleges that "if the form of the abdomen be conoid and projecting (child carried in front), the child is male; if the form be more flattened and rounded (child carried round about), the child is female."

Dr. McD. suggests that strips of lead be used to determine the shape or projection of the abdomen in the median line, and transversely. The doctor has reason to believe the method applicable to breech presentations, and to a period of at least two months before delivery. As yet, "he has not constructed an hypothesis to explain the facts." Oslander, as will be seen further on, has much the same views, claiming that the abdomen becomes more depressed with advancing pregnancy when boys are carried.

From Wagner³ we gather the following:

Occasionally it is well to note the diversity, according to the sex of the fetus, of the changes in the female body during pregnancy. Indeed, most of the phenomena derived therefrom in all ages are extremely fallacious and uncertain, and therefore judgment of the sex of the fetus during pregnancy can only be made cautiously, if indeed at all [*literally, and almost not at all*]. Hence, far back in the earliest times men endeavored to foretell the sex of the fetus, and as generally happens, sought to support their diagnostic signs with false hypotheses. At all events, it is plain to see from various proofs that there exists, according to the sex of the fetus, some diversity in the general mutations of the female body. For in the first place, experience teaches it is necessary that the constitution of the

¹ Nicholas Culpeper: A Directory for Midwives, etc. 12mo. London, 1671, pp. 102-3.

² The Antegenetic Discovery of Fetal Sex. London Lancet, February 3d, 1883, p. 222.

³ Wagner (Gulielmi): Commentatio de Fœminarum in Graviditate Mutationibus. 4to. Brunswige, 1816, pp. 150-155. (Secundum Fetus Sexuum.)

maternal body (which is) to procreate males be other than that (whose office it is) to procreate females; as this is already required in coitus—indeed, as pregnancy is naught else than the continuation and evolution of coitus itself, or rather of that state which begins with coitus, it is likewise evident that during pregnancy the constitution of the feminine body must be different according to the sex of the fetus; in fact, a certain peculiar disposition of the feminine body is required not only in coitus to determine the sex of the fetus, but also in pregnancy to evolve it. Thereupon we may, from the diverse nature of boys and girls, divine this; when the difference between them (quorum) has been evolved in the maternal body, and a peculiar (certa quadam) conjunction, this too dependent on sex, intervened with the maternal body, it may be considered probable that (all this) has entered into the disposition of the infant in a certain ratio; although (licet) the evidences of this are not always certain. This is especially illustrated in our times in the disquisitions of the celebrated Osiander,¹ who certainly affirms that those women who bear females in their womb universally enjoy better health than those who bear males. For the illustrious man asserts that in the latter there is frequently to be observed a fancy for certain dishes and again an immoderate appetite for other ones. That they are oftener taken with vertigo and fainting; that they are more inclined to somnolence, suffer oftener from inflammation of the breasts, heart-aches, colic, bad stomach (alvi obstructione), flatulency, and the swelling of the stomach which arises therefrom, but that the abdomen becomes more depressed with advancing pregnancy. But in those bearing females in their womb he more frequently noticed nausea and vomiting, and in the interval he saw that their infirmities were extremely prolonged (admodum dies ægrotantes). In addition, Astruc and Wigand² assert only in those pregnant of males that often a darkish tract may be seen on the abdomen, but not on those with females. Which nature of the female body appears to me upon great consideration to be more en rapport with the maternal one, and not to affect great changes therein; whilst

¹ Denkwürdige Reden für die Heilkunde und Geburtshülfe, Bde. 2.

² Beiträge zur theoretischen und praktisch. Geburtsh.

on the other hand the male fetus, more foreign to the nature of the mother, may affect it more deeply (*gravius*).

In addition to this, when we consider that it is probable that a larger proportion of females are conceived during the first half of the inter-menstrual period, and a larger proportion of boys during the last half of this period, we admit that different physical or physiological conditions are necessary to beget one or the other sex, and these conditions probably continue during gestation.

It has been observed among various peoples that a prepotent force is necessary on the part of the father to beget males, and on the part of the mother to beget females. Hence it is very clearly to be seen that in each coitus one or the other sex is determined at the time of the act. For whatever conditions preside at the time of conception continue to manifest their influence throughout the period of gestation, and the sex of the fetus in its turn determines the diversity of the peculiarities in the constitution of the mother. From the manifestations of these peculiarities we may read or foretell the sex of the fetus in utero.

Much has been said by the ancients, and gainsaid by the moderns, about the superiority, higher temperatures, and male-producing qualities of the organs of the right side of the body, and now we have to record the fact that in a certain case the milk of the right breast contained much more solid matter, from one and one-half to nine times as much as that of the left, and so great was this difference in quality that the children of each pregnancy refused to nurse the left breast.¹

The ancients also contended that the right ovary and the right testicle elaborated the elements which were destined to produce male conceptions, and the production of females was equally ascribed to the left.

Whether there is any difference in the size and weight of the ovaries of the right and left side I do not know, but we think it probable that the right ovary is larger and heavier than the left, because we know that the left testicle is larger and more pendulous than the right. And this, added to the following concerning the Fallopian tubes, tend to strengthen this view :

¹ London Lancet, April, 1871, p. 215, Amer. edit.

"Up to the seventh month of fetal life both tubes are of equal length; from that time, however, the growth of the left is a little less than that of the right. The average result of ten examinations of girls who died before puberty was as follows: The right tube measured 5.98 cm. in length, the left 5.7. The average of nine examinations of virgin women who died between the ages of 16 and 81, was 9.5 cm. for the right, and 8.3 cm. for the left tube. After the menopause the oviducts may grow from 2.5 to 4.5 cm.

During the active period of the sexual organs, the difference in length between the two tubes is less marked; the average of twenty-two examinations in married women under 45 years of age gave for the right 11 cm. and for the left 10.9 cm. After the menopause, however, the difference is striking, being in twelve women examined between 46 and 80 years 9.75 cm. for the right and 9.1 for the left."¹

Dr. Blackwood,² of Philadelphia, has recorded a most interesting case of alternate menstruation from the right and left sides. This woman, aged twenty-two years, was found to have a hymen with two orifices, a double vagina, with a cervix uteri opening into each, and the uterus was completely divided by a longitudinal septum. At the time of menstruation there was congestion of one side of the uterus only, viz., that from which the flow occurred. And menstruation occurred alternately right and left, with one exception, during the thirteen menstrual periods she was under examination.

A patient writes that "every other month menstruation continues two days, and on the alternate months its duration is from four to five days. The two-day periods are painless and natural, but the alternate periods are always attended with pain and a sense of weight or bearing-down for about twenty-four hours at the commencement."

The author³ from whom I quote the above says, "I am unable to suggest any theory which will explain these facts except that of alternate menstruation." Continuing, he says, "I have, however (having had my mind for years directed to this point), found several cases in which the patients had long been con-

¹ AM. JOURN. OBSTET., New York.

² Medical Times, Oct. 25th, 1879.

³ Trall's Sexual Physiology, 12mo, N. Y., p. 184-5.

scious of pain and distress only in the region of one ovary at the menstrual period, and in nearly all of these cases the patient (on calling her attention to the subject) could very clearly recollect that there had long been a difference in the menstruation, that of each alternate month being more prolonged, more painful, or more hemorrhagic, or different in all of these respects."

We have stated that the intention and tendency in a state of nature is to produce the sexes alternately, male and female, and we here see that it is probably the same intention to mature a Graafian follicle alternately, one month in the right ovary and the next month in the left, and these differentiations in the position (side) being different in incipient force and intention, cause a reflex action on the woman different in intensity, exhibiting itself in a different degree of malaise, lassitude, irritability, congestion of uterus, quantity, quality, and duration of menstrual and lochial discharge.

We have always argued that the begetting of *females* on the *part of males* was a higher or more difficult rôle than the begetting of males, or in other words, each sex calls into action a higher genetic force or function to beget the opposite sex than it does to beget its own. We know that the left testicle descends first into the scrotum, is larger, heavier, and more pendulous than the right, from which we infer that there may be some truth in the saying that the right testicle generates males and the left females. Those who have opposed these opinions by what they consider as unanswerable facts, rely upon the fact that men having lost one testicle and women having lost one ovary beget indifferently male and female offspring.

This argument is based upon a wrong interpretation of this theory, implying that the ovary or testicle of one side is *predestined* to beget *one sex* and that sex only. Whatever may have been the intention of the advocates of this theory, we do not believe that there is such a thing as *predestination of sex*, but that one or the other sex is produced, determined at the moment of conception only, by the relative maturity, relative vigor, or relative quantity or force of the elements of reproduction derived from each parent uniting to form the product of conception.

If it be admitted, therefore, that the right side is stronger,

more highly developed, warmer, etc., than the left, it is probable that the ovum developed in the right ovary may be superior in these respects to that of the left, and should the requisite quantity and quality of sperm arrive at the proper time, it will probably determine a male conception. Should one ovary or testicle be extirpated or become inactive by disease, the remaining one will perform the functions of both and evolve an egg or a spermatozoid capable of producing a male or female, according to the condition of the male and female during the elaboration of these elements, and this single ovary may tend to produce alternately ovules, or spermatozooids, at one time suited to produce a male, and at another time more apt to produce a female.

I do not wish to be understood to contend that males are produced only from the ovary of one side and females only from the ovary of the opposite side, but that, in healthy females living in a state of nature, there is an *intention* in the laws of nature to produce ovules in one ovary at a time and the next succeeding ovulation in the ovary of the opposite side, alternately right and left, and that the ovary of one side (probably the right) usually produces ovules better suited or in a condition more likely, when fecundated, to result in a male conception; and that the opposite side produces alternately ovules more likely, when fecundated, to produce females, though this rule is not invariable, and ovulation may take place from the same side twice in succession, producing the same or a different sex each time, probably a different sex each time, because the influence of the sex of the previous child on the mother always has a *tendency* to cause a change of sex in the gestation immediately succeeding, particularly where the woman nurses the child the usual period, and conceives within one year from the birth of the last child, or before the effect of the sex of the previous child on the condition of the mother has entirely disappeared.

1. *Influence of the Sex of the Fetus in Producing Extrauterine Gestation.*

Almost nothing is definitely known of the causes or conditions which favor extrauterine gestation, though some cases have been ascribed to fear or strong emotion at the time of coitus or conception.

It is a fact frequently observed that such women have shown previously a remarkable inaptitude to conception during periods varying from five to eighteen years in the cases recorded. In these cases we should suspect atony or abnormal adhesions from disease of the canal or extremity of the Fallopian tubes and adjacent organs.

Some years since, Dr. John S. Parry,¹ then preparing his book on extrauterine gestation, at my suggestion (having called his attention to the views of Dr. Hubbard²), sought out and recorded the sex in as many cases of extrauterine gestation as were within his reach, with the following result, *viz.* : 41 or 55.4 per cent males, and 33 or 44.6 per cent females. Dr. Hubbard contended that, from the position he took in regard to the determination of the sex of the child, females being produced at the commencement of menstruation, and males after menstruation ceased, that the fimbriated extremity of the Fallopian tube was more likely to fail to erect itself and seize the ovary at the point of the rupture of the Graafian follicle at the end or after menstruation had ceased, than during the earlier days of this process, the time by him ascribed to the conception of females, hence more male conceptions would fall into the abdominal cavity than female.

This view is not strengthened by the few cases Dr. Parry has been able to bring together. Dr. von Mansfelde³ contends that the child in cases of extrauterine gestation is usually of the female sex, an opinion deduced from his theory, and based on the few cases he could have seen.

Here again the importance of stating the sex of the fetus is of the greatest moment. As it is, it is wanting in so many cases that we have too few data to come to any valuable conclusion.

2. Influence of Sex of Fetus on Time of Quickening.

Avicenna taught "concerning the first motion of the infant: If to the time wherein the fetus received its formation, you shall only add twice that space of time, that is the time of the motion

¹ John S. Parry: Extra-Uterine Gestation. 8vo, Philadelphia, 1876, p. 82.

² Silas Hubbard, Buffalo Medical Journal, 1851, vol. vii., p. 251.

³ Sex Production. Proceed. Nebraska State Med. Soc., 1880.

of the fetus. Wherefore Hippocrates has said before, if the male be formed in thirty days, that is in one month, it will in like manner be moved in sixty days, after the thirtieth, that is the ninetieth, to wit, in the third month.

"In like manner, whereas he hath said that a female is formed in forty days, if more than double be added to these, that is, eighty-four more, then the female begins to move itself, to wit, in the fourth month and six days.¹

"But the female obtains its primary formation in two and forty days at farthest, but the male in thirty, which is the longest time. . . . Truly for the most part those women that bring forth females are purged two and forty days after the birth, which purgation, as it is very long, so it is complete, . . .

"But such as bear males have their purgation thirty days, which are the longest of that sort, and in fullness and perfection (p. 17).

"Women have their purgations after child-birth for this reason, because before the two and fortieth day, if it be a girl, and the thirtieth if it be a boy, there is but a small quantity of blood contributed for the increase of the fetus; but afterward it flows in, more abundantly, until the infant comes into the world. Wherefore it is necessary that the purgation at the birth should flow and be brought forth, in proportion to the number of days.

"Now I shall speak something, to demonstrate that the limbs of the child are distinguished and discerned, the female in two and forty days at most, and the male in thirty. Of which thing the purgation which happens after the birth, for two and forty days of a female and if a male thirty, which is indeed the longest space, is a confirmation. . . . And because the female increaseth and is formed more slowly, it is manifest that the seed thereof is weaker and moister than that of the male, and for that reason must be longer in forming than the male, and that the purgation must continue longer after the birth of the female than the male.²

"Tearmes flow after the fourteenth day, and the childe is felt to stirre and move after the fifth day. To be short, all things

¹ Gorraeus, *Annotat. upon Hipp. Concerning the Seed*, p. 98.

² Hippocrates, *Two Books Concerning the Seed*, etc.

are more quick, lusty, and strong in the male than in the female, *Reusn.*

“And to be short, because the seed of the male is hotter and livlier than of the female, therefore all things in the male are sooner performed, as the male receiveth all the lineaments of his body, and perfect shape of a man in his mother’s wombe in thirty days: the female in forty. The male beginneth to stirre in the wombe in three months: the female in foure. The male is borne and cometh forth of the wombe in nine months, the female in ten. And the mother herselfe after the birth of a male, hath forty daies assigned for her purification; but after the birth of a female, four-score daies, *Leviticus*, 12; 4, 5. And that men are hotter in constitution than women, may easily appeare in that they have fewer excrements, larger veines, blacker colour, greater and lustier members, greater voice, more audacity and courage than women, *Bertinas.*”¹

Concerning the movement of the child in the womb, Hippocrates says that it “happens to the male in three months, but to the female in four months.

“But the male is stirred first because it far excels the female in strength; and is likewise formed soonest, because the seed that composeth it is stronger and thicker.”

Elsewhere we have shown from the record of forty cases observed by Ravn, that the average number of days from conception to quickening was 134 with boys, and 149 with girls.

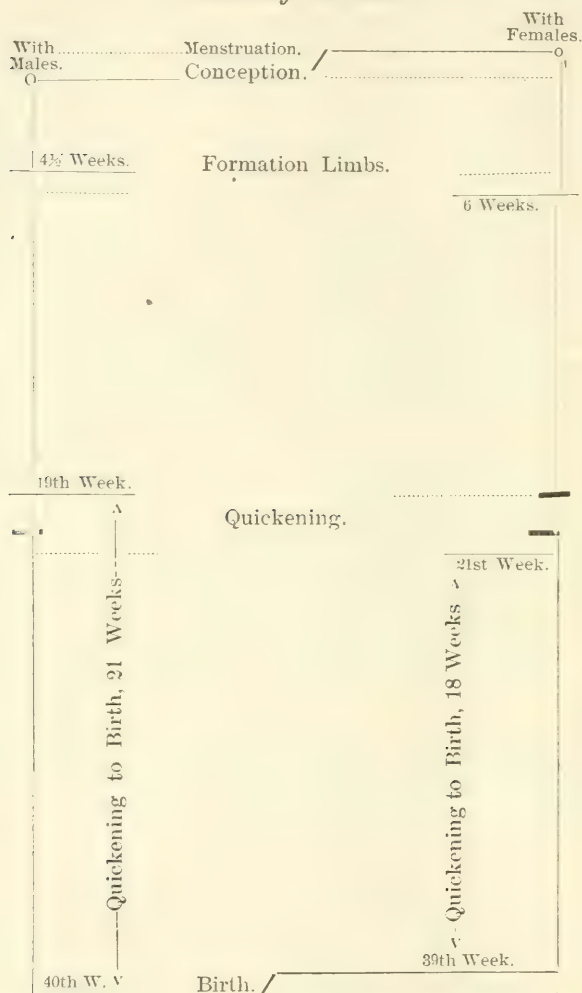
Unfortunately we have not been able to gain access to a copy of Claude Bailly,² wherein he gives his reasons for believing that the male is formed more quickly than the female.

The following table will exhibit more clearly these differences.

¹Fletcher, I. The Differences, Causes, and Judgments of Urine: according to the best writers thereof, both old and new. 8vo, pp. 124. Lond., 1641.

²Ergo mas celerius fœmina tardius conformatur. In folio, Paris, 1569.

Table Illustrating the Periods of Development of the Fetus According to Sex.



The function of nutrition, says Moreau,¹ commences immediately after the conception, and according to Hippocrates,² it is less active if the embryo is male, than if it is female. He adds, "If the opinion of Hippocrates was confirmed, the male

¹ Hist. Nat. de la Femme. Vol. i., p. 177.

² De Natura Pueri. Lect. x.

embryo would be formed thirty days after the conception, and the female forty days after the same epoch."

Moreau very properly observes, that "after the birth at least, this alleged slowness of development in the female will be changed, since the nutrition and growth go on with much more rapidity among women than among men. Their interior organs, their external form, their faculties, all develop in a very rapid manner, and the body of the woman is as well formed at twenty as that of the man is at thirty."

We contend that this change in the relative rapidity of development begins at the time of quickening, and that the female fetus consumes a fortnight less time in developing from the moment of quickening to the time of birth than does the male, as may be more clearly seen in another part of this paper.

3. *Influence of Sex of Fetus on Temperature of Mother.*

"Dr. Granville¹ has asserted that the temperature of the uterus sometimes rises as high as 120° F., the elevation seeming to bear some ratio to the amount of action in the organ." Dr. Dunglison had some observations made with a view of determining these questions, and Dr. Barnes, to whom was intrusted this duty, found that in three cases where the labia were respectively 100°, 100°, 102°, the uterus was 100°, 102°, 106°, during parturition. Fricke and Gierse² could not find any constant elevation of temperature in the vagina of pregnant females, yet they found a maximum excess of .54°, hence the following verses:

"For then, if she conceives the genial fruit,
The soil has strength to feed the spreading root.
Her vital heat increases, and her blood
Then swells within her womb, a rosie flood
From whence the future birth imbibes its food.

—Quillet, *Callipædia*, B. 1, p. 23. Edit. 1872.

Close observation would probably show an appreciable elevation in the temperature of the uterus during gestation, as compared with the non-pregnant state, and there would probably be a greater elevation when carrying a male fetus than when carrying a female, particularly before quickening, though this eleva-

¹Philos. Trans., 1825, p. 262; and Sir E. Home, *Compar. Anat.*, v. v. 201. London, 1828.

²Henle: *Handb. d. Ration. Pathol.*, 1846.

tion might be reduced in favor of the female, during the period subsequent to quickening, owing to the more rapid development of the latter.

Temperature of fetus a few tenths of a degree (Centigr.) higher than that of the mother.

4. *Influence of the Sex of the Fetus on the Pulse of the Mother.*

M. De la Brousse,¹ in a letter to M. Desbrest, says: "I maintain that the pulse of the radial, temporal, and other arteries (of the two sides of the body) is always equal in perfect health, and that in disease it is always stronger on the side affected or suffering." This observation, he says, is confirmed by the researches of MM. de Borden and Fouquet. Continuing, he says the contrary happens in pregnancy. There is often feebleness in the pulse on the side to which the infant inclines the more, and it is without doubt occasioned by the compression it causes on the arteries of the lower belly, which makes itself felt on the radial of the same side.

Hippocrates, in the forty-eighth aphorism, assumes that "foetus qui mares sunt dextrâ, foeminæ sinistrâ majis sunt." De la Brousse says, "I will endeavor to affirm this passage and add to the means of proving it."

He declares that in thirty observations made with the object of predicting the sex before the labor, made for the most part from the sixth to the ninth month, he always announced the sex correctly, except in three cases, from simply feeling the pulse of the pregnant woman. In the cases in which he failed, the women were more or less sickly.

He declares that the body is divided into two lateral halves, of which the right side is the stronger. Among pregnant women, on the side to which the child inclines the pulse is more feeble, by reason of the weight which compresses and hinders the circulation, and in consequence the physician who finds

¹ Sur la connoissance du poulx dans les grossesses, qui peut servir à distinguer les mâles et les femelles, avant l'accouchement.

Journal de Med. Chirurg. Pharm., Aoust, 1771, pp. 121-129.

Lettre à M. Desbrest qui peut servir de suite aux observations précédentes sur la connoissance du poulx dans les grossesses, *ibid.*, pp. 129-133.

Observations sur les différentes sortes de poulx, *ibid.*, p. 134-141.

De la Brousse. Réponse à M. Amoureux, fils, *ibid.*, Sept., 1771, pp. 227-233.

the pulse more feeble on the left side in a pregnant woman who is otherwise in good health could boldly announce a girl, and the contrary when the pulse on the right side will be more feeble.

Amoureux,¹ fils, in his letter to M. De la Brousse concerning the doctrine of the pulse, says that in China² they feel the pulse with four fingers in three different places, that is to say, at the wrist, at the junction of the wrist, and at the cubitus.

The pulse of the wrist of the right hand indicates that which regards the heart and the small intestines; the pulse of the juncture of the same side indicates that which regards the liver and gall; the pulse of the extremity of cubitus of the same side indicates that which regards the left kidney and the bladder. The pulse of the wrist of the right hand announces the affections of the lungs and the large intestines. The pulse of the juncture on the right hand regards the orifice of the stomach and the ventricles of the heart, and the extremity of the cubitus the disease of the kidney of the same side.

The Chinese "secret of the pulse" maintains that "in a man the pulse at the wrist should always be more strong than that of the cubitus. . . . On the contrary, the pulse of the woman at the cubitus ought always to be more strong than that of the wrist." P. 409.

"If a woman generally has the pulse at the extremity of the cubitus small, weak, and sharp, the abdomen is generally cold, and subject to various shiverings, though she be ever so young, and she may be certain she will never have a son, but if she is advanced in years she will have neither son nor daughter." P. 418.

The ancient book gives the plain rule. When the pulse is superficial or deep, as it ought to be in the three places of each arm, and the finger being pressed upon it, the beats continue to be felt, the woman is with child.

¹ Journal de Méd., Chirurg., Pharm., etc., 12mo, Paris, Sept., 1771, pp. 217-227.

Seconde lettre à M. De la Brousse, sur la découverte du pouls de grossesse qui fait distinguer les mâles et les femelles avant l'accouchement, *ibid.*, Juillet, 1772, pp. 62-76.

² Du Halde: Description de l'Empire de la Chine, etc., 4 vols., folio, Paris, 1735. (Fin du 3me vol., traduit. du Livre chinois sur le pouls par le père Harvieu, missionnaire. English transl., 2 vols., folio, London, 1791; another edition in 8vo, London, 1736.)

In the first months of pregnancy the pulse of the wrist is often small, and that of the cubitus quick; if, in pressing it with the finger, it seems to disperse, she is three months gone; but if it does not disperse, but keeps its usual consistence, she is in her sixth month.

When the termes cease after conception, if the pulse be long, tremulous, 'tis a false conception. In the seventh or eighth month of pregnancy, if the pulse be full, hard, and strong, 'tis a good sign; if deep and slender, the woman will have a hard labor, and die in child-bed.—Du Halde, p. 195, vii., ed. 1791.

Continuing, he says: "I have found nothing for the womb, but here is the pulse for that which concerns pregnancy: 'When the pulse of a woman is felt at the extremity of the cubitus and it is found to be *Hoa*, slippery, it is a sure sign that she is pregnant. If it is at this place of the right hand that you feel the pulse, and find at the same time *Hong*, overflowing, she is with child of a daughter.'

If it is in the left hand that the same is found, she is with child of a son. If the pulse at the same time is found the same in both arms, the woman is pregnant with two children. He who understands this method will never be deceived."¹

5. *Influence of the Sex of the Fetus on the Composition and Quality of the Blood of the Mother.*

Bequerel and Rodier analyzed the blood of nine pregnant women, and, according to Montgomery, "they conclude that pregnancy exercises a marked influence on the composition of the blood. The density, both of the defibrinated blood and of the serum, is diminished, the water, the fibrin, and the phosphorized fat are increased, while the corpuscles and the albumen are diminished."

Andral and Gavarret analyzed the blood of thirty-four pregnant women, and found that while the fibrin was below the physiological mean of 3 during the earlier months of pregnancy, that during the last three months of pregnancy the fibrin was found above the physiological standard, from 4 to 4.8, or a mean of 4.3 in the last month.—Montgomery, p. 283.

¹ Du Halde: *Description of China and Chinese Tartary*. 4 vols., 8vo, London, 1736. Vol. iii., "The Secret of the Pulse," pp. 366-469, in three parts, p. 376.

"The blood then," he (Andral) says, "manifests a remarkable tendency to assume the character of blood of inflammation, and without doubt we have to reflect on the relation which may exist between the kind of modification which the blood then undergoes, and the development of those special accidents, generally of an inflammatory appearance, which so often affect women recently delivered. Ought we to regard the slight excess of fibrin which in them exists in the blood as a predisposing cause of these accidents?"

On sheep and cows they made some interesting analyses several hours before and after delivery, showing that after delivery the fibrin and blood-corpuscles increased and the serum and water diminished.

We here see that pregnancy causes a change in the composition of the blood, but whether that change is greater in proportion or different in nature when a male fetus is carried from what it is when a female fetus is borne, we have no data upon which to base an opinion, as the sex of the fetus is not given.

We may infer, however, that as the proportion of the various substances entering into the composition of the blood is different in adult males from that found in adult females, and that these differences exist possibly to a greater extent in the fetus (if we may judge from the temperature, pulse-rate, etc.), so it is therefore probable the greater density or less watery condition of the blood in the male fetus determines in the circulatory system of the mother the production of more highly fibrinated blood than when a female conception is carried, and consequently exposes her to greater liability to the inflammatory maladies incident to child-birth.

The following differences were found to exist between the blood of an adult man and woman by Foedisch :

	MAN.	WOMAN.
Iron.....	9	8
Fibrin.....	28	25
Cruor.....	140	129
Albumin.....	91	96
Water.....	732	742
	1000	1000

From an examination of this table we see that the proportion of solids in the blood is greater in man, and the proportion of

fluids greater in woman. Now, whether this difference in the composition of the blood exists to as marked a degree in the fetus, we have no facts to appeal to, but arguing from analogy, we should be inclined to think it quite as great, if not greater, as the difference in the pulse-rate and temperature of the body we know are greater between the male and female than between the sexes in adults.

In the adult we have 58.5 per cent of water, and of fixed substances 41.5 per cent.

According to Bischoff, one finds in the composition of the new-born child 64.4 per cent of water and 25.6 per cent of fixed matters.

Fehling gives from his experiments 74.4 per cent of water in the composition of the new-born.

I have taken the trouble to calculate from a table given by Pinard¹ the relative proportion of water in the male as compared with the female fetus in the fifth, sixth, and seventh month of intrauterine life, with the following result, viz.: Seven boys were composed of 86.22 per cent of water, and six girls gave the proportion of 86.96 per cent of water to 13.04 per cent of solid matters.

We have, therefore, no positive evidence that the blood of the pregnant woman is different in character when carrying a male from that usual in carrying a female; yet we are persuaded that there probably is a difference, however trifling, as we have seen that female conceptions are attended with constipation, indicating an absorption of fluids into the circulation. If the blood contains a larger proportion of water in the case of female conceptions, this might in some measure explain the longer duration of the lochial discharge in such cases.

6. *Influence of Sex of Fetus in Producing Disturbance of Digestive Organs of Mother.*

Aristotle says that purgations happen to many after they have conceived for a certain time, namely thirty days, especially if a female be conceived, forty if a male.

Godart,² after quoting Cardan, *liv.* 12, *De Subtilitate*

¹ Article on Fetus. Dict. Encyclop. des Sci. Med., 8vo, Paris, 1878, p. 479.

² Godart: *Marque Singulière de la Grossesse du sexe.* Jour. Med., Chir., Phar. etc., 12mo, Paris, 1759, pp. 529-532.

and Albertus Magnus, *De Secretis Mulier*, Chap. VIII., says, it would be desirable to know why a certain class of symptoms accompany a masculine pregnancy, and certain others a feminine, and cites the case of a woman who in her first pregnancy was very constipated for a few days, and gave birth to a girl. The same thing happened again in her second pregnancy, and again a girl was born. During a third, fourth, and fifth pregnancy her bowels were moved regularly, even twice a day, and in each of these pregnancies she gave birth to a boy. The sixth pregnancy, finding herself constipated as in the two pregnancies first mentioned, she predicted that she would give birth to a girl, and was not disappointed.

Dr. Mattei¹ records the case of a woman forty-three years of age, in good health and rather stout, who was already the mother of four children, who, in speaking of her pregnancies, said that when she carried a boy she vomited very much, while with girls she did not vomit at all. Dr. M. explains this difference by saying that the presence of boys produces in the uterus more activity than the presence of girls. Possibly the slightly greater volume of the male fetus might aid in explaining this phenomenon.

The constipation in the case cited by M. Godart might be explained by the fact of there being a larger proportion of fluids in the female fetus than in the male. As the fetus draws its supply wholly from the mother, the female fetus must consume a larger proportion of fluids than the male, and this, coupled with the estimated greater rapidity of formation of the female between quickening and birth (probably amounting to a fortnight), we can readily understand why the nourishment and development of the female fetus should be perceptibly more exhaustive physically than the male. Hence we may infer that women are much more frequently constipated carrying girls than in carrying boys, and this would account for the other concomitant symptoms attributed from all time to a gestation with a girl, viz., heaviness of spirits, drowsiness, bad color, indigestion, etc., all symptoms of constipation.

7. *Influence of the Sex of the Fetus on the Urine of the Mother.*

It is now universally conceded that the condition of pregnancy

¹ Clinique Obstetricale, Gazette Obstet., 5 Mai, 1874, p. 104.

has a marked influence on the character, composition, and quantity of the urine.

In 1831 Nauche announced the discovery of the keyesteinic pellicle, in the *Lancette Française*.

Eguisier¹ in 1839 considered this pellicle an invariable attendant on pregnancy, others think it of some value as a corroborative indication, while some consider it of no value at all.

A woman is reported to have had diabetes in three successive pregnancies and not at other times.

Dr. Donné found forty to eighty parts of lime in common urine, while only thirty parts or less were found in the urine of pregnant women.

If a precipitate be made with a solution of baryta in urine of non-pregnant women, twelve to fifteen parts of the salts of baryta will be precipitated, while in pregnant women the urine will give but five to eight parts. Of thirty-six cases of doubtful pregnancy, Donné was only deceived twice by these tests, and Lubaniski detected three cases where all other means had failed.

Now we have shown that pregnancy has a decided influence on the urine, and it now remains to review the various alleged differences in the urine of a woman pregnant with a boy from that usual in women who are pregnant with a girl, from a due consideration of which it is contended that the sex of the fetus in utero may be predicted.

In nearly all the older books on the urine, a chapter is set apart on the prediction of the sex of the unborn child by an inspection of the urine of the pregnant woman, as may be seen from the following, in which this far-seeing wisdom is ridiculed. "After what manner (if divers pisse-messengers come together) they must be examined: How to show (by the urine) the sex, whether a woman be with a child or no, how long it is since she conceived of it, and whether she shall bring forth a boy or a girl, although the urine show none of all these," Chap. VI., p. 80.²

Fletcher³ (p. 72) gives the following signs of "conception in

¹ Du Diagnostic de la Grossesse par l'Examen d'Urine, 8vo, Paris, 1842, p. 79.

² Brian (M. Parlt): The Pisse-Prophet, or certain Pisse Pot Lectures, 12mo, London, 1637-1679, German transl. Hamburg, 1693, 1723.

³Fletcher: The Differences, Causes, and Judgments of Urine, According to the best Writers thereof both old and new, 8vo, Lond., p. 124, 1641.

women. Settling down of the sediment, which if they be red, through greater abundance of heat and blood, a male child is conceived. If white through less abundance of heat and blood, a female child is conceived.

“1. Motes in the sediment red and round, signify a male ; but motes white and round, a female.

“2. Milke [in conception of male] commeth sooner into her breasts, which being milked, and set in a glasse in the sunne, it waxeth hard into a stone, not unlike a bright pearle.

“3. All her right side is better and more lusty than her left, right eye fairer, right pap greater with milke, the pulse of her right artery swifter.

“4. If a woman's urine be kept three days in a glass bottle stopped, and after strain it through a fine, cleane linen cloath, if there appeare little quicke living creatures, and red, a male is conceived, if white a female.

The “uryne of a woman that is with chylde, her water shall have some cleare strykes, the most parte shall be troubled, and the troublennesse shall be reedysche, in the manner of tawney, and this token shall never fayle, as soon as the chylde hath lyfe, and if it be a gyrl, the troublennesse shall draw downwards and if it be a boye the troublennesse shall have above.”¹

While the composition of the urine of a woman pregnant with a boy is probably different from that of a woman pregnant with a girl, and easily demonstrable, yet it is too much to expect that it should be so invariably (owing to a difference in temperament, diet, age, kind of nourishment, etc.), that the sex of the child could usually be predicted by such a test.

On the other hand, we feel persuaded that, should the urine of a sufficient number of women, pregnant with boys, be compared with an equal number in same condition of life, temperament, etc., pregnant with girls, there would be found an appreciable difference in its composition, density, quantity, color, etc., sufficient to show that the sex of the fetus in utero has a perceptible influence in determining the character, composition, and quantity of the urine secreted during pregnancy, thus affecting the physical condition of the mother during gestation.

(To be continued.)

¹ Here beginneth the seigne of the uryne, etc., 16mo, Lond., 1552, p. 32.

A CONTRIBUTION TO THE THERAPEUTICS OF PUERPERAL
ECLAMPSIA.

BY

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It is not my intention to discuss the pathology of this disease, nor yet to go into details regarding its treatment. I wish to record two cases, one of which was fatal, while the other recovered.

Mrs. D. sent for me at five o'clock A.M. When I arrived at the house, I found her in labor, the os rigid and but slightly dilated. The feet and hands were swollen, the eyelids puffy, there was severe headache, and some nausea. I waited with her for about half an hour, when she had a convulsion. I gave her chloral hydrate gr. xv. every fifteen minutes until three doses were given, and after an hour another dose of fifteen grains.

Convulsions came on at short intervals, and were very severe. I then put her under chloroform, keeping up the action between the attacks, and administering freely when the first symptoms of a return appeared. Still this did not suffice. I kept the patient for some time steadily and fully under chloroform; and, while thoroughly anesthetized, she had a convulsion. This convinced me that something more than chloral and chloroform were required. At this juncture I summoned Dr. H. H. Wright, Professor of the Practice of Medicine in the Toronto School of Medicine. He came about noon, and it was decided to give her a hypodermic injection of morphia. One-third of a grain was inserted into the arm. Dr. Wright remained with me for an hour, but owing to urgent engagements had to leave. During the time that Dr. Wright was with me, she had one hard convulsion. After this they became much lighter and less frequent. The morphia had a decidedly good effect. At 3 P.M. the os was dilated to about the size of a fifty-cent piece, when I decided to use the forceps. In this I experienced more than usual difficulty. The child was delivered also with difficulty, as the pelvis was small. The labor over, the patient gained some apparent strength, and was sufficiently intelligent to answer me in whispers. I gave her some milk and water with brandy at short intervals. She soon became very restless, throwing the arms about and sighing, while the pulse grew weaker and weaker. I injected half a drachm of ether into the arm. Shortly after this she had another slight convulsion, from which she never rallied, but became more and more comatose till she expired. No urine was passed during my

stay in the house, and there was none in the bladder as tested by the catheter. The uterine contraction after delivery was not very good, for which, and to arrest bleeding as speedily as possible, hot water injections were employed. The pupils not being very much contracted by the morphia, the foot of the bedstead was raised some ten inches or more, to increase the supply of blood to the brain. The child was still-born.

Such is the history and termination of the above case. I had not long to wait for the next.

Sent for one day in haste to attend a young woman in her first confinement, and being informed that she was acting strangely, I lost no time in reaching the house. On my arrival I found that she had just recovered from a convulsion, but was still drowsy. Shortly after my arrival, she passed a few ounces of bloody urine. The face, feet, and hands were swollen. I waited quietly for half an hour or so, when the patient vomited with considerable straining. A little later there was a violent convulsion. This time I was determined not to trust to anesthetics and sedatives alone; and fearing that purgatives would prove too slow, I resorted to the hot pack. This was applied in the following simple manner: A cotton sheet was folded and spread upon the floor; upon this was spread an oil cloth, and over this again was laid four folds of flannel wrung out of very hot water. The whole was then passed under the patient and pinned round her body, extending from the axillæ to the great trochanters. It was not long till she was in a profuse perspiration. She became exceedingly thirsty. To allay this, she was ordered water as hot as she could drink it. In this way she continued sweating and drinking; the water taken in of course being pure, while the perspiration was loaded with impurities. After the application of the hot pack, which was changed from time to time to keep up its action, she had only one slight convulsion. One-quarter of a grain of morphia was then given hypodermically. The labor progressed well. The forceps were applied as soon as practicable, and the woman made a good recovery. In this case both mother and child were saved.

The outlook at the beginning was just as bad as in the first case. The convulsions were as violent and at as short intervals, the headache was intense, there was vomiting, the bowels were constipated as compared with the first, where they were relaxed, and both were primiparæ. In the first case the skin was dry, whereas in the second speedy, effectual, and continued diaphoresis was secured. I do not know how things would have been under some other line of treatment, but I know how they were under the method adopted. We all know how speedily soothing a warm bath is to a convulsed child, and a hot application as just described cannot fail to be so in the

adult. In more than one case of acute nephritis have I obtained good results from the hot pack. This was the only case in which I have used it in the nephritis of pregnancy; but should another occur in my hands, I would not hesitate to employ it, and, along with other means, would expect it to do me yeoman service in an hour when we are thankful for anything that can strengthen our hands.

A CASE OF SUB-MUCOUS FIBROID OF THE BODY OF THE
UTERUS, COMPLICATING PREGNANCY AND LABOR.

BY
P. BUDIN,

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THE existence of a uterine fibroid in a pregnant woman subjects her to a number of complications, amongst which we may mention hemorrhages and abnormal presentations.

Ordinarily, hemorrhages during pregnancy often determine or accompany miscarriage; in the case of fibroid tumors, however, as we are told by Lefour in his admirable thesis, pregnancy is frequently not interfered with. This point, he believes, should be specially emphasized, since it is a very important diagnostic element. "However profuse the hemorrhage, the physician should never despair of the continuation of the pregnancy." And this is not all; the same author recalls the fact that "the hemorrhage, occurring in cases of pregnancy complicated by uterine fibroids, is ordinarily met with at the periods which correspond to the menstrual epoch, and therefore, where it be not over excessive, may suggest menstruation."

As to abnormal presentations, Gueniot, Tarnier, Toloczinow, Susserott, Nauss, and Lefour, all agree as to their frequency. Lefour, having collated all the cases in which the presentation was noted, found that the vertex presented in 50.58 per cent of cases; the breech in 32.35 per cent; the shoulder in 16.66 per cent. The following case confirms the above statistical data, and proves, in addition, how difficult it

may be to diagnosticate the fibroid and how it complicates delivery.

On July 9th, 1883, at nine o'clock in the morning, Alfreda P. was brought to "La Charité" on a stretcher. Of good general health, she had first menstruated at twelve years of age, and since had always been regular; she had never had menorrhagia, and her menstrual periods had always lasted about the same number of days. Latterly she had noticed nothing unusual as regards her menstruation. With the exception of a slight enlargement of the abdomen, on which she laid no stress (seeing that often before the same thing had occurred after her meals), and infrequent attacks of vomiting a few months previously, her general health had been good. The morning of the day before she had had a few abdominal pains; in the afternoon she was surprised at a sensation of wetness, and thought she was involuntarily micturating. All that evening she lost water; during the night the abdominal pains increased. Owing to her great suffering, and from an unusual feeling about her genitals, a physician was called, and he found a hand at the vulva. She was astonished to find herself pregnant and in labor. She did not deny sexual intercourse, but had never suspected her pregnancy. She was immediately sent to the hospital.

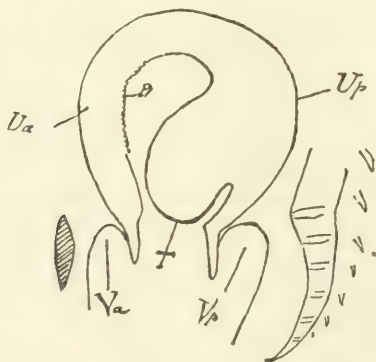
An examination determined the fact that the right arm was presenting, blue and swollen. The abdomen was not very large; the uterus was ovoid in shape, its long axis from above below, and from the right to the left; it was quite firmly contracted and constantly so. Nevertheless the fetal head could be felt to the right and superiorly.

On practising the touch, the finger entered with difficulty a vagina with narrow introitus and resisting walls. The external os was from 0.04 to 0.05 centimetres in diameter, and its relatively thin border was closely applied over the arm. The promontory of the shoulder was not accessible; the axillary depression pointed to the left, and the point of the finger could not quite reach the thoracic parietes and ribs. Auscultation revealed the fetal heart-sounds above and a little to the left of the umbilicus. The presentation then was the right shoulder, and the position right acromio-iliac. The body of the fetus, doubled on itself, was very high up, the head and the breech being about at the same level. The woman was put to bed and vaginal injections of corrosive sublimate (1:2,000) given every two hours. During the day the pains continued intense, and at five o'clock the following was the condition: The fore-arm, outside the genitals, was more swollen and cold, the uterus was the same in shape as in the morning, and strongly contracted. The fetal heart was still to be heard, though at times very slow; the external os was nearly dilated. On pushing the finger deeply, the lower segment of the uterus was found closely encircling the fetus, which was still very high up. Although the life of the fetus was seriously compromised, it seemed a duty to endeavor to save it, and it was resolved

to chloroform the woman in the hope of relaxing the uterus and performing version.

At 5.45 p.m., the pulse was 92, and the temperature 37.2°. When the anesthesia had been complete for some time, a sling having been applied to the procident arm, the right hand was introduced into the uterine cavity at a moment of relaxation. A knee was seized, but a tetanic contraction absolutely forbade any further action on the part of the operator. This contraction lasted seven minutes; the thorax was felt to lift as though the infant were endeavoring to breathe, the half-paralyzed hand was withdrawn. The chloroform was continued, and about ten minutes afterwards, during slight relaxation, the hand was again introduced, a knee was seized and a foot brought down to the vulva. As it was impossible to bring the foot outside the vulva, a sling was applied to it. From this time on there occurred such uterine contraction, such a tetanic state in fact, as to render it impossible to complete the version.

The woman was allowed to come out from under the influence of the anesthetic. The pulse was 104, the temperature 37.4°. A short time before regaining consciousness and for a little while after, she vomited, as the result of the chloroform. The fetal heart could no longer be heard. At 7½ p.m., the woman was placed in a bath and remained in it for one hour, during which time the pains were intense. On removal from the bath she was given 0.01 centigrams of the chlorhydrate of morphia subcutaneously. At 10.45 p.m. the temperature was 36.8; the uterine contractions were still tetanic; vaginal examination gave her much pain, though the junction of the arm and shoulder could



be reached. Embryotomy was decided on. Decapitation was impossible, owing to the uterine contraction, the height of the head, and the impossibility of reaching the neck. It was determined, then, to separate the arm at its junction with the shoulder and attempt forced version by pulling on the foot which lay at the os. The woman was again anesthetized, and the arm was

soon separated by means of P. Dubois' scissors. Repeated traction was then made on the foot, to which a sling had been applied, but the breech would not descend. During an interval in the contraction, the shoulder at the superior strait was pushed up, traction again made on the foot, the breech was brought down and little by little the whole fetus extracted. It was a male and weighed 2,500 grams. The placenta soon followed, its border appearing at the vulva; slight uterine expression sufficed to deliver it.

On vaginal examination a puzzling condition of affairs was found. A careful examination revealed the following: The posterior vaginal cul-de-sac (Fig. Vp) not obliterated, the same of the anterior cul-de-sac (Va). There was a laceration of the cervix on the left, not quite down to the vagina. The finger within the uterus felt on its anterior wall (Ua)—of normal thickness—the inequalities marking the placental site (Pl). Posteriorly, existed a globular mass, the size of the fist (T). This mass was adherent to the posterior wall of the uterus (Up), not distinguishable from it, and rounding out down to the internal os. Between the lower part of the mass and the posterior wall of the uterus there existed a groove—a species of pouch.

An intrauterine injection of a litre of a solution of corrosive sublimate (1:2,000) was given, and the woman put to bed. Temperature 37.8°. Every two hours vaginal injections were given. The patient slept calmly. At 6 A.M., no pain, temperature 36.8°, pulse 90.

Convalescence was normal, without either hemorrhage or fetid lochia. For three days vaginal injections of the sublimate solution were given every two hours, reduced to six, and finally to four in the twenty-four hours.

Four days after delivery, the finger could still be passed to the uterine cavity; the tumor projected to the internal os; the finger could scarcely be insinuated between it and the anterior uterine wall. Posteriorly, the groove could no longer be detected, but the tumor, closely applied to the posterior wall of organ and intimately connected with it was of such size, that its antero-posterior diameter was nearly equal to the vertical diameter of the uterus. By rectal touch the tumor could be felt as an abnormal projection backwards. On the twelfth day a vaginal examination was again made, and the os was shut against the examining finger. It was with difficulty Alfreda P. could be kept in bed, and she left the hospital in excellent health.

Remarks.—1. The existence of this submucous fibroid, diagnosticated by the intrauterine touch only after delivery, allows us to understand why it was the patient had no knowledge of her pregnancy. The persistence of menstruation, or rather the occurrence of hemorrhages during the course of her pregnancy, explains her lack of knowledge; for though, on

second thought, the sanguineous flow did not occur at as regular intervals as formerly, she laid no particular stress on it. She had had scarcely any sympathetic disturbances, except infrequent vomiting; she had noticed nothing out of the way with her breasts or other organs. She had suffered occasionally after meals from tympanites, but this had occurred before, and to it she laid the increase in size of her abdomen. Neither had she interpreted aright the first fetal movements.

2. The presence of a large submucous fibroid at the level of the inferior segment of the uterine body gives also a sufficient explanation why there had been difficulty in the normal accommodation, in fixation of the head, and presentation of the shoulder. It was the tumor which kept the shoulder as well as the fetal body so high up; it was it also, without doubt, which, added to the contraction of the uterus, prevented the evolution of the fetus after the foot had been sized. The administration of chloroform to the surgical degree was powerless over the tetanic contraction of the uterus. Since the body of the fetus remained ever high up, the neck was not accessible, whence decapitation was impossible and branchotomy with the scissors, followed by forced version, had to be resorted to.

REPEATED PREGNANCY IN A UTERUS BICORNIS.

BY

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New York.

I ATTENDED Mrs. F. in confinement for the first time in the latter part of 1878. At my first examination, I found a shoulder presentation, a fully-dilated cervix, and the liquor amnii evacuated. The child was turned and extracted, and both mother and child did well. I afterwards got the following history: Mrs. F. was then thirty-five years old, and this was her seventh confinement. Four children were born in head presentations without any trouble, except that in the second confinement a part of the placenta was left behind and had to be removed by a physician,

after severe flooding, and she had afterwards some acute trouble. The fifth child was a transverse presentation, the child dying during delivery. She does not know the position of the child at the sixth confinement, as she was delivered under chloroform. The seventh confinement I have described above. On April 31st; 1881, I was called again to deliver her, and diagnosed, on external examination, twin-pregnancy, one child lying in the vertical, the other in the transverse diameter. On vaginal examination, I found the os dilated to a diameter of about one inch, head presenting. Without much trouble, the child was delivered, and the cord tied. On palpation over the abdomen, I found the second child in transverse position, the head to the left, the extremities to the right. On passing my hand through the vagina and the dilated os into the uterus, I came upon a large empty cavity, which contained nothing but the placenta. Retracing my steps along the cord, I produced uterine contractions, by placing my hand on the abdomen, and found my fingers entangled in a loose and unruptured sac of membranes. Repeating the manœuvre, it became clear that I had to the left a cavity which had expelled the child, and contained the placenta not yet detached. To the right of this cavity, and divided by what appeared to the touch quite a thick membrane, there was another cavity containing the other child in as yet unruptured membranes, and in a transverse position. During this examination, the loose sac of membranes (quite distinct from the dividing membrane) became more distended, and ruptured spontaneously, revealing a shoulder presentation. It was quite difficult to reach the legs of the fetus, and I had to place the patient across the bed to finish the delivery by version and extraction. The child was dead, and probably had been so for some hours. During the delivery of the second child, the other cavity remained uncontracted, while my hand entered it by mistake, in the attempt to reach the feet; but, after the removal of the second child, one strong pain set in and expelled two entirely separate placentæ, which were in no way connected; each with its cord inserted in the centre. The uterus contracted well. I made no further examination at that time, intending to do so after her getting-up. On May 30th, she called on me, and I found the following condition: The uterus well contracted; the cervix about one inch long; the os closed. On introducing a uterine sound, I observed that it entered the cavity of the uterus in the middle line for about one and a half inches, and then turned to the right side. I now introduced a second sound and, turning it to the left, both sounds could be felt over the symphysis, one a little to the right of the other. I then brought the handles of the

sounds close together, and withdrew one slowly, and, still keeping both close together, one inch and a half from the os externum, I distinctly felt this sound striking the other. I recently had a further opportunity of verifying the diagnosis. On November 8th, 1883, I was called again to deliver the lady. I made out, by external examination, a transverse position, head to the left, body to the right. The child was delivered by version, and did well. After expulsion of the child, the uterus contracted firmly, but there was on the left an appendix, or rather a part of the uterus itself, of the size of a duck's egg, which was soft and did not appear to participate in the action of the rest of the organ. To ascertain the cause of this condition, which was unaccompanied by any flooding, I passed my entire hand into the cavity and verified by bimanual examination the existence of a cul-de-sac, which had not participated in this pregnancy. This cul-de-sac was undoubtedly the second horn of the uterus, which had in the last pregnancy contained one of the twin embryos.

BATTEY'S OPERATION, PERFORMED IN A CASE OF MALFORMATION OF THE GENERATIVE ORGANS.

BY

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Philadelphia.

SUSIE REGAN, born in Wales, domestic, aged eighteen, arrived in America September, 1882; entered service in a family, remaining with them throughout her life, coming under my observation last February, when she entered my house with the family in whose employ she was. Her height was about five feet four inches, she was well built, had a ruddy complexion, black hair, and dark eyes; form well developed for her age; good tempered, and willing to work. At first she seemed to stumble, and fell down-stairs so frequently that I wondered if it was due to stupidity, or another and perhaps better reason. It was some time before I was able to ascertain the true state of affairs, the girl being reticent in speaking of herself. I found she had never menstruated, that at times she suffered from headache, dizziness and pain in her back. Her falling down-stairs was often due to a dark cloud coming over her eyes, which resulted in her losing her footing; she suffered from a habit of constipation, which I endeavored to correct by pills of aloes and myrrh; and I also gave her bromide of sodium, which seemed to relieve her for a short time. Being

away from home a few days during the first part of May, I returned, and found her in bed, helpless, and suffering from an aggravation of the symptoms. I then made my first physical examination, for she had had no sign of menstruation so far, and found no vagina, and by a rectal exploration could discover no uterus.

I explained to her the nature of her deformity, and asked an examination under ether, with other medical aids; she consented, and on the following morning she was etherized in the presence of Drs. Scott and Peterson. We found a band, which we supposed to be the broad ligament, extending across the pelvis, and thickened above the bladder. On the left side we could feel an ovary, but none on the right side. It was decided to give the patient every advantage from medical treatment before resorting to operative procedures, watching carefully the course of events. By this time her feet pained her very much if she stood on them for any length of time, while her head seemed to ache incessantly, unless for half an hour after thirty grains of bromide of sodium had been taken. Local blood-letting was resorted to, after which she seemed to stumble less frequently, and for the space of ten days, being without help, did the work of the house alone. In the first week of June, however, the same symptoms returned with renewed activity, and the patient entreated to have something done which would make her either well or end her misery; for by this time she was helpless, and unable to walk about without staggering. I again etherized her in the presence of Drs. Harris, Smith, Scott, and Broomall, who all found the condition previously described, and in addition a small body on the right side, compared to the size of a coffee grain, which they thought might be an ovary. Oöphorectomy was decided upon as promising the only relief.

I also interested Dr. Wm. Goodell in the case, who very kindly offered to examine her with me, and then agreed to assist me in the operation, having concluded that it was the proper course to pursue; and having also obtained the sanction for it from the patient's father and aunt, the day was fixed for June 28th, 1883. The day for the operation was bright and clear, patient composed and in remarkably good spirits.

At 1.30 P.M., assisted by Drs. Wm. Goodell, A. V. Scott, and C. Peterson, and in the presence of Drs. R. P. Harris, A. H. Smith, A. E. Broomall, A. E. Tyng, and H. T. Croasdale, I made an incision about five inches long, commencing one and a half inches below the umbilicus, cutting down in the median line, and entering the peritoneal cavity. The abdominal walls being tense, it was somewhat difficult to find the left ovary; when found, however, it was about the size of an almond, ellipsoid in form, with a well-developed Fallopian tube; and about one and a half inches to the right was what appeared to be the body of a small uterus, which, however, by subsequent examination, proved to be only the left cornu. After having firmly grasped and ligated the left ovary and Fallopian tube, search was

made for the right ovary, by following the broad ligament around to its pelvic attachment, but nothing could be found to correspond to this body, the thickened ligament becoming more cord-like as it neared its attachment; and it was concluded there was none on the right side. The question arose as to the advisability of removing that which appeared to be a rudimentary uterus, and Dr. Goodell, thinking that possibly there might be some ovarian tissue in the mass, it was deemed best to ligate and remove it also. The pedicle was then dropped into the pelvic cavity, and the lips of the abdominal wound brought together with ten silk stitches; the operation was performed and wound dressed by the antiseptic method.

The following is the record kept after the operation:

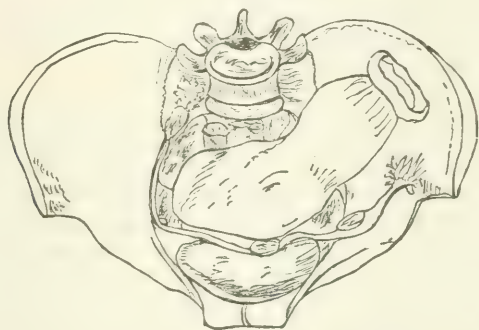
	DATE.	TIME.	TEMP.	PULSE	RESP.
June	28th.....	7 P.M.	99.	134	28
	28th.....	11 P.M.	99.8	137	34
	29th.....	3 A.M.	101.5	139	30
	29th.....	7 A.M.	101.	129	22
	29th.....	12 A.M.	101.5	119	24
	29th.....	4 P.M.	103.4	150	24
	29th.....	8 P.M.	104.	176	29
	30th.....	12 M.	102.6	144	32
	30th.....	8 A.M.	102.	140	34
	30th.....	12 M.	104.	160	34
	30th.....	4 P.M.	105.6	180	38
	30th.....	6 P.M.	106.2	184	44
	30th.....	7 P.M.	107.2	200	37

Treatment (Resumé).—Patient was given a suppository of one-half grain of ext. of opium every four hours, which kept her quiet, until the temperature rose to 103°, when whiskey was administered in teaspoonful doses every hour. As it reached higher, an enema of bisulphate of quinine was given, which was retained only half an hour and then expelled; after which I tried suppositories with a like result. Sponge baths and ice bags to the head seemed to reduce the temperature, temporarily. At 7.25 P.M. patient died; the thermometer reached 108° Fah. just prior to her death.

July 2d, autopsy, 4 P.M., forty-five hours after death. An Egyptian embalming fluid had been injected into the abdominal cavity two and a half hours after death. Body and organs well preserved; entire track of the abdominal wound healing by first intention, stitches firm; embalming fluid clear in color as it escaped. Lungs, heart and liver normal; spleen slightly softened. Kidneys—left kidney normal in position, enlarged and highly congested; the lower edge of the right kidney found lying one and a half inches above the promontory of the sacrum, and extending upward upon the centre of the spinal column, with its pelvic side turned toward the left iliac region; lobulated but normal in color; a half teaspoonful of pus was found in the tubules near the pelvis of the kidney.

Omentum normal in color; no pus present; peritoneum injected and darkened in color around the region of the wound in the pelvic cavity, but no sign of blood present; the distal end of the pedicle congested.

On the right, lying a half inch above the promontory of the sacrum, and midway between the lumbo-sacral and sacro-iliac articulations, was found the fimbriated extremity of a Fallopian tube, the tube being about two and a half inches long, and ending blindly in the broad ligament. From the end of the Fallopian tube, a band of connective tissue, inclosed in a fold of peritoneum, extended about four inches in length, and then passed into a thick mass of muscular tissue, which subsequently proved to be uterine. In the folds, and on the back of the broad ligament, corresponding to the position of the parovarium, there was an irregularly shaped, somewhat elevated collection of tissue, which had been the seat of cystic degeneration; the tissue on the



border presented a dark, pigmented appearance, and was of a soft, jelly-like consistence, which grew more firm towards the centre. This mass was about one-half inch wide, and one and a half inches long, with about a quarter of an inch elevation of surface; this was the small body which was mistaken for a rudimentary ovary, on that side, in the physical examination during life. Near the fimbriated extremity of the Fallopian tube, and extending along the course of the oviduct, was a cyst about the size of a small almond, containing a clear liquid.

On the left, extending from the median line, a mass of tissue was found, about four inches in length and one-half inch in diameter, covered on all sides by peritoneum. On section it proved to be composed almost exclusively of smooth muscular tissue resembling that of the uterus; this tissue ended abruptly, and a quarter of an inch from its extremity was found a transparent band of tissue, the result of the ligature applied antemortem.

Results of the microscopical examination made by Dr. Henry Wile: Sections taken from what has heretofore been described as the Fallopian tube on the right side, showed the characteristic

structure of that organ. An external serous layer; then a layer of unstriated muscular tissue, a loose areolar submucosa, and finally a mucosa thrown into longitudinal folds; these folds were rather deep and capped with columnar ciliated epithelium.

The collection of tissue described above as existing on the posterior surface of the broad ligament, examined microscopically, proved to be composed of a dense reticulum of embryonic connective tissue, containing many small, round lymphoid cells; this reticulum seemed to be without any definite arrangement, and towards the surface passed into a species of tissue which was very much allied to a myxomatous variety. Here and there scattered about in the centre and immediate vicinity of the specimen, were seen several tortuous narrow tubules of various lengths, which, on careful examination, showed a lining of columnar epithelium, resting upon a very delicate basement membrane, not unlike the uriniferous tubules of the kidney. There were also seen here and there variously sized collections of pigment which were in some places circumscribed, and in others more diffused.

The structure was supplied for the most part with fine capillaries, which were all in a state of congestion.

Section taken from the mass of tissue described above as uterus, was found to be composed of an external serous investment, under which was found tissue made up exclusively of smooth muscular fibres, which were arranged loosely and interlaced without any apparent regularity; bundles of fibres were seen running longitudinally, others obliquely, and many had been cut transversely. The structure altogether was exceedingly vascular, the arteries had unusually thick walls, the veins were large, and had an irregularly outlined lumen; the general appearances, together with the foregoing observations, left no doubt as to the tissue, namely, uterine.

Microscopic examination of the tissues removed during operation showed one part to be composed of uterine tissue, the other part Fallopian tube of about one and a half inches, together with a small piece of broad ligament with an ovary attached, which was ellipsoid in form, about one and a quarter of an inch in its long diameter, and about one inch in its short diameter.

The ovary was surrounded by a capsule, composed of fibrous connective tissue, which was thickened and in certain places seen to send prolongations into the cortical substance of the ovary.

The medullary portion was composed of a loose areolar tissue, in the stroma of which, running from the hilus of the ovary, were numerous fine tortuous capillaries, which branched out in all directions; between the medullary portion and the cortical layer was seen a layer of myxomatous-like connective tissue; the cortical substance was made up of connective tissue having a very compact structure, the compactness increasing as the surface of the ovary was approached. In the myxomatous-like stratum was found an irregularly round cellular body containing a cluster of small, round pigmented inflammatory cells; other parts of this body were found to be a seat of diffused pigmentation; the ap-

pearances indicated strongly that this body had been the seat of some inflammatory process. Passing the cortical substance, several multinuclear granular cellular bodies appeared, which were oval in shape, decreasing in size and proportion as they neared the surface. These bodies were epithelial structures occupying Graafian follicles, which, unable to penetrate the dense structure of the cortical substance, gradually atrophied. About the centre of the cortical substance was a collection of epithelial cells, surrounded by a dense, concentric arrangement of connective tissue; the epithelial cells were loosely arranged and had nuclei; thought to be an ovum.

THE DELIVERY OF THE SECOND FETUS IN LABOR WITH TWINS.

BY
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IN a majority of cases of labor with twins, the second fetus is born naturally, and shortly after the birth of the first. Indeed Spiegelberg compared twin labor to two single labors following quickly on one another; of which the second is much shorter than the first, because the period of dilatation is common to both.

And yet in a not inconsiderable number of cases complications arise after the birth of the first fetus which seriously endanger the life of the second and subject the mother to increased risks. In fact Spiegelberg himself said¹ that in labor with twins the prognosis was much more unfavorable for the children than in single labors; not only on account of their smallness, but because of the more frequent anomalies of presentation, and therefore more frequent artificial delivery: especially unfavorable, he pointed out, is the prognosis, for the second child, on account of the more frequent occurrence of transverse positions. Since more than 25 per cent of twin pregnancies terminate prematurely,² it is not unexpected that the mortality in early infancy should be large; nor is it surprising that the mother's convalescence should often be protracted, on account of the greater demand on her organism for the

¹ Geburtshülfe, 2te Aufl., S. 195.

² Reuss gives 26.5%; Spiegelberg, 27.5%.

simultaneous development of two offspring. It is to be regretted, however, that owing to what I believe to be preventable causes connected with labor, the life of the second fetus is not infrequently compromised or lost, and the maternal convalescence greatly retarded. Supposing now the first fetus to have been delivered, and excluding, therefore, complications which might arise from the simultaneous presentation of both children, I propose to consider the delivery of the second fetus, the difficulties which may arise, and how best to prevent their occurrence.

The chief cause of dystocia from the second fetus arises from abnormal presentation. In 1,144 cases, Spiegelberg¹ found that in 121, that is in 10.5 per cent, the second fetus lay in transverse position. Carl Braun² places the proportion even higher, namely, at 12 per cent. Of 208 cases analyzed by Hecker, the second fetus lay in oblique or transverse position in 34 instances, or 16 per cent. In single fetation transverse position occurs in only 0.7 per cent of all cases. This faulty position is in the great majority of instances secondary; that is to say, the second fetus, occupying originally a longitudinal position, owing either to the sudden rupture of its membranes, to the greater roominess of the uterine cavity after the birth of the first child, or to both these causes, assumes an oblique or transverse position, and presents the shoulder, arm, or trunk. And these presentations entail in most cases artificial delivery, with more or less danger to the fetus and to the mother, according as the interference is skilful and timely, or the reverse.

In the beginning of single labors in which the head presents, if the pelvis and fetal head are not disproportionate, the head rests either at the brim or within it, in a state of moderate flexion; and the effect of the first expulsive pains acting on the head through the vertebral column and aided by the relatively greater resistance on the longer, sincipital arm of the cephalic lever is to increase this flexion. Excluding from present consideration the effect of pelvic contraction and marked dolicho-cephalism in thwarting this mechanism, it may be said that the head descends into the pelvis well flexed provided the uterine obliquity is not extreme; that is to say, provided the

¹ Op. cit., p. 192.

² Lehrbuch der gesammten Gynäkologie, 2te Aufl., S. 201.

fetal spinal column does not deviate from the axis of the brim so far and in such a direction as to transmit the force of the uterine contraction to the sinciput, and thus produce extension. Now in labor with twins, when the second fetus presents the head, the first fetus being born, two circumstances may lead to a malpresentation which it is needless to say is a most undesirable one. In the first place, the second fetus may be retained for a time in its intact membranes somewhat above the brim: a sudden rupture of the membranes may cause the chin to drop away, as it were, from the sternum, and thus the head enters the pelvic inlet more or less extended: at the same time the cord may be swept into the pelvis by the sudden gush of amniotic fluid. Secondly, owing to the greater roominess of the uterine cavity and the laxity of the uterine walls, the fetus may fail to maintain its longitudinal position, and the breech may fall to the right or left, according to the mother's position. If the breech deviates towards the chin, any extension of the head that may have occurred will be in a measure corrected, and the labor may terminate normally: if, on the contrary, it deviates toward the occiput, the extension will be increased, the force of the uterine contraction will be conducted to the sinciput, and the brow or face will descend as the presenting part.

Another source of possible danger to the second fetus arises from the fact that in a very large proportion of cases the pelvic extremity presents. The proportion of pelvic presentations in all cases is commonly placed at three per cent: if cases of premature and plural births are excluded from the calculation, the proportion falls as low as 1.5 per cent.¹ In labor with twins, however, it is found that the second fetus presents the breech, foot, or knee in over 40 per cent of all cases. Allowing one-fourth of the cases to be prematurely delivered, there would remain 30 per cent of cases at full term in which the second fetus would present the pelvic extremity. In a large proportion of cases, therefore, unless the second fetus be very small, it would be subjected to danger from pressure on its cord; especially since the second fetus is generally the larger,² and does not, therefore, pass through an entirely dilated canal. More-

¹ Conf. Spiegelberg, *op. cit.*, § 162; and Schröder, *Geburtshülfe*, 5te Aufl., § 170, Anm.

² This was the experience of Spiegelberg; *vide op. cit.*, p. 188.

over, if the fetus is small and there is no danger from compression of the cord when the head passes the brim, the very smallness of the presenting part and its imperfect adjustment to the pelvic inlet increase the danger of funic prolapse.

In recapitulation it may be stated that the second fetus is liable to assume certain malpositions after the birth of the first child; that these malpositions, varying from the slightly oblique to the completely transverse, may result in the presentation of the shoulder, arm, or trunk, or of both hands and feet; further, that even if the head presents at the superior strait, the decided lateral deviation of the fetal trunk possible in these cases may cause extension of the head and a consequent presentation of the brow. It is also to be noticed that in a large proportion of cases the second fetus presents the breech; and that although this presentation may not make the labor more difficult for the mother, it increases the danger for the child. Finally, it is to be remembered that in presentations of the trunk, shoulder, brow, or pelvic extremity, there is greater danger of prolapse of the cord, since these parts do not accurately adapt themselves to the pelvic brim. With a view to the prevention of these malpositions and complications, I suggest the following method of treatment.

Immediately after the birth of the first child, the mother should be bidden to lie upon her back, the funis tied with two ligatures and cut between them, and the infant delivered to an attendant. On no account should an attempt be made to remove the placenta. Without delay examination should then be made to ascertain the position of the second fetus. If it is found that either the breech or a well-flexed head has normally engaged in the superior strait, it is probable that the labor will be speedily and naturally completed: it is only necessary, if the breech presents, to exercise the care usually required in such cases. But if, as is commonly the case, the tired uterine muscle does not at once contract, and the membranes remain unruptured, the second fetus will not be found within easy reach of the examining finger, but will remain at or somewhat above the pelvic brim. Under these circumstances, in order to examine with accuracy and to be prepared to deal with any complication that may threaten, a hand should be passed into the vagina: since this canal has just been dilated

and its sensitiveness obtunded by the passage of the first fetus, this manœuvre can cause no pain. While one hand is thus occupied in vaginal examination, the other hand should support the fundus uteri and prevent the womb from assuming a marked degree of lateral obliquity. If perchance it should be found that a malposition already exists, bipolar version should be performed to restore the fetus to a longitudinal position, and external pressure should be properly exerted to maintain it therein. At the same time, if the cord is in advance, it should be pushed above the presenting part before the membranes are ruptured.

If now the fetal heart is heard, and the mother's condition does not demand immediate delivery, the patient should be allowed to rest. At the end of half an hour or more, if pains have not recurred spontaneously before that time, abdominal frictions and, if necessary, rupture of the membranes should be resorted to to stimulate uterine contractions. If the head occupies the brim, with the hand already in the vagina perfect flexion can be secured: if the pelvic extremity presents, or if the membranes rupture suddenly before the presenting part has occluded the superior strait, the hand is ready to prevent a prolapse of the cord. In other words, it may be briefly stated that the proposed treatment embraces the intelligent use of one hand upon the abdomen to support the uterus and maintain its contents in longitudinal position, and of the other hand in the vagina to prevent prolapse of the funis, and to adjust properly the presenting part to the pelvic inlet.

I append brief notes of three cases germane to the subject of this paper:

CASE I.—A multipara gave birth with easy labor to a fetus weighing about four pounds: the midwife in attendance then discovered that the uterus contained another fetus, and, as speedy delivery did not follow, I was sent for. I found the second fetus in left transverse position presenting both feet and both hands: the liquor amnii had drained away, and the uterus had contracted firmly upon its contents. With some difficulty I turned and delivered a living child weighing four pounds.

It is probable that in this case the second fetus originally occupied a longitudinal position and presented the breech, and that in the interval of uterine inactivity the change of position

occurred. The case shows that the commonly-received opinion that the small fetus offers no obstacle to labor is erroneous.

CASE II.—A multipara, aged twenty-four, after an easy labor of five hours, bore a female child weighing six and one-half pounds. It was then discovered to be a case of labor with twins. One hour and a half later the membranes of the second fetus ruptured, and the pains became so severe that ether was given. The head descended into the cavity, but would not advance farther; and about six hours after the birth of the first child I was sent for by the student in charge of the case. I found the head low in the pelvic cavity, O. L. A., the brow presenting. The woman being fully anesthetized, I tried, both with my hand and with one blade of the forceps used as a vectis, to bring down the occiput and thus produce flexion of the head. Failing in this attempt, I applied forceps, hoping by their means to correct the malposition, but without success. I now had recourse to podalic version, and with great difficulty succeeded in delivering a still-born male infant weighing eight pounds.

In this case the pelvis was not contracted, and the fetus was not dolicho-cephalic: it is probable, therefore, that the extension of the head was caused either by the sudden rupture of the membranes before the head entered the brim, or by undue uterine obliquity, or by a combination of both these factors.

CASE III.—A secundipara, aged twenty-eight, was thought to be with twins before labor began. Four hours and twenty minutes after her initial pains, she was delivered of a female infant weighing seven pounds, and it was then evident by inspection that the uterus contained another fetus. Passing my hand into the vagina, I felt the head presenting, O. L. A., the hands extended over the face, and the membranes unruptured. As the head was still above the superior strait, I conceived that a sudden gush of liquor amnii might cause a further extension of the arms, and perhaps a prolapse of the cord, before the head could engage in the brim: I therefore kept my hand in position to correct these mishaps, should they occur. Meanwhile, the patient lying on her back, the uterus was maintained in the median line. The membranes soon ruptured, and head, hands, and cord descended and occupied the superior strait. I at once pushed up the hands and cord, and held the latter on my fingers' tips above the brim until the next pain caused the head to descend and prevent further prolapse. The fetal heart was still beating, as, indeed, the cord had been subjected to no pressure, and the second stage was completed naturally in forty minutes: the child was male and weighed nine pounds, the combined weight of the twins being sixteen pounds.

Since this paper was written, a case has occurred in my experience which is of interest in this connection:

I had induced labor in a case of acute hydramnios at about the fifth month: twin pregnancy had been suspected, but not diagnosed. After the birth of a fetus weighing one pound two ounces, a second fetus appeared at the brim presenting O. R. A. In the absence of uterine contractions, I kept my finger on the presenting part: soon the head disappeared and a shoulder presented. As the child was small and not viable, and as I believed that no harm could befall the woman by my non-action, I did not rectify the malposition, but waited to see what would happen. Very soon strong pains ensued, the shoulder was crowded down, and in thirty minutes from the birth of the first fetus the second was born by spontaneous evolution.

The change of position, here shown under my finger, is what might naturally have been expected in a uterus which had been much distended by dropsy of the amnion, and which did not at once contract upon the second fetus. But a very similar condition exists in twin labor at full term, and similar changes of position are quite as likely to occur, unless prevented by appropriate treatment.

A REVIEW OF THE OPERATION OF GASTROTOMY FOR MYO-FIBROMATA OF THE UTERUS.
WITH COMPLETE STATISTICAL TABLES.

BY
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(Continued from page 49.)

Remarks upon German Table.—In Dieffenbach's case, of 1826, there is nothing to show that the tumor was uterine. The post-mortem examination in Martin's case, of August 7th, 1867, showed hyperplasia and new growth of the uterus, chronic peritonitis, dropsy of Fallopian tubes, more than three litres of blood in abdomen, broad ligaments thickened and adherent. Pamezzi refers to the "Deutsche Klinik" as "Clinica Te-desca."

In addition to the cases appearing in the tables, Peruzzi reports two cases of G. Simon, of Rostock, both of which died, and one by Walther, of Offenbach, with recovery. Gustav Simon reports a case (operator's name not given) which will be found in Scanzoni's "Beiträge zur Geb.," III., 1858, p. 133, which ended fatally

Number.	Name of operator.	Date and pl. of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision.	Anæsthesia.	Character of tumor and complications.
1	Prof. E. Albert.	Innsbruck Jun. 11, '78	Com.	Tumor...	Long.	Pedic. fibroid; pedicle 6 cm. long, 4 cm. br.
2	do.	Innsbruck Oct. 14, '77	"	Sup. vag. ut. and r'gt ova.	"	Fibroid; w. 3,306 gm., right ovary enlarged, adhesions.
3	Colleag. of Billroth, of Zurich	1866.	"	Uterus...	Edematous fibroma of uterus.
4	Do. of Vien	1870?	Fibroid of uterus. ...
5	Th. Billroth.	Vien. Nov. 14, '74.	Com.	Ut., tubes, and ova's.	Long.	Multiple ut. fibroids, interst.
6	do.	Vien. May 23, '75.	"	Uter. and ovaries.	"	Multiple ut. fibromata, interst.
7	do.	Vien. Nov. 1, '75.	"	Sup. vag. ut. and left ova'y	$\frac{3}{4}$ h.	"	Multiple ut. fibroids; w. 34 lbs.
8	do.	Vien. Nov. 2, '78.	"	Sup. vag. ut. and r'gt ova.	2 h's	"	A.C.E mixture.	Fibro-myoma, int'rst. cystic.
9	do.	Vien. July 14, '78.	"	Tumor...	"	Spray	Cysto-myoma; weight 8 kilogrammes pediculated.
10	Czerny ...	Heid'lb'rg (6 cases)	"	Sup. vag. ut. remov in 6 cases	"	"	Fibro-myoma.....
11	J. F. Diefenbach.	Berlin, '26?	Not c.	"See Remarks."	"	Hard tumor attached to uterus and bladder.
12	V. Dumreicher.	March 8, '79.	Com.	Ut., ov., and tub's	1 h.	"	Ut. myo-fibromata...
13	Freund ...	Breslau (2 cases).
14	C. Gussenbauer.	Prag. Jun. 4, '79.	Com.	Sup. vag. uter. and r'gtappen	Long.	Anti-sepsis	Multiple uterine fibromata; $3\frac{1}{2}$ kilo.
15	do.	Prag. Nov. 3, '80.	"	Sup. vag. uterus.	2 h's	"	do.	Long fibroma of ut., neck; interstitial.
16	do.	Prag. June 4, '80.	"	Uter. and append.	$2\frac{1}{2}$ h	"	do.	Cystomyoma of ut., $2\frac{1}{2}$ kilo.; adhesions.
17	do.	Prag. May 14, '79.	"	Uter. and tumor.	do.	Interst. pedicle, fibromyoma of uterus.
18	do.	Prag. Nov. 25, '79.	"	Sup. vag. uterus.	$2\frac{1}{2}$ h	Long.	do.	Fibroma of ut.; w. $12\frac{1}{2}$ kilo.
19	do.	Prag. Feb. 17, '80.	"	Sup. vag. uterus.	1 h.	"	do.	Cysto-myoma of uterus.
20	Heer.....	March 30, '70	"	Tumor...	2 h's	"	Fibro-cyst, pediculated, weight 643 grammes
21	A. Hegar.	Freiburg. Nov. 8, '75	"	Tum., ut., and ov.	"	Pedic. and inters. fibro-myoma, adhesions.

M A N Y.

Operation.	Age, Married or single.	Previous operat'n.	Condition of patient	Result and Cause of Death.	Reporter and when reported.
Ecraseur, stump in wound.	45	Recovery...	Wien. Med. Presse, xx., 1879, p. 47.
Ecraseur, stump in wound.	53, M...	Death, perit.	Wien. Med. Presse, xx., 1879, p. 45.
Ligatures en masse	21, S...	" 2d day, perit.	Wien. Med. Woch., xxvi., p. 2.
.....	Death, perit.	" " " " p. 3.
Morcelement, stump outside.	38, S...	" 3d day, perit.	" " " " p. 26.
Clamp drainage of Douglas' pouch.	31, S...	Death, 28 h'r's. coll.	" " " " 1876, p. 27.
Double lig., drainage thro' vagina and abd. wound.	19, S...	Recovery...	Wien. Med. Woch., xxvi., 1876, p. 28.
2 drains in Douglas' pouch.	39	"	Wien. Med. Woch., xxix., 1879, p. 543.
2 drains in Douglas' pouch.	55, M...	"	Wien. Med. Woch., xxix., 1879, p. 514.
4 times stump everted, once Schröder's plan, 1 lig.	2 rec., 4 died	Wien. Med. Presse, xx., 1879, p. 1265; Ob. J., London, xiii., 1880, p. 216.
Tapped.....	44, M...	Recovery...	Rust's Mag. f. d. ges. Heilk., xxv., 1826, p. 349.
Stump outside....	40, S...	"	Wien. Med. Presse, xx., 1879, p. 379; Med. Soc. Vienna, March 14th, 1879.
.....	1 recovered.	Trans. Ger. Gynec. Soc., Sep., 12th and 13th, 1878: AM. J. OBS., xii., 1879, p. 199.
Stump in wound and drain., Cintrat's serre-noeud.	34, M...	Septic perit., death 2d d.	Prag. Med. Woch., vi., 1881, p. 205.
Stump in wound..	38, M...	Recovery...	Prag. Med. Woch., vi., 1881, pp. 196 and 204.
Stump in wound, drainage.	50, M...	Death 3d d.	Prag. Med. Woch., vi., 1881, pp. 186 and 195.
Stump extraperit.	39, S...	Recovery...	Prag. Med. Woch., vi., 1881, p. 165.
"	50, M...	Death 6th d.	Prag. Med. Woch., vi., 1881, p. 174.
"	33, M...	Recovery...	Prag. Med. Woch., vi., 1881, p. 184.
Ligatures....	48, M...	"	Inaug. Diss. Zürich, 1874; Pozzi, p. 85.
Stump in angle....	33, M...	"	Berlin. Klin. Woch., xiii., 1876, pp. 154 and 168. Ob. J. London, iv., 1876-7, p. 199.

Number.	Name of operator.	Date and pl. of operation.	Completed or not.	What was removed.	Duration of operation.	Length of incision.	Antiseptics.	Character of tumor and complications.
22	A. Hegar.	Freiburg, Nov. 8, '75	Com.	Sup. v. a. ut. & ov	1 1/4 h	Long	Inters. vascu. fibroid, weight 4 kilo.
23	do.	Freiburg, Dec. 27, '76	do.	Ut. and ov
24	do.	Freiburg, Aug. 1, '79	do.	do.	Fibroma of uterus...
25	do.	Freiburg, Oct. 21, '79	do.	do.	Two fibroids of fundus uteri.
26	do.	Freiburg, Dec. 8, '79	do.	do.	Soft inters. myoma..
27	do.	Freiburg, Feb. 10, '80	do.	do.	Two fibromas of ut ..
28	do.	Freiburg, Feb. 28, '80	do.	sup. v. a. ut. & ov	Fibroma of ut., 6 lbs., interstitial.
29	Kaltenbach.	Freiburg, Mar. 2, '80	do.	do. do.	Fibroid, 4 mos. preg., fetus macerated.
30	do.	Freiburg, Mar. 19, '80	do.	do. do.	Fibroma, w. 4 1/2 lbs...
31	do.	Freiburg, Mar. 27, '80	do.	do. do.	do. w. 2932 gr..
32	L. Kleinwachter	Innsbruck, Nov. 22, '79	do.	Tumor & ovaries. 7 cm.	Anti-sepsis	Fib. of ut. an ov. ped.
33	Kocher...	Bern, June 24, '77.	do.	Sup. v. a. ut. Long.	do.	Fibro-cyst ut.
34	Kundrat	Not c.	Fibroid, partly sarcomatous.
35	E. Kuster.	Com..	Sup. v. a. ut.	Myo-cyst à géodes....
36	Leopold ..	November 23, '80.	do.	Ut. and r. ovary.	Interstitial myoma...
37	H. Lossen	Heidelberg, July 18, '78	do.	Ut. and ov	1 h.	22 cm	Fibro-myoma, 700 gr.
38	do.	Heidelberg, Nov. 26, '79	do.	Tumor & left ov.	1 1/2 h	20 cm	Anti-sepsis	Fibro-cyst, 2 1/2 kilo ...
39	Martin ...	Berlin, Aug. 7, '67	do.	Tumor... Long.	Ped. adhesions, vascular myoma, 12 3/4 lbs.
40	do.	Berlin....	do.	4 operations
41	do.	do. May 24, '80 (?).	do.	Enuc. tu. rem'd ov.	1 1/2 h	17 cm	Carbolic spray	Subm. fibro-myoma, cystic ovaries.
42	P. Müller.	Bern, June 4, '78.	do.	Sup. vag. ut. Long.	do.	Interstitial fibroid ...
43	Mueller...	Bern, cases.	2	Uterus	Fibroid

Operation.	Age, Married or single	Previous operation	Condition of patient	Result and cause of death.	Reporter and where reported.
Stump in angle...	42, M...	Recovery...	Berlin. Klin. Woch., xiii., 1876, p. 169; Obs. J., London, iv., 1876-7, p. 199.
do. do.	49	do.	Wiener Med. Presse, xviii. pp. 449, 481, 513, and 547.
do. do.	36, M...	do.	Central. f. Gynäk., iv., 1880, p. 265.
do. do.	49, M...	do.	ibid., p. 269.
do. do. drainage.	39	do.	do. do.
Stump in angle...	56, M...	do.	do. do.
Stump in wound..	35	do.	do. p. 270.
do. do.	32	do.	do. p. 271.
do. do.	42	do.	do. do.
Ligature.....	50, M...	do.	do. p. 270.
Ligature in wound	44, S...	do.	Archiv. f. Gynäk., xvi., 1880, p. 145.
Ecraseur & stump in wound.	36, S...	do.	Corres. Blatt f. Schweiz. Aerzte, vii., 1877, p. 693.
....	59, M...	Death, sept.	K. K. Gesellschaft der Aerzte in Wien, March 30th, 1883, Wien. Med. Presse, xxiv., 1883, p. 475.
....	49	Recovery...	Trans. Soc. Med. and Gynec., Berlin, December 10th, '78; Berlin Klin. Woch., xvi., '79, p. 360.
Stump dropped...	41, M...	do.	Obs. Soc., Leipzig, December 20th, 1880; Deut. Med. W'ch., vii., 1881, p. 306.
Péan's operation..	50, M...	do.	Berlin. Klin. Woch., xvi., 1879, p. 193.
do. do.	47, M...	do.	ibid., xvii., '80, p. 481.
Clamp in wound..	27	Death, hem.	Trans. Obs. Soc., Berlin, Jan. 12, '69; Monats. f. Geburtsk., xxxiii., 1869, p. 242; Pozzi, p. 86.
1st case, Péan's operation, others piece excised.	4 deaths	Trans. German Gyn. Soc., Sep- tember 12th and 13th, 1878; AM. J. OBS., xii., '79, p. 199; Cent. f. Gynäk., ii., 1878, p. 496.
Drainage.....	35, S...	Recovery...	Deutsch Med. Woch., vi., 1880, p. 357.
Stump in wound..	48	do.	Corresp. für Schweizer Aerzte, 1878; Cent. f. Gynäk., iii., 1879, p. 96.
do. do.	2 recoveries.	Trans. German Gyn. Soc., Sep- tember 12th and 13th, 1878; AM. J. OBS., xii., '79, p. 198; Cent. f. Gynäk., ii., '78, p. 495.

Number.	Name of operator.	Date and pl. of operation.	Completed or not.	What was removed.	Duration of operation.	Length of incision.	Antiseptic anesthetics.	Character of tumor and complications.
44	R. Olshausen.	Halle, M'y 4, 1880.	Com.	Tumor...	1 $\frac{3}{4}$ h	Long	Carb'c spray.	Cysto-myoma, 45 lbs.
45	do.	4 operations.	Enucleation.	Twice pedic. fib.; 1 inters.; 1 partially pedic.
46	V. Rokitsansky.	Vienna, May 16, '7	Com.	Tum. sup. va. ut & ov	2 $\frac{1}{4}$ h	20 cm	Pedic. and inters.; myo-fibroma.
47	do.	do. Feb. 9, '78.	do.	Sup. vag. ut., ov., & tubes.	Long	Carb'c spray.	Myo-fibroma, weight 4450 gr.
48	do.	do. Nov. 18, '75.	do.	do.	2 h.	do.	Inter. myo-fibroma..
49	Schroeder	Berlin.... ¹ ¹	Ut. fibroids
50	do.	do. Oct. 22, '78.	Com.	Ut. & tum	Var. fibro-cyst.
51	do.	do. Jan. 27, '79.	do.	Tumor...	Myoma.....
52	do.	Berlin....	do.	Tum. & ut	Fibro-myoma.....
53	do.	do. Nov. 15, '79.	do.	Tumor...	... Long	Preg. 4 months; fibro-myoma.
54	B. Stilling	Cassel, Aug. 23, '64	do.	Ut. and r. 1 ovary.	1 h.	do.	Myoma, 12 lbs.....
55	Tappehorn.	Oldenberg, June 15, '71?	do.	Ut., r. ov., and tube.	2 h.	do.	Fibro-cyst.
56	Winckel.	Dresden..	do.	Tumor...	Fibroid.....
57	Wolf.....	Hamburg, May 8, 1878.	do.	Tum., ut. & r. ov. Long	Antiseptis	do. 11 lbs.....

¹ 6 operations for uterine fibroids.

Operation.	Age. Married or single.	Previous operation.	Condition of patient.	Result and cause of death.	Reporter and where reported.
Stump in wound, drainage.	49, M.	Recovery ...	Archiv f. Gynäk., xvii., 1881, p. 424.
....	2 deaths....	Trans. German Gynäk. Soc., Sept. 12th and 13th, 1878; AM. J. OBS., xii., 1879, p. 198; Cent. f. Gynäk., ii., 1878, p. 496.
Péan's clamp and drainage.	42, S.	Death	Wiener Med. Presse, xx., 1879, p. 805.
Péan's cl'p, Chas- saignac's chain: stump in w.	44, S.	do.	ibid., p. 772.
do.	31, S.	Recovery ...	ibid., p. 708.
....	5 recoveries, 1 death.	Transactions German Gynäk. Society, September 12th and 13th, 1878; AMERICAN JOUR- NAL OF OBSTETRICS, xii., 1879, p. 198; Cent. f. Gynäk., ii., 1878, p. 494.
....	Transactions Society Midwifery and Gynecology, Berlin, Oc- tober 22d, 1879; Berliner Klinische Wochenschrift, xvi., 1879, p. 280.
....	Transactions Society Midwifery and Gynecology, Berlin, Jan- uary 28th, 1879; Berliner Klinische Wochenschrift, xvi., 1879, p. 392.
....	Transactions Society Midwifery and Gynecology, Berlin, Jan- uary 24th, 1879; Berliner Klinische Wochenschrift, xvi., 1879, p. 606.
....	M.	Transactions Society Midwifery and Gynecology, Berlin, No- vember 25th, 1879; Berliner Klinische Wochenschrift, xvi., 1879, p. 113.
Ligature.....	39, S.	Deutsche Klinik, xvii., 1865, p. 95.
Ligature outside..	50, M.	Tap	Emas- cua- ted.	Death	Deutsche Klinik, xxiii., 1871, p. 268.
...	do.	Transactions German Gyneco- logical Society, September 12th and 13th, 1878; AMERI- CAN JOURNAL OF OBSTETRICS, xii., 1879, p. 199; Central- blatt f. Gynäkologie, ii., 1878, p. 495.
....	28, S.	Recovery ..	Transactions German Gyneco- logical Society, September 12th and 13th, 1878; AMERI- CAN JOURNAL OF OBSTETRICS, xii., 1879, p. 187; Archiv f. Gynäkologie, xiii., 1878, p. 466.

H U N -

Number.	Name of operator.	Date and pl. of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision.	Antiseptics.	Character of tumor and complications.
1	Jos. Kovács.	Budapest, Nov. 20, 1879.	Com.	Tumor & uterus.	Long.	Inter. myxo-myoma
2	W. Tauffer	Budapest,	do.	Ovaries...	Ut. fibroma.....
3	do.	do.	do.	do.	do.

S P A -

Number.	Name of operator.	Date and pl. of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision.	Antiseptics.	Character of tumor and complications.
1	Juan Creus.	Manse, Feb. 20, '79	Com.	Tumor	Long.	Spray	Myoma, w. 4500 gms.

I T A -

Number.	Name of operator.	Date and pl. of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision.	Antiseptics.	Character of tumor and complications.
1	Ceccarelli	Rome, Jul. 3, 1868.	Com.	Tumor ...	1 h.	Long.	Sub. p. fib., w. 8 kilo.
2	Fenanti ..	Sinigaglia May 30, '65	do.	do.	10m	do.	Fibroid.....
3	Landi	Pisa, Mar. 30, '70.	do.	Part l. ov. and tum.	1 h.	do.	Myoma, w. 3515 gr..
4	Rizzolo ..	Bologna, June 6, '62.	do.	Left ov. & tube.	do.	Fibroma, w. 1470 gr.
5	Peruzzi...	Lugo, Oct. 25, '69.	do.	Tum. and uterus.	44 m	do.	Sessile Fib., 1121 kilo.
6	do.	do. Sept. 29, '70.	do.	do.	ov'r 1 h.	do.	Myoma, w. 9300 kilo.

GARY.

Operation.	Age, Married or single.	Previous operat'n	Condition of pat't.	Result and cause of death.	Reporter and where reported.
Drainage	43, M.	Recovery....	This made the fourth case of ext. of ut. fib. by Koviács, with two recoveries. Pester Med. Chir. Presse, 1879, p. 969; Med. Soc. Pest, November 29th, 1879.
....	26, S.	do.	Pester Med. Chir. Presse, 1879, p. 987.
....	37, M.	do.	Pester Med. Chir. Presse, 1879, p. 988.

IN.

Operation.	Age, Married or single.	Previous operat'n	Condition of pat't.	Result and cause of death.	Reporter and where reported.
Stump in wound..	42, W.	Death	Anales de la Soc. Gynecol. Española, Mad., No. v., 1879, p. 68.

LY.

Operation.	Age, Married or single.	Previous operat'n	Condition of pat't.	Result and cause of death.	Reporter and where reported.
Stump in wound..	43, S.	Death.	D. Peruzzi Sull' Ov., Milan, 1873, p. 184.
....	Recovery ..	ibid., p. 190.
Clamp and cautery	31, S.	Death.	ibid., p. 186.
Ligature.	42	do.	ibid., p. 182.
Stump in wound..	28	Recovery ...	ibid., p. 184.
do.	36, M.	Death	ibid., p. 186.

CORRESPONDENCE.

THREE CASES OF PROBABLE PELVIC LYMPHORRHEA.

BY

MARY HARRIS THOMPSON, M.D.,
Physician to Chicago Hospital for Women and Children,
Chicago, Ill.

TO THE EDITOR:

DEAR SIR:—Having just read in the AMERICAN JOURNAL OF OBSTETRICS for October, 1883, your paper on “Non-Puerperal Pelvic Lymphadenitis and Lymphangitis,” I wish to express to you my gratification, as your theory may enable me to solve what has been for years a pathological mystery.

My reasons for calling these cases lymphorrhœa may not be to you a proof of that disease, but allow me to give them in brief; then be so kind as to explain the source of a *white* discharge which apparently came from hardened pelvic tissue per rectum, and which at that time I diagnosed simple pelvic cellulitis. In only one instance was I called in an early stage of this disease, and not thinking of the character as other than the one mentioned, I did not observe the point of beginning of inflammation.

No. I. was that of a lady from Canada, about thirty years old. She had been married a few years, but had had no children.

When I was first called to visit her she had been ill some time, and was “trying to get along without calling a doctor,” but gradually grew worse. I thought she had been sick a week or more.

I found a hard mass or disc across the pelvis on a level with the roof of the vagina. This disc was equally dense from side to side of the pelvis.

No treatment that I gave seemed to make the least impression in causing a resolution, but the hardened tissue remained, the patient gradually lost her appetite, and after a few weeks a *white* discharge, which was slightly thicker than milk, occurred from the rectum. It would appear in the stools and alone. The quantity varied from one-half ounce to an ounce, and would occur six and eight times daily, and a less number of times at night, but in larger quantities.

The patient wasted gradually until there seemed to be no fat or muscles left at the time of her death, which occurred some three months after the beginning of the disease.

Case II. was Mrs. K. K., whom I saw in the year 1869. She

was a tall, well-formed Swedish lady, thirty-five years of age, with light complexion, light-brown hair, blue eyes, regular features, good teeth, healthy skin. She had never had children.

While making my first visit, I learned that she was being attended by two of Chicago's best physicians; hence I declined to attend her. But she insisted that I should do so, and I consented on condition that she should tell them how unwillingly I took the responsibility, and that I could not cure her if they could not.

My diagnosis was pelvic cellulitis, as a hard mass was found occupying the roof of the vagina and extending from side to side of the pelvis, and which seemed to level off the roof, carrying above this disc all parts usually mapped out in this place, except the vaginal portion of the cervix uteri. This and the preceding case were unlike yours in being *smooth*, hard, and tender.

The cervix when exposed did not appear much congested, but of its normal pink color. On one side of the disc it was slightly softer. The general mass was of a rock-like density.

I prescribed chloride of iron and iodide of potassium, and later cod-liver oil. The value of the hot douche was not known at that time here. I used leeches (six) upon the walls of the vagina and cervix uteri, and afterwards, at intervals of a week, the tincture of iodine.

In about two months from my first visit, this hard mass had gradually softened, and the patient was well and acting as her own house-maid.

Some three months after her recovery, she was again taken ill, caused by "taking cold" and over-exertion. Though she called me again, she would not allow the use of the leeches. I prescribed otherwise as at first. She had but little desire to get well, and took all medicine irregularly and in an indifferent way, but remained in bed, as she was obliged to do.

I now watched the progress of inflammation, which began at the junction of the cervix and corpus uteri, then gradually extended to the whole pelvic boundary.

Instead of absorption, the density continued, and in some three or four weeks a *white* milky or creamy discharge occurred, as in the first case. The patient began to emaciate and became a mere skeleton before her demise, which occurred some three months after the commencement of her second illness.

Case III. was that of a Canadian-French woman, thirty-five years old and the mother of five children, the youngest one and one-half years old. She was of medium height, dark complexion, dark-brown hair, dark bluish-gray eyes, and imperfect teeth.

This woman had also had another physician, who had been preceded by another, and each had declared her pregnant.

She had a hectic fever and diarrhea, which alternated with constipation. High in the pelvis posteriorly was a round, fluctuating tumor, as large as a fetal head. The remaining pelvic tissue was so tender as to make the required examination almost unbearable. In this case there was not much hardening, but more of an edema. I called it cellulitis enveloping an ovarian cyst.

After some time, her temperature rose and continued up to 102° F. to 103° F. One day she informed me that she had had "a terrible dysentery, and that the stools were clear matter, but that she felt better, though it seemed as though something had broken." It now occurred to me that the cyst was an abscess and that this was the source of the purulent stools, streaked with blood. As the stools continued the cyst disappeared.

Some days after the pus appeared, another discharge began per rectum. It would be found alone or with the pus and stool, or the pus would be found alone or with stool. This last discharge was the same as in the first and second cases and in the same quantity. When it appeared with the other excretions it would remain, as the patient said, "as distinct and separate as oil in water."

I trouble you with this, as you seem to believe that lymphangitis can occur as a disease in the pelvis; hence may I ask if this fluid of a purely *white* color and of that consistence can be the contents of the lymphatic vessels? If that fluid, would it be made thicker by inflammation of the epithelium lining those vessels, and would it be secreted in such quantities?

I have treated many cases of nearly all diseases that can be found in the pelvis at the Chicago Hospital for Women and Children, but none with this discharge.

Perhaps you will ask me why I did not examine it microscopically. I did not at that time use the microscope, but should do so now.

638 N. JACKSON STREET, CHICAGO,
December, 1883.

[The cases reported by our correspondent seem to be of that rare disease, lymphorrhœa, or oozing from some larger lymphatic vessel. Such a discharge has been observed from other parts of the body (the external genitals, for instance); why should it not then occur from the pelvic lymphatics, per rectum or vaginam? Lymphangitis would be likely to increase its density by addition of endothelia, and a lymphatic discharge sufficiently profuse to produce gradual emaciation and ultimate death is by no means impossible.—ED.]

BORO-GLYCERIDE IN GYNECOLOGY.

TO THE EDITOR OF THE AMERICAN JOURNAL OF OBSTETRICS:

DEAR DOCTOR:—In the New York *Medical Record* for September 23d, 1883, you will find a letter published by me on *Boro-Glyceride*. I directed attention then chiefly to its invaluable properties as an antiseptic. Since that time I have used it very much in my gynecological practice. In solution as an injection for leucorrhea, etc., and also pure boro-glyceride suppositories. It is an excellent application for ulcerated cervix, spread thickly on the diseased surface. It can also be spread upon absorbent cotton and introduced in that manner. I intend to have some large hollow vaginal suppositories made of boro-glyceride, which I believe will be found very desirable. I regret to say that I have been unable to find satisfactory samples of boro-glyceride, except from Theodore Metcalf & Co., of Boston, whose preparations of boro-glyceride are the finest in the market. Various mixtures of *borax* and glycerin, and also of boracic acid and glycerin, *simply mixed*, have been called boro-glyceride. The preparation manufactured by Messrs. Theodore Metcalf & Co. will be appreciated by those using it, and its superiority recognized at once, as they use only *pure* glycerin and English boracic acid. After considerable experience with the various preparations used for local treatment in vaginitis, leucorrhea, etc., etc., I find none yielding such satisfactory results as the boro-glyceride, although I still use the sulpho-carbolate of zinc to some extent. The cleanliness and the gentleness of this remedy and the steady improvement resulting from the use of boro-glyceride will be very satisfactory to both patient and physician. I sincerely hope that it may receive more attention from gynecologists and prove to be all that I predict for it.

W. THORNTON PARKER, M.D.

MORRISTOWN, N. J., December 2d, 1883.

TRANSACTIONS OF THE OBSTETRICAL
SOCIETY OF NEW YORK.

Meeting, June 5th, 1883.

OVARIAN CYSTOMA COMMUNICATING WITH THE RECTUM.

DR. WILLIAM M. POLK showed a specimen, and related the history of the case.

DR. JAMES B. HUNTER thought that ovarian cysts communicating with the rectum must be rare. He had seen one case in which such a communication had been suspected from the clinical

history, but the patient was not long under his observation, and the question was not settled.

DR. JOSEPH E. JANVRIN had seen an instance of the sort, in which the cyst twice discharged its contents through the rectum. No operation was performed, as the patient was suffering with Bright's disease, of which she subsequently died.

DR. WILLIAM T. LUSK alluded to a case that had come under the observation of the late Dr. Peaslee, in which a large abscess, which had discharged through the rectum, was supposed before the patient's death to be an ovarian tumor.

OVARIAN TUMOR TREATED BY PARTIAL REMOVAL AND DRAINAGE.

DR. POLK also showed the uterus, broad ligaments, and rectum of a patient whose history was as follows: She was sixty-nine years old, and had had good health until a year and a half before, when a tumor made its appearance in the right iliac region. It increased rapidly in size until by its weight and bulk her health and comfort were seriously affected. When she was admitted into Bellevue Hospital the tumor was found to be ovarian. The uterus was pushed to the left and well down into the pelvis, the fundus resting against the lower margin of the left obturator foramen, imparting a feel of resistance to the left lateral region of the pelvic floor. The remainder of the floor was occupied by a fluctuating mass—the whole crowded down to the pelvic outlet. It seemed probable that the tumor was without a pedicle.

The operation was done March 1st. The tumor was emptied and turned out without difficulty, there being few adhesions. After cutting away the larger part of the sac, its cavity was found to extend between the folds of the right broad ligament quite to its base, so that the tissue intervening between the cavity of the vagina and that of the sac was barely a third of an inch in thickness—in fact, the bottom of the sac, as the specimen showed, was attached to the upper end of the vagina in Douglas' cul-de-sac, being thence reflected to the anterior wall of the rectum. All this portion was covered with a side-growth of papillomatus tumors, and near the upper edge of the broad ligament were several nodules having a suspicious resemblance to cancerous deposit. In view of these pathological products, enucleation was deemed preferable to any attempt at drainage. This was accomplished throughout the entire extent of the attachment, save a spot about as large as a half-dollar, situated partly on the anterior rectal wall and partly in Douglas' cul-de-sac. This was denuded with scissors, and seared with a cautery. The time consumed, over an hour, had seriously taxed the patient's powers. A drainage-tube was introduced, the end resting in Douglas' pouch, and the wound was closed; but the patient never recovered from the operation—she died of exhaustion on the sixth day.

Owing to the nature of the growths in the sac, it had seemed improper to follow the plan of stitching its walls to the abdominal incision, and treating it by drainage. That would have shortened

the operation, but would have robbed the patient of the benefit of the permanent removal of tissue subsequently shown to be cancerous. The one spot at which denudation, but not entire enucleation, was accomplished was situated too deep to be brought and stitched to the abdominal opening. The use of a drainage-tube was therefore all that was left.

It occurred to Dr. Polk that, in simple sacs, free from carcinomatous deposits, in which the bottom was close to the vagina, and in which enucleation was difficult, an advantage might be gained by making a counter-opening into the vagina, stitching the walls, as usual, to the abdominal incision, and then passing a tube so as to secure drainage from both ends of the sac. There would be no danger of implicating the peritoneum in such a procedure, as the attachments of the sac above and below would isolate the tube.

TAIT'S OPERATION.

DR. POLK related two cases, and showed the specimens. In the first case the operation had been done three weeks before, and the patient was doing well. The indications calling for the operation had been well marked—for seven or eight years the patient had suffered from intense ovarian dysmenorrhea, but her general health was good, so that he had regarded the case as likely to furnish a good test of the value of the operation. He would report the ultimate result at the proper time. The operation had not been found difficult.

The second case proved fatal and the specimens shown consisted of the ovaries and the uterus. In this case the operation had been found troublesome, owing to excessive thickness of the abdominal wall from the deposit of fat. This condition had also interfered with the proper closure of the wound—the peritoneal surfaces could not be brought into due apposition, and, consequently, the fatty tissue remained to a certain extent in contact with the intestines. An abscess formed and opened into the abdominal cavity, and this was the cause of death.

DR. LEE was not inclined to lay so much stress on the union of the peritoneal surfaces as was generally done.

DR. POLK had formerly regarded it as highly desirable, but he had lately seen it questioned by Dr. Goodell, of Philadelphia.

DR. LEE added that views similar to Dr. Goodell's had been published by Mr. Knowsley Thornton, Sir Spencer Wells, and others, but that he could only look upon them as theoretical. For his own part, he would aim at perfect union.

OÖPHORECTOMY.

DR. HUNTER showed the ovaries removed in two cases of oöphorectomy. In the first case, on cutting the Fallopian tube, a dark fluid spurted out, as from the umbilical cord when divided between two ligatures. This was carefully sponged out, but septic peritonitis took place and ended fatally. The ovaries showed cystic degeneration.

In the second case there was likewise cystic degeneration. The

patient had done well, although at first there was a moderate peritonitis. In the first case he had derived great aid, in searching for a bleeding point, by passing a large Fergusson's speculum through the wound down to the source of the hemorrhage. Sponging through this instrument, he had been able to see the exact point from which the bleeding proceeded, and apply a Paquelin's cautery to it.

DR. LUSK asked if patients had not been known to be restored to health, after suffering with the symptoms for which Battey's operation was now done, by other than operative treatment. He referred to times preceding the introduction of the operation.

DR. WILLIAM M. CHAMBERLAIN remembered an instance in which restoration to health followed treatment in a lunatic asylum where the opium habit was overcome.

DR. BENJAMIN F. DAWSON could not see how medicinal treatment could prove curative in cases where the ovaries were bound down by adhesions.

DR. LUSK called attention to the fact that, of the five cases reported at this meeting, three had proved fatal. He thought the inference was, that all possible measures should be exhausted before resorting to the operation.

DR. HUNTER and DR. PAUL F. MUNDÉ would emphasize the distinction that ought to be drawn between organic disease of the ovary, on the one hand, and its functional derangement or its crippling by surrounding deposit, on the other hand.

FIBROUS POLYPUS SIMULATING INVERSION OF THE UTERUS.

DR. MUNDÉ showed a polypus that he had removed in a case in which difficulty had been met with in the diagnosis, owing to the tumor having contracted adhesions to the cervical canal on all sides. It was only when the patient was anesthetized and actually on the operating table that he had been able to feel the body of the uterus, which was very small.

PREGNANCY WITH FIBROUS TUMORS OF THE UTERUS.

DR. LEE showed an unruptured four months' ovum that had been expelled spontaneously from a uterus in which the existence of two fibrous tumors had been diagnosticated. Their presence had made the condition of pregnancy difficult to determine.

DR. LUSK was accustomed in such cases to rely a good deal on the mammary signs, and he attached considerable importance to the peculiar bluish tint of the vagina.

ADENOMA OF THE RECTUM.

DR. POLK showed a patient from whose rectum he had enucleated an adenoma as large as a mandarin orange, using the finger and the handle of a scalpel, after having performed proctotomy by Volkmann's method. The patient had suffered for several years with pain and a bloody discharge, and had been growing worse. The tumor was situated just within the external sphincter.

Meeting, October 2d, 1883.

ARREST OF FETAL DEVELOPMENT.

DR. LEE presented a fetus, supposed to have been born at full term, in which there seemed to be absence of the thighs and of the neck; at the situation of the cerebellum there was a large, soft mass covered by integument; the entire figure resembled that of a dumpling. He had not received permission to dissect the specimen. The mother was an unmarried woman, thirty-two years of age, well formed and healthy. During labor she professed simply to be suffering from cholera morbus, and decidedly resented a vaginal examination. A foot was seized, turning was effected, and delivery completed without further difficulty.

DR. B. F. DAWSON thought there was often a scarcity of liquor amnii found at the birth of such monsters, and that it was a factor in their development, and asked if such was the case in this instance.

DR. LEE replied that no liquor amnii was observed.

DR. H. F. WALKER mentioned the case of a woman, confined last spring, giving birth to a child imperfectly developed. Delivery took place about a fortnight sooner than she had expected. Dr. Walker diagnosticated a breech presentation, and felt what he supposed to be the scrotum, but which proved to be a spina bifida of about the size of an English walnut. The feet were clubbed, the legs short, the thighs flexed upon the abdomen, and capable of being straightened to a right angle only. Afterward hydrocephalus developed, and at present the head was of double the normal size; the spina bifida had increased to the size of a Mandarin orange. The mother's health had been good; that of the father only fair.

DR. H. T. HANKS said, with reference to the difficulty of making out the presentation, that he once mistook an encephalocele for the breech and scrotum. The error was ascertained by noticing the reflex contractions of the child's lower limbs when the hand pressed the base of the tumor.

DR. LEE referred to a case once reported to the Society by Dr. Gillette, in which long delay in delivery was finally discovered to be due to a hydrocephalic tumor, which was then punctured, allowing of delivery. The child died a few hours after birth, apparently from exhaustion rather than from shock.

DR. J. B. HUNTER asked the President if, in the case of the monster presented, the arrest of development might not have been due to the effect of ergot or other means which the mother had probably taken to produce abortion.

DR. LEE replied that a very interesting and suggestive question had been raised, but he did not think it would be practicable to answer it with our present knowledge of fetal pathology and the effect which maternal impression produced upon fetal development.

OVARIOTOMY IN OLD AGE.

DR. J. E. JANVRIN presented a monocoyst removed in June last from a woman seventy-seven years of age. Except for the influence of the rapid growth of the tumor, the patient was in good health. The operation was very simple, there being no adhesions, and the

tumor being removed through an incision only two inches long. She made an excellent recovery. He believed this to be the oldest patient upon whom successful ovariectomy had been performed in this country, and perhaps in the world. During the same month he operated upon two other patients, in one performing double ovariectomy, and in both recovery went on rapidly. These three patients were operated on in the extremely hot weather of that month, and yet, in spite of the enervating effect of the heat, the recovery was very quick with all of them.

DR. P. F. MUNDÉ said that the operation had before been performed successfully upon patients seventy-seven years of age in this country, in one case each by Dr. A. G. Gerster, of New York, and Dr. M. D. Mann, of Buffalo. He thought that in the case of old people the question would arise whether it were not better to resort to the temporary benefit from tapping rather than subject them to the risks of ovariectomy.

DR. HUNTER had a case at present in which he had advised the patient, seventy years of age, against ovariectomy, tapping having already given relief several times.

PUERPERAL CONVULSIONS.

DR. W. M. CHAMBERLAIN had recently been called in consultation, and received from the attending physician the following history: A young woman, well developed, but of the dark type, of strumous habit, and often ailing, was now in the eighth month of her first pregnancy. During the earlier months she had suffered to an unusual degree from neurotic and digestive troubles. For the last two months she had been in the country, and had been much better. She had returned to town the day previous, entertained company at dinner, and retired at eleven, with a very bad headache. At three in the morning she had got up to pass water, and while doing so had been seized with a convulsion, in which she fell to the floor, bruising her face and cutting her lip badly. Convulsions had recurred several times in the day, and, when Dr. Chamberlain saw her, at 3 P.M., she was profoundly comatose. The face was swollen from bruises and livid with venous congestion. The urine, on boiling, was nearly all solid coagulum. The vagina and cervix, exposed by Sims' speculum, were even more livid than the face. The uterine neck was still columnar, and the os closed. Dilatation was begun at once with hard-rubber dilators, followed by Barnes' bags. In the first hour there were three convulsions. After the os was opened to the size of a dollar they ceased, and did not recur. Twelve hours after beginning dilatation, and twenty-four after the first convulsion, delivery was accomplished with the forceps. The uterine pains had been of the feeblest kind, and manual expression had been continuously employed. The loss of blood was very moderate. The child, whose heart-beat had been unrecognized for an hour previous, was asphyxiated. Its restoration was for a time much in doubt, but was finally accomplished. The mother remained unconscious. Dr. Chamberlain advised purgation by

croton-oil, a hypodermic injection of one-sixth of a grain of pilocarpine, and chloroform inhalation in case the convulsions should return. The condition of the patient seemed so very unpromising that the attending physician did not execute these suggestions. He, in fact, did nothing, and six hours later reported the patient as still comatose, as having had two convulsions, and as apparently moribund, with a temperature of 106° and a pulse of 160. But from this point she began to improve. Consciousness slowly returned, and six hours later the temperature had fallen to 101° . The second day albumen disappeared from the urine, and now, after five weeks, she was quite well and nursing her baby. The case is particularly interesting as showing how Nature sometimes relieves the uremic patient. Undoubtedly a full venesection was indicated in this case, but, although considered, it was not done.

DR. MUNDE mentioned a case in which he had employed *accouchement forcé*, using the fingers and hand in dilatation instead of instrumental dilators. The patient was the mother of several children, and was eight months advanced in pregnancy. Convulsions began in the night, and when her physician arrived, at 10 A.M., he found her comatose, in which condition she remained until Dr. Mundé saw her in the evening and effected delivery. One convulsion occurred while dilating the cervix. This procedure required about fifteen minutes. The patient was then breathing stertorously, the temperature was 102.5° F., the pulse 140, and she was considered moribund. Death took place half an hour after delivery. The child was born apparently dead, but was easily resuscitated, and lived a month.

DR. E. L. PARTRIDGE had seen a good many cases of puerperal eclampsia, and had sought for a reliable sign upon which to base a favorable or an unfavorable prognosis. The number or rapidity of the convulsions did not constitute an indication. He had found that, when a rapid rise of temperature took place, a degree or two within one or two hours, until 104° F., or more, was reached, the patient usually died. While delivery should undoubtedly be produced after the occurrence of puerperal convulsions, he did not think that we could foresee their development with sufficient certainty to justify us in anticipating them by the induction of labor.

DISAPPEARANCE OF UTERINE FIBROIDS DURING PREGNANCY.

DR. HANKS stated that at a former meeting of the Society he had related the history of a case of successful delivery in a patient carrying a fibroid tumor in the cervix uteri. She afterward became pregnant, and he, Dr. Lee, and others, diagnosticated a fibroid in the cervix, and also in the posterior wall of the body of the uterus, and advised the induction of labor at the eighth month. The patient entered the Nursery and Child's Hospital, and Dr. Partridge and the house physician were unable, both before and after delivery, to discover any signs whatever of a tumor of the uterus. It would seem, therefore, that the tumors must have undergone retrograde metamorphosis or absorption during the course of pregnancy.

DR. PARTRIDGE said that several very careful examinations were made in the case, and no signs of a tumor could be found.

DR. LEE said that an amusing case of this kind occurred some years ago when he was associated with the late Dr. Elliot. A wealthy lady of Brooklyn became pregnant, and consulted Dr. Elliot, Dr. Emmet, and some other eminent New York physicians, with regard to her condition. They agreed that there existed a large fibroid tumor of the uterus. As pregnancy advanced, the patient felt less trouble from her condition, but placed herself under the care of Dr. Budd when the time for confinement approached. At that time Dr. Budd was unable to discover any tumor, and, at the request of the lady, wrote a certificate to that effect. The lady made this the basis of a suit against the physicians, to some of whom she had previously paid large consultation fees.

DR. HUNTER had seen a case in which a fibroid tumor of the uterus, of the size of a Mandarin orange, disappeared during pregnancy, but reappeared after involution.

DEATH FROM PUERPERAL ECLAMPSIA.

DR. DAWSON related the history of a case as follows :

M. A., primipara, aged twenty-two, finished her last menstruation January 17th, 1883. Until within two hours preceding the first convulsion she complained of no unpleasant symptom except an occasional pain in the cardiac region, with slight palpitation.

On September 25th she ate a hearty dinner and retired in her usual good health, but was awakened about 1.30 A.M., September 26th, by severe pain in the abdomen, and, thinking that she was to be confined prematurely, she started at once for the New York Foundling Asylum. She had no sooner got into bed than she was seized with a convulsion. This was at 3 o'clock A.M. Dr. Blodget, who was immediately summoned, reached her bedside at 3.15, when the second convulsion occurred. He administered one-fourth of a grain of morphine and drew off two ounces of urine, which was all the bladder contained. The urine had a specific gravity of 1.029, and, on boiling, was found to be nearly solid with albumin.

Dr. O'Dwyer arrived at 3.45 A.M., and at 4 o'clock another quarter grain of morphine was administered, and the patient was placed in a hot wet pack, where she was kept for five hours, during which time, as well as nearly all the remainder of her life, she was kept under the influence of chloroform. Wet cups were also applied over her kidneys.

At 6 A.M. one ounce of infusion of digitalis and one drachm of acetate of potash were given by the rectum, and three drops of croton oil by the mouth, but no movement of the bowels was effected.

In spite of the chloroform and morphine, the patient had eight convulsions between 3 o'clock and 9.15 A.M., being constantly in an unconscious condition. At 9 A.M. the pack was removed, four ounces of urine, of the same character as before, were drawn, a third quarter grain of morphine was given, and attempts were made to induce labor.

A vaginal examination, made when the patient was first seen, gave no evidence of commencing labor. The cervix was long and conical, and the external os was firmly closed. By 10 A.M. dilatation had been made with the finger sufficiently to permit of the introduction of a Barnes' dilator. During the remainder of the day half a grain of morphine was administered, making in all one grain and a quarter; also four ounces of brandy, four ounces of the infusion of digitalis, three drachms of the acetate of potash, and several ounces of beef extract by the rectum were given.

The inhalation of chloroform and the attempts to dilate the cervical canal by means of the Barnes' dilator were continued until about 4 P.M., September 26th. At that time, the dilator—having proved ineffectual, although they were so thoroughly distended that one of them burst, and the convulsions still continuing (twelve had thus far occurred), the cervix was incised and stretched and torn with the fingers until the forceps could be applied, and at 5 P.M., a dead child was extracted by Dr. O'Dwyer. The placenta and membranes were removed intact about ten minutes later; the uterus contracted well, and hemorrhage was very slight.

At this time the patient's general condition was still fair, the pulse was 128 and tolerably strong, the temperature was 103° F., and the respirations 29.

The convulsions, however, recurred with nearly the same frequency as before delivery, in spite of the continued administration of chloroform, and from 9 A.M. until death she passed only two ounces of urine. Between the time of delivery, which occurred at 5 A.M., September 26th, and death, which took place at 2.45 A.M., September 27th, there were five convulsions, making, in all, nineteen in the twenty-four hours.

The hot wet pack was reapplied at 7 P.M., September 26th, and the patient perspired profusely for several hours, but to no purpose.

A microscopical examination of the urine, made by Dr. Northrop, showed hyaline, granular, and epithelial casts (most of them large), and free granular epithelium. There was no blood nor fat, nor fatty casts.

DR. DAWSON asked the question whether in such cases, when it was evident that the patient must otherwise die, it might not prove of benefit to withdraw the poisoned blood by venesection, replacing it at the same time by transfusion of fresh blood.

DR. MUNDÉ said the suggestion had been made before, but he was not aware that it had ever been acted upon. It was to be remembered that transfusion was always attended by symptoms of shock, more or less severe, and, considering the condition of the patient in these cases, the procedure would be very hazardous. Nevertheless, as, on theoretical grounds, it would seem to offer a possibility of rescuing the patient from death, he thought that he would resort to it on the next favorable opportunity.

DR. CHAMBERLAIN had performed venesection without transfusion, apparently without effect.

DR. LEE thought that patients with puerperal convulsions were too liable to be over-treated. In his opinion, the induction of labor and narcotization during convulsions by hypodermic injections of morphine constituted all the treatment that was called for.

DR. CHAMBERLAIN called attention to the interesting fact that, in his own case, and in most cases, when the patient lay in the comatose state, the irritation produced by dilatation of the cervix caused convulsions, and that after dilatation was complete the convulsions usually ceased. The nausea of pregnancy and that produced by the presence of fibroids, etc., were known sometimes to disappear after dilatation of the cervix.

In the further discussion upon Dr. Dawson's case, the impression prevailed that venesection and transfusion would prove of doubtful benefit.

Meeting, October 16th, 1883.

VAGINAL HYSTERECTOMY FOR EPITHELIOMA OF THE CERVIX UTERI.

DR. PAUL F. MUNDÉ related a case of epithelioma of the cervix uteri in which he had removed the uterus and the ovaries by the vaginal method at Mount Sinai Hospital six days before. The specimen was shown, and, as Dr. Mundé remarked, it seemed to indicate that the diseased tissue had been thoroughly removed. Thus far, the patient had done perfectly well. It was now a question how long the drainage-tube should be left in place. It was held by a flange, and there was no leakage around it.

DR. T. A. EMMET thought the tube would have become so encysted at the end of a week as to answer no further purpose.

[*Note by Dr. Mundé.*—The tube was removed on the eighth day, the ligatures and sutures on the sixteenth, all but one ligature, which was still retained five weeks after the operation. The patient left her bed on the fourteenth day, and the house during the fourth week, and (Jan. 20th, 1884) thus far remains well.

TRANSACTIONS OF THE WASHINGTON OBSTETRICAL AND GYNECOLOGICAL SOCIETY.

Meeting, October 5th, 1883.

The President, DR. SAMUEL C. BUSEY, delivered his first annual address, as follows:

CRANIOTOMY UPON THE LIVING FETUS IS NOT JUSTIFIABLE.

GENTLEMEN:—The meeting to-night completes the first year of the existence of this Society, and it gives me pleasure to congratulate you upon the success attained.

The duties of the presiding officer have been exceedingly light and pleasant. During the entire session there has not occurred a single infraction of the rules and courtesies of deportment, or of parliamentary order or decorum, and there has been but one failure to comply with the individual obligations of the members. The attendance has been prompt and the average unusually large.

The essays and discussions have exhibited study, thought, and a high order of professional and scientific attainment. There has not been one indifferent paper submitted. The zeal and enthusiasm displayed by the essayists should command the admiration of the entire membership, and every member must acknowledge the value of the instruction derived from the efforts of his colaborers in this field of obstetric, gynecic, and pediatric study.

This young and vigorous Society has not, however, been permitted to complete its first year's existence without a sorrow. The memory of the lamented Ashford lingers in the freshness of commingled affection and grief. We who knew him so well, who had learned to value the qualities of his heart and mind, and had so often listened to his terse and cogent utterances, will not soon forget our friend, companion, counsellor, and co-worker. He died at the early age of forty-two, yet had made for himself a reputation which but few can hope to attain, and he has left to his friends and family the heritage of an untarnished name and unsullied character.

Medical societies with limited memberships, for the mutual instruction of their members in special departments of the science of medicine, have become quite common in the larger cities, both abroad and at home. Societies similar to our own have been established during the last decade in many cities no larger than Washington. The time had surely come when it was the duty of those who were specially interested in the study and practice of obstetrics and the diseases of women and children to unite and concentrate their individual efforts for their common good. The results in other places have demonstrated the literary and scientific advantages of similar organizations, and it cannot be doubted that this city possesses professional talent which will cope with the highest order of medical intellect. This position cannot, however, be won if individual capacities and acquirements are confined to the privacy and isolation of the sick-room. Enlarged thought and advanced study demand the broader field of intellectual comparison, analysis, and trituration. These desiderata can only be secured by organized effort, systematic presentation of well-considered opinions, clinical experience and observation, and the examination and discussion of these by men engaged in the same field of scientific labor. The success of the past year gives assurance of the complete fulfilment of the paramount objects of the association. The first being mutual improvement in scientific knowledge and practical skill, and the second a more intimate mutual acquaintance and personal intercourse.

A careful record of the proceedings and the publication of its transactions constitute the chief incentives to intelligent, systematic, and persistent work. No medical society prospers long and continuously without these aids. Upon the Recording Secretary and Committee of Publication important duties are devolved, to the complete and impartial discharge of which the continued existence and success of this Society mainly depends. The Committee should be clothed with full and necessary powers, and the Society should hold it to a strict responsibility. The duties, though very onerous, are too important to be neglected, and the committee should feel that the life, usefulness, and standing of the Society will be the measure of a careful and exact observance of its duty.

It would be invidious in me to select any one of the papers read before this Society for special commendation or criticism. It will, however, be pardonable to recall your attention to a subject which, in somewhat different aspects, was discussed on several occasions.

I am induced to again refer to the justifiability of the operation of craniotomy upon the living fetus because of the great importance of the subject and the increasing interest which it is now exciting, hoping that with the re-examination now in progress there will result a modification of the extreme views which have been held by a large majority of the ablest and most renowned obstetricians of the past, and, perhaps, of those living of equal ability.

IS CRANIOTOMY UPON THE LIVING FETUS A JUSTIFIABLE OPERATION ?

It is, probably, the most ancient of obstetric operations, and the hook and perforator are, perhaps, the most antique of obstetric instruments. The operation means death and mutilation of the fetus, and is performed solely in the interest of the mother. When the fetus is already dead it is an accepted procedure, within certain well-defined limits, to effect delivery. But then, as well as when the fetus is living, it demands skill and dexterity. In a large percentage of cases it is attended with serious dangers to the woman, and, according to Churchill and Tyler Smith, with a mortality in the proportion of one to five. Many other authors claim a higher death-rate, and a few a lower one. When performed upon the living fetus it necessarily involves the deliberate killing and mutilation of a human being. In every case of labor two lives are more or less in danger. Fortunately, in a vast majority of cases both lives are conducted safely through and past the perils of parturition. For the comparatively few cases in which delivery cannot be accomplished without artificial aid, the operation of craniotomy was originally devised in the interest of the mother, and from the times of Hippocrates and Celsus it has been recognized as a justifiable procedure, even when the fetus was alive.

In the remote periods of antiquity many pregnant women must have perished in the travail of labor undelivered, but even after the invention of the hook and perforator, and through all subsequent time down to the present, with the progressive improve-

ments in instruments, advances in obstetric knowledge and science, and the acquisition of manual dexterity, no operator has ever yet assured the life of the mother, even after the life of the fetus had been sacrificed, and the best result that can be offered is, according to the standard ¹ of Churchill and Smith, the saving of the lives of four mothers out of five, or, in other words, the saving of four lives out of ten, imperilled.

In the earlier times, when obstetric operations had their beginning and were at best performed with rude appliances in a bungling and unscientific manner by operators lacking knowledge and experience, the preservation of four lives out of ten which would certainly have perished must be cherished as a blessing to humanity, and the means by which it was accomplished must be regarded as a great advance in the obstetric art. But as yet there is no proof that Hippocrates, Celsus, or the Arabian physicians ever deliberately destroyed the life of the fetus preliminary to the extraction of the mutilated body of the unborn child. It may be that their mortality of mothers was greater, perhaps far greater, than it is to-day, but when Baudeloque and Klüge rated it at more than fifty per centum, Rokitsanski at forty-one, and Hemming, Jones, Churchill, and Smith at twenty, the comparison of our later-known results with the unknown of the remote past does not conduce to a very high appreciation of the progressive improvement in the operation of craniotomy. If, however, the question of mortality be studied chronologically, it will appear that the death-rate of mothers has diminished with the lapse of time and advance of obstetric science, and that it is now less than at any former period. It must, nevertheless be admitted that whilst the mortality of mothers has been diminished by the more dexterous performance of the operation and the better management of the cases, the relative proportion of feticides has greatly increased, and the sum-total of lives lost and sacrificed has been greater during the present than during any previous century since the operation was devised. The more frequently the operation is performed on the living fetus the greater the number destroyed, for half of the lives imperilled must necessarily be sacrificed, and the chances of saving the remaining half can only be enhanced by a percentage equal to the death-rate of the mothers, whatever that may be. The most expert and experienced operator cannot save more than half the lives at risk, and the more dexterous the greater the number of ventures, consequently the greater the loss and sacrifice of life. No one has or can hope to attain the success of saving a possible fifty per centum of the lives at stake.

Just here I will be confronted with the statement of those who

¹ In view of the advantages of antiseptics in surgical operations, this standard may be unfavorable to craniotomy, but as the object is to illustrate the fact that it is attended with a mortality, not now possible to ascertain, it answers the purpose.

have performed one, two, three, four, or five craniotomies without the death of a mother. The assertion of such a fact is probable proof that the operations were hasty and unnecessary. The successful craniotomist, more often influenced by ambition than judgment, and dazzled by the desire to gain an additional success, precipitates the death of the fetus that the dangers of delay may be avoided. If it is justifiable during the lifetime of the fetus, it is best to proceed to its execution before the exhaustion of the patient by ineffectual efforts, and before the contusion of the soft parts incident to attempts to effect delivery by the more conservative intrapelvic methods. But if such a rule should become the established practice, it is only the exceptional few who can avoid the guilty killing of a human being in cases where delivery of a living child might have been accomplished *per vias naturales*. Even so distinguished an obstetrician as Albert Smith¹ relates a case where a living child was born while the messenger had gone for the instruments of death, and more than once the shocking illustration of bungling haste has been exhibited in the cries of a mangled infant.

Craniotomy offers no hope, not even a ray of the promised life to the unborn, but proclaims from the altar of professional justification death by violence to the fetuses of women who are physically incapacitated to give birth to a living child. Nay, more, it offers immunity from the travail of labor, and protection from the annoyances of maternity to those who have accepted the pleasures of concubinage or wedlock, and have become copartners in the creation of a new being and a new soul to live forever, but who cannot complete the highest and noblest purpose of woman's creation. So repulsive does it present itself in this aspect, that many who have advocated and performed it recoil from its repetition upon the same woman. The renowned Meigs (Montgomery), who had twice successfully delivered Mrs. Reybold, refused to incur the responsibility of a third operation; Gibson subsequently twice delivered her by Cesarean section of a living child, and she lives to-day in the ripeness of a happy old age in the enjoyment of two children and six grandchildren.²

With such a showing it must follow that the operation is detestable where a living child is at stake. If so, is it entitled to a place among the scientific surgical procedures of the present time? Does the fact that in a limited number of those cases where the mother's life only is in peril, an early and skilful performance secures a reasonable prospect of recovery, give it such a place? Must

¹ Med. Times, March 10th, 1883, p. 412.

² "Several of the subjects of Cesarean delivery have reached advanced life in the United States, the oldest known being sixty-eight and seventy-four years respectively. Several of the children have been heads of families, and one in this city [Philadelphia] is now forty-eight years old, and has given birth to eight children."—Medical News, October 13th, 1883, p. 411.

this one fact, the only one in justification, and always embarrassed by the probability of repetition, and the consequent variations in the chances of success, counterbalance the enormity of deliberately taking the life of the unborn? To admit this in the fulness of its import is to concede that intrauterine life forms no part of the heritage of human existence, and that the violent destruction of a new being just at the moment when nature has completed the processes which fit it for an independent life, is a matter of such trivial concern that it can be determined upon the probabilities of enhancing the prospects of the woman's recovery.

If craniotomy is justifiable, science and the good of mankind demand that the limits of its application should be definitely fixed. This question is now engrossing the attention of some of the ablest and most conscientious members of the profession. It cannot be disposed of by words or sentiment, but must be settled by an examination of the facts derived from an intelligent experience, and an impartial study of the complications of labor and the methods of relief.

It matters not whether craniotomy is, or is not, the most ancient of obstetric operations; in a scientific aspect all the other methods and procedures, which have for their purpose the saving of the lives of both the mother and child, must be regarded as its substitutes. These may be divided into intra- and extra-pelvic. Under the first must be classed delivery by the forceps, by turning, the induction of premature labor, and symphysiotomy; under the latter, the Cesarean section and its substitutes, laparo-elytrotomy, the utero-ovarian amputation, and the total extirpation of the uterus, in all eight. The mere enumeration of this number of obstetric devices, and the constant and persistent efforts to improve and popularize them, constitute a reasonable presumption of a widespread detestation of craniotomy, and the present revival of interest in, and discussion of the relative merits of the extra-pelvic methods, and their advantages over craniotomy, would seem to be conclusive against its continued acceptance as a scientific procedure, and relegate it to the class of desperate expedients, of doubtful propriety under any circumstances.

According to Tyler Smith, about half of the cases of craniotomy are occasioned by contraction of the pelvis. This estimate is too low. The great improvement in the forceps, and greater dexterity acquired in the execution of the intra-pelvic substitutes, have vastly lessened the field of application formerly claimed for it, other than in cases of pelvic deformity. This, I believe, is now universally conceded by competent authority. And, even in very many cases of faulty pelvis to which, until recently, it was applicable, the better result to mothers, now obtained by other methods, have entirely excluded it.

In the justo-minor or equally contracted pelvis, it is inadmissible. In support of this statement, I need only quote two recent authors.

Lusk, in 1879, reported¹ a case in which the conjugate diameter measured, in the dried specimen, three and one-sixth inches, craniotomy proved fatal. In his review of the subject, he could find but five recorded cases of "generally contracted pelves, in which the conjugate ranged from three to three and a quarter inches, and all died as a consequence of delivery through the natural passages." In the same paper he refers to the case of Korman, nearly identical with his own, in which, "after more than three days' labor, the head adapted itself to the pelvis, and the child was delivered alive by forceps. The mother died of peritonitis." Even in cases of such extreme general contraction, nature and the timely application of the forceps have yielded better results than craniotomy. In discussing the proper management of such cases, he says that laparo-elytrotomy, which had been recently revived by the "genius of Thomas and the daring of Skene," is peculiarly fitted to such conditions, and concludes with the assertion that, after a careful study, he is convinced that "where there is a diminution of nearly an inch in all the diameters, Cesarean section or, probably, laparo-elytrotomy holds out the best chances of success."

Professor Isaac E. Taylor, in a masterly paper on the equally faulty or justo-minor pelvis, recently published,² asserts that "in almost all the cases recorded of equally faulty or contracted pelvis, when the diminution is from three-fourths to one inch, both mother and child are lost." In considering the treatment, he declares, with marked emphasis, that the loss of lives of mothers by craniotomy and cephalotripsy in such cases, when the diminution is from three-fourths to one inch (and the total loss of life to the child even in the minor degree of lessening of from one-third to one-half inch) is so disastrous that a conscientious discharge of duty demands the substitution of Cesarean section or some of its modifications, or symphysiotomy even in the minor degree of one-half inch. In the higher grades of contraction a more appalling presentation than by either of these operations could not exist. In justo-minor pelves craniotomy is inadmissible.

The most common form of deformed pelvis is the simple flat, rickety or non-rickety, in which the faulty condition is mainly in the conjugate diameter, hence it is usual to discuss the relative applicability of the various methods of treatment with special reference to the measurements of the conjugate. There is not, perhaps, living to-day a single obstetric authority of accepted repute who will claim the practicability of craniotomy in cases where the conjugate is one and a half inches or less. Indeed, but few hold it justifiable when the conjugate is two and one-half inches or less. Parry, as early as 1878,³ demonstrated that in pelves with a conjugate of two and one-half inches or less, craniotomy gave no better results to mothers than Cesarean section. When the total number

¹ Gynecological Transactions, vol. iv., p. 358.

² AM. JOUR. OBST., August, 1883, p. 811.

³ Amer. Jour. Med. Sci., vol. lxxiv., 1878, p. 323.

of lives at stake is considered, the results are vastly less favorable than from Cesarean section, even when performed under disadvantageous conditions, for one-half of the lives are certainly sacrificed by the murderous operation, and in those pelves where the conjugate is less than two and one-half inches Cesarean section is preferable whether the fetus is dead or alive. "As much as I have advocated," says Taylor, "craniotomy in preference to Cesarean section, in simple flat pelves, in my former papers on craniotomy and cephalotripsy, I am constrained to believe that one of the external operations, as the Cesarean section, or laparo-elytrotomy, early performed, or symphysiotomy when the labor is more advanced and the head wedged in the cavity, should be selected." In fact, in the light of recent experience and the improved results obtained from the intra- and extra-pelvic substitutes, professional opinion seems to be rapidly approaching the definite conclusion, that when the conjugate is less than two and one-half or two and five-eighths inches, craniotomy is absolutely inadmissible.

"As we have," says Montgomery, in a very able paper read before the Philadelphia County Medical Society,¹ "but three-fourths of an inch between three and twenty-five-hundredth inches, the maximum diameter at which craniotomy is supposed to be necessary, and two and one-half inches, the minimum diameter, in which it is safer for the mother than Cesarean section, we have certainly reached a period when we are justified in abolishing the murderous operation of craniotomy from the list of elective operations when the fetus is still alive." In the same paper, with equal ability and fairness, he discusses the relative advantages and merits of other alternatives, and reaches the following conclusions, which I accept in their entirety. These methods are equally safe to the mother, and afford the child a chance for life. They are suggested in the following order: "Where the conjugate measures three and one-fourth inches or over, the forceps; two and three-fourths or over, version; two and three-eighths or over, symphysiotomy, followed if necessary by the forceps. In all subsequent pregnancies, and in the first, when distortion is discovered sufficiently early, premature labor should be induced."

I need not pause to portray the value of the forceps. Perhaps no other instrument ever invented has contributed as much toward the alleviation of suffering, and saved so many lives. It is almost universally recognized as both a mother's and a child's instrument, and it is a significant fact that those who most frequently use forceps have least occasion to resort to either of the deadly expedients. The danger, if any, is not the result of their application, but of delay in using them.

When the conjugate measures between three and one-fourth and two and three-fourths inches, version² offers greater prospect of

¹ E. E. Montgomery, *Medical Times*, March 10th, 1883, p. 387.

² In pelves whose conjugate ranges from 2.75 to 3.25 inches, turning

success than craniotomy. The mortality of version has been generally estimated at one in sixteen of mothers and one in three of children. This is believed to be too high; but whatever it may be it is in marked contrast to that of craniotomy, even when the latter is limited to like conditions of pelvic deformity or other causes of obstruction in which either is claimed to be admissible. The mortality in cases of version is not, however, due wholly to the method, but quite as often to the causes which indicate it, as in placenta previa and rupture of the uterus, which may make it speedily imperative.

The induction of premature labor offers a more decided antagonism to craniotomy, because it is specially applicable in those conditions of pelvic distortion in which the craniotomists insist the latter operation is the proper elective procedure. Its application lies within the limit of two and one-half inches minimum, and three and one-half inches maximum measurement of the antero-posterior diameter. It is conceded that a viable child cannot be extracted through a pelvis with a conjugate less than the minimum, and that a living child at full term can be delivered through the natural passages with a pelvis measuring not less than three and one-half inches antero-posteriorly. Ritgen has constructed from the various measurements of the size of the head (quoted from Montgomery), at different periods of utero-gestation the following table showing the application of induced premature labor after the period of viability, to various grades of pelvic deformity. He says labor may be induced at the

29th week,	when ant.	post.	diameter of pelvis is	2" 7"
30th	"	"	"	" 2" 8"
31st	"	"	"	" 2" 9"
35th	"	"	"	" 2" 10"
36th	"	"	"	" 2" 11"
37th	"	"	"	" 3" 0"

From statistics of artificially induced premature labor, in cases of pelvic deformity, which I collated some years ago, when studying the value of the procedure in aggravated uremia, I determined the maternal mortality at five and twenty-seven hundredths per cent, and that of children at forty per cent. In these figures were included a large number of operations performed in the interest of the mother and under conditions which necessarily sacrificed the life of the fetus. With the improvement in the management of such cases greater safety has been secured to mother and child.

It thus appears that the induction of premature labor covers the exact limits of pelvic obstruction, and offers better results than craniotomy. It will, however, be said that, as a rule, it can only

should be the initial step.—Goodell, *AMER. JOUR. OBSTETS.*, vol. viii., p. 215.

¹ Kiwisch says $2\frac{1}{2}$ inches conjugate is necessary for a viable fetus at 30th week to pass safely.

apply to second and subsequent pregnancies, because the incapacity will not have been discovered until labor has begun. This, unfortunately is too true; but it is not an argument against the induction of premature labor, or in favor of the practice of craniotomy. It is simply proof of failure or neglect to ascertain the fitness of the pregnant woman to give birth to a living child *per vias naturales*, before it is too late to advise her, and to adopt the means science offers for the better protection of herself and her offspring from the dangers incident to her condition.¹

I must express my detestation and abhorrence of the wide-spreading vice of criminal abortion. I hold with that most eminent man and pious physician, the elder Hodge, that conception brings into existence a new being and an immortal soul, and that it is alike criminal in the mother and in the physician to employ means to destroy that being that sin and shame may be concealed or pride maintained. No reputable and conscientious physician will engage in or connive at the criminal production of abortion. Does the crime consist in the performance of the operation, in the effort to conceal vice, hide shame, prevent disgrace, obviate the discomforts of maternity, or in the deliberate act of killing? If in the latter, then tell me where the obligations of professional duty cease and crime begins. To me abortion as a substitute is equally as reprehensible as craniotomy. It cannot, however, occupy a place among the conservative methods, for in those cases in which pelvic deformity will not afford a chance to a viable fetus, craniotomy is conceded to be inapplicable.

The revival of the invention of Sigault, by Profs. Morisani and Novi in the Neapolitan hospitals has supplied another conservative procedure, which promises most favorable results. Harris² informs us that in the last sixteen years fifty-three operations have saved forty-three women and forty-two children. This result, says Taylor, "is equal to that of early performed Cesarean section under favorable circumstances." It has been alleged that symphysiotomy, even when the pubic joint "was opened to the extent of three inches, without impairing or injuring the sacro-iliac joint, only three or four lines at most could be gained." This is true in regard to the antero-posterior diameter, but the transverse and oblique diameters (Taylor, Harris) "are increased to the extent of one inch for the cavity and inferior strait." If the conjugate is not materially lengthened, the cavity is greatly amplified.³ This

¹ "There have been as many as sixteen children sacrificed in the successive labors of one lady in this city (Philadelphia), each head having been locked in her pelvis, which was small but not deformed; a seventeenth was saved, by an accident inducing labor, when she was eight months pregnant."—Harris, *Amer. Jour. Med. Sci.*, vol. lxxxv., p. 31.

² *Amer. Jour. Med. Sci.*, January, 1883, p. 17.

³ In Naples, the section is made subcutaneously with the probe-pointed and sickle-shaped bistoury of Galbiati. An incision is made above the pubis, and the knife slowly passed behind the symphysis until it reaches

amplification is what is needed to effect delivery in the class of pelvic distortions which have been considered. If necessary, delivery may be facilitated by either version or the forceps. In fact, symphysiotomy, premature labor, and either version or the forceps may be combined, and the delivery of a living fetus accomplished without detriment to the mother. The pubic section is not, however, recommended when the conjugate is less than two and five-eighths inches,¹ and consequently does not cover the precise limits of three-fourths of an inch between two and one-half and three and one-fourth, in which its advocates claim that craniotomy is, *par excellence*, the elective operation.

If I should rest the argument here, it might be maintained that version, the induction of premature labor and symphysiotomy failed to supply sufficient opportunities for delivery between those limits only separated by a fraction of an inch, and that craniotomy would be equally preferable in conditions of pelvic obstruction other than those caused by bony deformity, as in those cases where version or the forceps had proved ineffective. Its advocates cannot be permitted to cover retreat even under such ambiguous assumptions. The important and substantial improvements in the external methods leave it without justification even as an ultimate resort in these debatable conditions.

The Cesarean section and its substitutes offer additional preferable procedures. The weight of the later and more recent authorities, other than the British, is vastly in favor of this operation when the conjugate is one and one-half inches or less. In fact, it cannot be said that craniotomy competes with it when the conjugate is two inches or less, and even when the measurement reaches two and one-half its performance is considered by some of its warmest adherents of doubtful propriety.² If the relative merits

the pubic arch, when the ligaments are divided from below upwards. The pelvis is not forced open, neither is the fetus turned or dragged upon, but when the head presents, the case is left mainly to nature. In about one case out of four, the forceps are applied. The incised part is treated antiseptically, and by irrigation if in warm weather. And as soon as convenient the ossa pubis are kept in apposition by an immovable apparatus, to secure an early union of the severed parts. Harris, *loc. cit.*, p. 27.

¹ In the cases reported, the conjugate ranged between three and one-quarter and two and one-half inches. Raffaele reports a case operated at the beginning of the eighth month in which the conjugate measured only two and one-quarter. The patient, forty days after the operation, walked without inconvenience.

² "According to the investigations of Parry, whose decision I have verified by my own researches, craniotomy has scarcely a fractional advantage in saving life over gastro-hysterotomy in cases where the conjugate diameter of the superior strait measures two and one-half inches or less, and not even this claim when the latter is performed, as it should be, very early in labor." Harris, *Amer. Jour. Med. Sci.*, vol. lxxvii., p. 46.

of the two methods are to be studied with impartiality, they ought to be measured by their respective results in like conditions of pelvic obstruction below the maximum conjugate at which either is admissible, and not by excluding from the mortality of craniotomy its disastrous results to mothers in the higher grades of diminution of the conjugate and the total loss of children at all grades. It does not subserve the purposes of science to limit one to an elective sphere of very narrow proportions, where even its best results will not save fifty per centum of the lives imperilled, and compare such percentage of maternal recoveries with those obtained from Cesarean section and its substitutes extended over a far larger field, and applied to less favorable conditions. But even this method of investigation does not present the external operations performed during the past five years in a less advantageous aspect than craniotomy. Harris has shown that when Cesarean section has been performed during the first twenty-four hours after labor has begun, seventy-four per cent of women¹ were saved and eighty-one per cent of children delivered alive. Dufriellay (Lusk) has shown that in timely operations eighty-one per cent of mothers are saved. In the Santa Caterina of Milan, and the Krankenhaus of Vienna (Harris), the Porro-Cesarean operation has saved seventy-three per cent of women and all the children. The application of antisepsis has so greatly improved the results in other abdominal operations that no one can doubt that it will prove equally beneficial in these. This seems to have been already demonstrated by the immensely (Eustache) more favorable results to both mother and child which have been achieved in recent years. "The effects of antiseptic (Harris) measures and greater cleanliness have been shown by the results of the Porro and other capital operations in large lying-in institutions, and in nothing more than the entire change of results in the two maternities of Naples, in which the old and the new pubic sections have been performed. Perhaps no tabular record of Italian surgery is so much to the point in exhibiting the possible variation of results from unfavorable to favorable as that prepared upon the first, second, and third hundred ovariectomies by Dr. Peruzzi. From having lost nine of the first operations in succession before achieving one success, they have gradually improved in results until now the mortality is reduced to a moderate percentage."

Even when limited to the narrow area of its chosen field of election, craniotomy can never save the percentage of lives already obtained by gastro-hysterotomy and its modifications, nor can it ever attain the brilliant results of the Porro substitute, which for the whole number of operations has saved forty-five and five-eighths per cent of mothers and seventy-seven and one-twelfth per cent of children. In fact, from a later classification by Harris

¹Of the one hundred and thirty-two operations performed in North America prior to July, 1882, forty-five and five-elevenths per cent of women were saved. Harris, *Amer. Jour. Med. Sci.*, vol. lxxxiv., p. 155.

(*Amer. Jour. Med. Sci.*, Oct., 1883, p. 438), it appears that the Porro operation, carried out as originally designed, has saved $46\frac{1}{4}$ per cent of the cases, the Porro-Müller method, unmodified, has saved $52\frac{1}{7}$ per cent, and the two combined $48\frac{8}{9}$ per cent of women, and 90 out of 118 children. In view, then, of the greater number of lives saved, and, in fact, when all the conditions and circumstances are impartially considered, the greater number of mothers saved, the classical Cesarean section and its modifications and substitutes must be regarded as conservators of life. But this is not all; Cesarean section may restore to women incapacitated by pelvic deformity the privilege and power of giving birth to an indefinite number of living children. Lungren¹ has shown that in one hundred and nineteen multiple operations upon forty-eight women, there were only eight mothers lost. In three of these cases, the operation was performed seven times, in two six times, in one five times, in three four times, and in three three times, all of which recovered. The Porro modification has not only saved a larger percentage of lives, but prevents subsequent pregnancies, and therefore in such cases there cannot occur such successive repetitions of craniotomy as related in the case before cited. In the face of such facts, can the most enthusiastic craniotomist continue to class the operation among the elective procedures?² It has been alleged that in occasional instances women have after one craniotomy given birth *per vias naturales* to living children, and such is probably true; but it only proves that either the operation was hasty and unnecessary in the first pregnancy, or that the obstruction was due to disproportion of the fetal head. The rule is that each subsequent fetus has been killed and mutilated, as in the case cited by Harris (see note, page 185), or one of the external operations has been finally resorted to, as in the case of Mrs. Reybold, with equally satisfactory results.³

It is not necessary in this connection to discuss the relative merits of the classical Cesarean section and its substitutes, nor to point out the indications which should give one or the other the preference. Garrigues,⁴ Harris, and others have so fairly and ably reviewed these questions that, even if relevant to the present issue, I could afford to pass them with this reference.

Nor is it necessary that I should review in detail the comparative merits of craniotomy and its alternatives in the minor and rarer forms of obstruction to labor. In the very rare forms of

¹ AMER. JOUR. OBSTET., vol. xiv., p. 78.

² Keyser estimates the mortality from second operations on the same woman at twenty-nine per cent; in the United States it has been twenty-five. Harris, *Amer. Jour. Med. Sci.*, vol. lxxvii., p. 61.

³ We have had women in this country who have endured several hours of suffering under craniotomy, and narrowly escaped with their lives, who were afterwards delivered safely of living children by gastro-hysterotomy. *Amer. Jour. Med. Sci.*, vol. lxxvii., p. 59.

⁴ AMER. JOUR. OBSTET., vol. xvi., p. 337.

pelvic distortion, as in the oblique-ovate, the Cesarean section is generally most strongly advocated. The time has probably passed when any one would hold craniotomy on the living fetus justifiable in cases of cancer of the cervix uteri or advanced phthisis. The results in either case are equally, if not more, favorable when left to nature unaided, or, at most, assisted by the forceps or version. In transverse positions, Harris has shown that in the United States twelve Cesarean sections have yielded nine successes. But neither cancer, malacosteon, exostoses, or uterine fibroids contraindicate Cesarean section or its substitute. In pregnancy complicated with fibroid tumors of the uterus, it has, however, proven very unfavorable in this country, because of the attending exhausting hemorrhage; yet in some it is not only the best but the only possible operation (Garrigues), as in Sanger's case, where a fibroid as large as a child's head sprang from the posterior wall of the cervix.

Prof. G. Eustache (Lille, Belgium) concludes a paper on the "Parallel between Embryotomy and the Cesarean Section," read before the London International Congress, as follows: "Considering, on the one hand, (1) the recent results of ovariectomy and of all other abdominal sections, (2) the improvement in the prognosis of all surgical injuries under antiseptic treatment, (3) the success of Porro's operation, (4) the immensely favorable results both to mother and child after the Cesarean section, which have been published during recent years."

"And considering, on the other hand, that embryotomy, while it always sacrifices the child, exposes the mother to as grave dangers as the Cesarean section; that it is inapplicable in many cases of deformed pelvis, *e. g.*, when the conjugate is five centimetres and under."

"I. When the child is living at the beginning of labor, and when the pelvic strait is under 78 mm.—the extreme limit for the application of the forceps—the Cesarean operation should be performed early, that is to say, as soon as labor has really set in, and with antiseptic precautions."

"II. When the child is dead and the superior strait measures five centimetres, recourse should be had to embryotomy. Below five centimetres the Cesarean section becomes an operation of necessity."

"To sum up, the Cesarean section should be the method of election, embryotomy that of exception."

Harris² struck the keynote of success when he wrote "that the first and most important step is to make the operation one of anticipation and choice rather than one of dire necessity and last resort." The largely increased percentage of recoveries in the timely operations has demonstrated beyond dispute that delay is the chief factor of danger. The exhaustion of the patient and

¹ AMERICAN JOURNAL OBSTETRICS, vol. xiv., pp. 944-5.

² Amer. Journ. Med. Sci., vol. lxxxiii., p. 374.

bruising of the soft parts by long continued ineffectual efforts to accomplish delivery through the natural passages have, in very many cases, destroyed every prospect of recovery before the operation was begun. "A very early operation (Harris) in the United States will save three out of four women and as many children; a moderately late one will lose about two out of three, and one half the children; and a very late operation, that is from three to fifteen days or more after the commencement of labor, will lose three, four or five to one, according to circumstances."¹ The application of antiseptics,² improvements in the method of operating, management of the uterine wound and subsequent treatment of the patient have contributed largely to the better success. And to-day the prospect is so encouraging that even the most skeptical will soon be compelled to accept the results as conclusive.

So much for the obstetric and surgical substitutes for craniotomy. There is another plan of treatment even more conservative than either of the procedures heretofore considered. Nature, says Lusk,³ will, under favorable circumstances, in all but the extreme forms of pelvic contraction, "do her own work with the least expense of infant life, and with a relative small maternal mortality." He cites the fact that, in cases of contracted pelvis in the Dresden, Leipsic and Breslau Maternities, "four hundred and seven spontaneous deliveries took place with the loss of fifty-three children, and, from puerperal diseases, of twelve mothers, the latter representing very nearly the usual mortality in lying-in hospitals." By favorable circumstances he means a "presentation and position of the child's head suited to the form of the pelvis, and a sufficient degree of uterine activity." I have previously incidentally referred to unnecessary and hasty craniotomies and to the dangers of delay in the performance of its substitutes. These can only be avoided by a careful and accurate study of the existing conditions, based upon a complete knowledge of the obstacles presented and applicability of the methods of procedure. The wisest course will always yield the best results, but where to interfere and how to proceed are not always either easily or quickly determined. Human judgment, even when supported by intelligent experience and the highest scientific attainments, is not infallible. The expectant plan of treatment is not mere guess-work, or the hap-hazard conclusion of the tyro, but the deliberate judgment of one who knows what not to do, as well as what to do and when to do it. It is not the sloth of idle expectations, but the masterly inactivity of experience, discretion, and knowledge.

But, after all, the question hangs upon the right of election between two lives at stake. Just so long as this right is maintained just so long craniotomy will have its advocates and operators. The

¹ Amer. Journ. Med. Sci., vol. lxxvii., p. 62.

² Garrigues, AMER. JOURN. OBSTET., vol. xvi., p. 508.

³ The Science and Art of Midwifery, p. 476.

law of justification will always be invoked to cover the plea of necessity. If the life of the mother could certainly and only be saved by the killing of her fetus, and the death of both was otherwise inevitable, the execution of the child might be justifiable as the only alternative. But no such relation of conditions ever did or can exist. Unless delivery is accomplished, both lives will be sacrificed, but the killing of the fetus is not necessary to, and does not guarantee, the recovery of the mother. Neither are the two lives in equal danger. Either may be saved with or without the saving of the other. Craniotomy offers no chance to the fetus, but a reasonable prospect of recovery to the mother. Its substitutes offer three out of four chances to the fetus, and quite equal, or, at most, but slightly lessened chances to the mother. Then it must follow that this right of election subordinates the life of the fetus and the larger number of lives to the possible enhancement of the chances of life to the mothers, and relegates the resources of obstetric science, which offer in the aggregate largely more favorable results, to the category of methods of dire necessity and almost hopeless resort. It is not then the greatest good to the greater number, but the offer of possibly improved prospects to the chosen few. Is that the humanity which the science of medicine should espouse and proclaim to the world as an illustration of its beneficence?

This right of election is furthermore based upon the alleged greater value of the life of an adult woman than that of her unborn child. By whom, and in what manner is this valuation to be estimated? Is it a mere matter of trade and business to be determined always in favor of one because the future and possibilities of the other cannot be known? Are social position, personal qualities and domestic relations commodities of value to be purchased at the cost of the life of the unborn? But it so happens that one such life would rarely liquidate such indebtedness. If the right to take life on such a pretext is indisputable, the number of such sacrifices can only be determined by the number of subsequent pregnancies, and the breeding and killing may go on at the pleasure of the woman and will of the executioner. Even the most uncompromising advocate of the murderous operation could not view such a picture without the utmost abhorrence.

The relative value of two lives cannot be the only arbiter, for every life is of equal value to every holder. It is true that the fetus has not gained an independent existence. It is, nevertheless, true also that in these cases it is the life of a woman who cannot give birth to a living child *per vias naturales*. And such incapacity is the one and only claim of right to destroy the fetus. Can such a right rest exclusively upon such a basis, when it is established that other procedures offer almost, if not quite, equal chances of the mother's recovery, and rescue the larger number of children imperilled?

It is, moreover, claimed that a mother's life is above and beyond any and every consideration of a fetus in utero. A specious subterfuge! But very few, if any, such women could ever become mothers, unless the delivery of a living child is accomplished by one of the conservative methods. Not one of the forty women cited by Lungren could ever have had a child to foster, or could ever have experienced the first pleasure of a realized maternity, but for the Cesarean section or one of its substitutes; and the woman who had submitted sixteen fetuses to destruction might have added one or more but for the timely accident which Providence interposed as its expression of abhorrence.

It is also alleged that the law of moral responsibility imposes the obligation of professional duty to destroy the fetus that the chances of the woman's recovery may be improved. In timely Cesarean sections seventy-four per cent of women, and eighty per cent of children, and in the recent Porro-Cesarean operation seventy-three per cent of mothers and all the children have been saved. These results are comparable with eighty per cent of recoveries of women and loss of all the children after craniotomy. It may be that if the comparison were based upon the results of craniotomy during the last few years, it would be less unfavorable. Surely such a law or duty cannot find its vindication in such data, and its supporters must seek some other defence than results. If such a law or rule of conduct has any foundation at all, it is the unwritten *ipse dixit* of by-gone periods—the outgrowth and excuse of a dire necessity, which science never did accept and can no longer tolerate.

The beneficence of medical science consists in the alleviation of suffering, and the prolongation and saving of life. In the face of the facts hereinbefore presented, there can be no rule of morals or of duty which clothes it with the prerogative to take life as a mere choice of obstetric or surgical procedures, and this too when such choice incurs the responsibility of its repetition, whilst another operation might have either prevented any subsequent pregnancy, or fitted her to bear living children independent of her physical incapacity.

If more attention were paid to the saving and more regard held for the life of the fetus, more women would be saved. The dangers of delay would be avoided, and the life of the fetus would not be lost in ineffectual and unskilful efforts to accomplish delivery of a living child through the natural passages, when the obstruction precluded its possibility. It would demand an early diagnosis of the condition, and invoke a more minute and more general study of the causes of obstructive labor and the proper methods of procedure. The sentiment of a higher responsibility would be infused, and the pride and glory of saving two lives would often-times supplant the detestable act of killing one and possibly losing both.

In conclusion, I disclaim any assault upon individual opinion or

practice. I have endeavored to consider the question in its scientific aspect, and entirely free from the influence of ecclesiastical doctrines. I fully realize the embarrassment of individual cases when a professional brother may be dazzled by the emotional appeals of a false and sentimental humanity, and his judgment made to swerve from a sound discretion and logical conclusions to the adoption of a plan of treatment which the experience of past ages has handed down to us, and vindicated by the assertion of the right to take one life rather than leave two to die. But, in the interest of a broader humanity and a far wider field of usefulness, I would follow the pathway illumined by science and supported by the results of recent progress, and offer chances to two lives rather than take the one which cannot assure the safety of the other.

TRANSACTIONS OF THE GERMAN GYNECOLOGICAL SOCIETY.

(Continued from p. 94.)

Second Day—Afternoon Session.

FREUND (*Strassburg*) in the Chair.

SCHATZ (*Rostock*) read a paper on

THE EMPLOYMENT OF HYDRASTIS CANADENSIS IN GYNECOLOGY.

S. is of opinion that the medicinal treatment of the diseases of the female sexual organs has been crowded too far into the background by the operative treatment; that nowadays the knife is not rarely resorted to in cases in which favorable curative results could be attained by less formidable measures. The author thinks that, especially in functional disturbances of the uterus and ovaries, in menstrual anomalies, direct or reflex nervous, or even congestive troubles, medicinal treatment ought to be tried if the difficulties are but moderate, if an operation is dangerous or mutilating. He calls attention to the fact that often accidental changes in the mode of life, of the climate, psychical alterations, nervous irritations, and finally medication prescribed for other purposes are followed by obvious and unexpected changes in the affections named.

With this view, S. experimented with *Hydrastis canadensis* in menstrual disturbances. He used the fluid extract (supplied by Parke, Davis & Co., Detroit, Mich.) in about fifty cases. Two-thirds of these can be utilized in estimating the value of the drug. In general, it seems to act on the mucous membranes by exciting their vessels to contract. In the female genital apparatus, it seems not only to diminish the blood supply of the mucous membranes,

but to act on them as a whole. It is remarkable that the remedy is often effective in cases in which ergot has failed, or even has rendered the symptoms worse.

Favorable results were obtained by S. mainly in metrorrhagias due to myomata (ergot had long been used in vain), in hemorrhages in the puerperium, in menorrhagias of young persons from fifteen to eighteen years of age, finally also in those forms of endometritis in which curetting had failed. In most cases, he commenced the use of the drug one week before the onset of the menses; where the catamenia recurred with undue frequency, even longer previous to the normal date of their appearance. In several cases, the flow became not only less profuse and shorter in duration, but several times it failed to set in altogether. In the case of myomata, too, the hemorrhages disappeared often for months. The incidental effects of the drug generally were only agreeable in their nature. Particularly noticeable was an increased appetite. Once only a certain lassitude occurred: in another case, states of exaltation. The dose of the fluid extract is about twenty drops three times a day.

SIMPSON (Edinburgh) exhibited his

AXIS-TRACTION FORCEPS.

This instrument is, in the main, formed after Tarnier's model, but has the advantage over the latter of being simpler in construction. S. employs it not only where the head is high. He thinks, since he has used it, he has seen fewer lacerations of the perineum. To a question by Levy (Munich), as to whether, in using these forceps, we did not, during traction, lose the sensation by which we feel convinced that they still grasped the head, he answered in the negative. On the contrary, in this respect the accoucheur feels even more certain than with the ordinary forceps, which forces us to pull and keep the blades together at the same time.

WIEDOW (Freiburg) confirmed Simpson's statements, on the strength of two extractions performed in the Freiburg clinic with Tarnier's model.

HEGAR (Freiburg) made some

COMMUNICATIONS REGARDING MENSTRUATION.

He briefly touched upon the former and the present views on the connection between ovulation and menstruation. He then spoke of the view held by Goodman, an American author, according to whom menstruation is but a partial manifestation of a more extensive and more intense process. G. holds that the life of a woman proceeds in stages. The duration of a stage comprises one menstrual epoch. The latter is divided into two halves. During the one, there is an increase in the vital processes, characterized by augmented heat, more active tissue metamorphosis, greater frequency of the pulse, and higher blood pressure, while the other

half shows a diminution in the intensity of the vital processes. Thus every epoch comprises, as it were, flood and ebb. Menstruation does not occur at the time of the flood, but at that of the ebb. If we choose the simile of waves and depressions, it commences with the transition of the slope into the hollow. The actual proofs which G. has brought forward in support of this theory are very slight.

Subsequently, a lady, Mary Jacobi, in an essay on the dietetics of menstruation, has published a long series of observations on temperature, elimination of urea, and pulse in women. Others have cited these in proof of Goodman's theory. However, only four observations, which, it must be admitted, clearly exhibit the above-mentioned relations, can be actually utilized. In order to furnish any forcible confirmation, we require series of observation continued for a sufficient length of time; determination of the temperature during menstruation, as compared with that shortly previous and subsequent to it. It appears particularly desirable to H. to obtain information as to the relative blood pressure. At his instigation, Dr. von Ott (Petersburg), together with Prof. von Kries, made the required investigations by means of Basch's hemodynamometer (Blutdruckmesser). As gynecological patients are hardly fit subjects, women from the internal clinic furnished the material.

An unquestionable result was the fruit of these investigations—namely, that a material reduction in the blood pressure occurs with or even shortly before menstruation. During the menses, it remains almost always below the mean, and rises again subsequently. Among fourteen cases, there was but a single exception.

Further investigations remain to be made respecting the exact condition during the interval, and in reference to the pulse and temperature. H. does not attribute the cause of the lowering of the blood pressure to the loss of blood.

BANDL (Vienna) read a paper on

THE NORMAL POSITION AND THE NORMAL STATE OF THE UTERUS AND
THE ANATOMO-PATHOLOGICAL CAUSES OF "ANTEFLEXION," SO-
CALLED.

The author first referred to the difference and the changes in the views concerning the normal position of the uterus. He himself endeavored to solve the question as to which position is to be considered the normal one, by careful examinations of the living female, by examination and observation before and during laparotomies, by bimanual examination of the cadaver, both with intact and opened abdominal walls, and by comparative anatomical examination of many uteri.

By examinations (in dispensary practice) of living women he convinced himself that apparent anteversion and anteflexion are the most frequent; also that in a large number of nulliparæ the uterus, when the bladder is empty, lies in anteflexion, as had

been described by B. S. Schultze. Therefore the author believed for some time that slight ante flexion is the normal form and position of the uterus when the bladder is empty; but he came near forgetting that almost all women to be examined in the dispensary come for advice on account of some, even if trifling, morbid condition of their genital organs. Soon he accidentally found in nulliparæ here and there a uterus which was not ante flexed even though the bladder was empty, but remained nearly straight, that is to say, was about in the position represented as normal by Marion Sims and recently by A. Kölliker, and which Langer terms the median position. Such women had absolutely no difficulty with their genitals.

Normally the uterus is found in the pelvic axis when the bladder is moderately distended; when the latter is emptied, it moves forward in such a way that its anterior surface becomes more or less palpable through the anterior vaginal vault for the examining finger, without our being able to demonstrate the formation of an angle forward, on bimanual examination. But many individual deviations are encountered, ranging from a perceptible forward curve to actual ante flexion, without giving rise to any disturbance in consequence thereof.

Hence it becomes somewhat embarrassing to express a decided opinion about normality, abnormality, or disease of the uterus. Therefore, it seems important, in order to have a landmark for the examination of pathological cases, to consider not only the position and form of the uterus, but also to determine the state of the normal uterus in reference to its consistence, flexibility, size, mobility, its canal, its mucosa, the extent of its menstruating surface, and the quality of its surroundings.

The consistence of the fully developed, normal uterus, both in the living and the cadaver, is almost uniform from the point of its insertion into the vagina upward. Only the vaginal portion has a firmer feel in the nullipara. The flexibility, too, is nearly uniform in the cervix and the body, in recent preparations as well as on bimanual examination. The size varies. The mobility of normal uterus is considerable; the possibility of moving it forward, upward, and backward is pretty faithfully shown on injecting rectum or bladder to the utmost with some sebaceous mass. Downward the normal uterus can be readily dislocated by means of a tenaculum almost to the introitus vaginæ without giving rise to any pain.

The canal, if carefully examined with the sound, shows no projecting internal os. A sound of three to four millimetres in thickness, with the usual curve, always penetrates into the uterine cavity without meeting any resistance in a normally situated or stretched uterus. This circumstance seems to B. to be a very important characteristic indication of the normality of the organ. Kölliker likewise has called attention to this fact, as the author has done

four years ago at the meeting. Neither the perfectly normal, non-pregnant uterus, nor that of the primigravida or the primipara, exhibits a decidedly projecting internal os. Such a one is a more or less pathological condition. The external os usually shows a transverse opening, with soft, yielding margins. A sound four millimetres thick easily passes through it. The vaginal portion appears to the touch only about one centimetre long, and is easily engaged in a tubular speculum.

The mucosa of the canal does not bleed at all during the introduction of the sound. In the canal proper is found only a small quantity of glairy mucus. A uterus presenting on bimanual examination the appearance of "anteflexion" differs greatly if all these points are taken in consideration.

The cervix may be found in its normal place, or else it is set farther backward, or backward and higher, or even lower in the pelvis. Usually, however, there is present a lesser or greater retroposition of the parametran part of the uterus. The consistence is not uniform in anteflexion. The entire cervix or merely parts of it feel decidedly firmer than the body. The flexibility, as a rule, has remained normal only in the body. The cervix is found more or less rigid; generally it is larger, in proportion to the uterus. The mobility of the uterus is ordinarily somewhat lessened. Often it cannot be dislocated forward and downward with equal readiness or to the same distance.

The canal of the anteflexed uterus, when examined with a sound three millimetres thick and having the ordinary curve, shows a projecting internal os, or else the sound encounters resistance even lower. Should the uterus remain *in situ*, sounding is sometimes difficult, sometimes quite impossible. We are enabled to perform it by either depressing the sound farther or by pushing the uterus upward from the anterior vaginal vault, or backward from the abdominal walls, or by a greatly increased curvature of the sound.

In such cases, the difficulty of sounding is not due to a simple anterior curvature or a narrowing, but to a retroposition or latero-position, or elevation and retroposition. The latter has been stated by B. S. Schultze to be characteristic of so-called pathological anteflexion. This retroposition of the cervix is generally the result of condensation and shortening of the posterior and lateral parametran tissue and the peritoneum in consequence of terminated morbid or inflammatory processes. In these cases the cervix itself is found either entirely or partially more voluminous, harder, diseased. Sounding is furthermore rendered more difficult in consequence of the multifarious dilatations of the cervical canal caused by the retention of the catarrhal secretion. The point of transition of the cervix into the body, in many cases, is also somewhat narrower, harder, and less dilatable as compared with the uterine cavity.

A resistance to the introduction of the sound is encountered at different points. In nulliparæ and parous women they have generally a definite location. In the former, it is, first, the external os which, although not appearing narrowed to the eye, is yet much less dilatable than normally; second, the internal os; the cervical canal is more or less expanded by increased secretion. Third, a point in the middle of the cervix, where the dilatation by the secretion extends only half way down the cervix. In these cases, an internal os may often be simulated at a more inferior point. In parous women, it is usually only a single spot where the sound encounters resistance; it lies about two centimetres above the external os, but usually it is not the consequence of a constriction, but of the retroposition of the cervix which has been diseased at its lateral and posterior circumference. Still, sometimes, there is a cicatricial narrowing, especially where the cervix had been more deeply lacerated during labor; here it generally corresponds exactly to Müller's ring and to the upper angles of the former lacerations. In rarer cases we find above this a second slight projection corresponding to the os internum of nulliparæ. In rare instances, too, in parous women, the external os is cicatricially contracted and then a part or the whole of the cervix is more or less dilated.

During gentle to-and-fro movements of the sound in the ante-flexed uterus a little blood frequently escapes—a sign that the mucosa is diseased at some point.

In ante flexion the vaginal portion is often apparently elongated, partly because the hardened cervix is more perceptible on examination, partly because the vagina at the insertion of the uterus is drawn higher up, especially backward, by the condensed, shortened parametran tissue, and is more firmly fixed in its deeper layers. Usually the vaginal portion is engaged with difficulty in a tubular speculum, often only the anterior margin becomes visible.

As regards observation during laparotomy, only those rare cases come into consideration where ovarian cysts of rapid growth, not too large, are present in young nulliparæ or virgins. In one of these, B. found the uterus, with axis barely curved forward, inclined toward the empty bladder.

By bimanual examination of the cadaver, B. found ante flexion several times, but the uteri, subsequently dissected out, were quite straight. The same surprising result was found on examination of very many uteri. Among the last-examined two hundred cases, B. found a still present decided ante flexion only four times—a surprising fact, if we bear in mind the frequent occurrence of ante flexion in the living.

A comparative examination of a great number of uteri of the new-born, of children, and of adults showed:

In the *new-born*, the uterus was partly straight, partly in slight ante flexion, lying in the lesser pelvis midway between the disprop-

portionately large bladder and rectum, in such a way that it could hardly be otherwise than in the pelvic axis, even if bladder and rectum be empty. Usually projects farther above the pelvic inlet than in adults. The delicate membranous corpus, as opposed to the firmer cervix, makes an alteration of the axis in the cadaver appear very likely. It is questionable, however, if this had been the case also during life.

In older children, in whom the body of the uterus has acquired greater firmness, that organ is far more frequently found straight. Two uteri from fourteen-year-old girls were exhibited. In one of these, the axis is straight, the peritoneum around the posterior surface of the cervix easily displaceable, the folds of Douglas not very conspicuous and prominent, delicate and soft. In the second, there is a small forward angle of the axis, no simple curvature of the uterus. The parametran part of it (*i. e.*, from the vaginal attachment to the firm connection of the peritoneum in front and behind) is slightly retroposed, the peritoneum at the posterior cervix not so movable, the folds of Douglas more prominent, somewhat thicker and harder.

On the second specimen it can be seen that the uterus, for anatomical reasons, need not come into either great anteversion or antelexion, even if the bladder is empty. Bladder and rectum, moreover, if empty, are far more bulky organs than they are usually represented in schematic drawings. In young persons, the bladder, after evacuation, forms an almost uniform, thick-walled oval, the upper end of which still projects far above the pelvic inlet. In parous women, on account of the greater laxity of the surrounding structures, its shape is more spherical, and does not project far above the pelvic inlet.

The straight form of the uterus is frequent as compared with the antelexed. The anatomical cause of this retroposition of the cervix is a condensation and shortening of the lateral parametran and peritoneal tissues springing from the cervix. B. inclines to the view that this deviation is to be considered morbid, that we see therein the initial stage of the higher degrees of antelexion.

In girls and women, a great many uteri exhibit at the cervix, in its canal, and in its lateral parametran or peritoneal surroundings, or both, patho-anatomical alterations which can only be interpreted as the remains of former slight inflammatory processes. In rarer cases, the disease reaches only to the middle of the cervix; the angle of forward flexion is then situated at that point. Should the disease extend as far as the internal os, the angle of flexion is in the middle of the organ. In both cases the body is healthy. Confirmatory specimens were exhibited. Another one of these shows the just-described changes in the cervix—increased firmness, diminished flexibility, slight narrowing at the internal os, infiltration of the parametran cellular tissue. The cervix posteriorly is more firmly adherent to the investing peritoneum. The

latter, in the pouch of Douglas and the vesico-uterine excavation, is altered by a past inflammation—thickened and shortened in all directions. The latter circumstance materially contributes to the retroposition and the effectuation of the antelexion. This is a condition which can be felt during life very frequently and distinctly,

Such slight affections in the cervix very often cause minor forms of parametritis and peritonitis in its adjoining and also more remote parts, and particularly often, too, in the ovaries. Various specimens were exhibited in proof of the connection of antelexion with morbid processes of the cervix or of the entire organ.

The order in which the disease develops in such cases is as follows: First, affection of the cervix and thereby the occurrence of antelexion; then, disease of the whole organ; after that has run its course, the entire organ rigidly in antelexion; finally, the surrounding peritoneum is implicated.

According to his investigations, B. is of opinion that the cause of antelexion is generally to be sought in an inflammatory disease of the cervix which starts in the mucosa and occasionally extends to the nearest—especially the lateral and posterior—parametran environments, also to the peritoneum and beyond. In practice, however, the recent inflammatory processes are more rarely met with, but the remains of an old terminated inflammation, occasionally of an insidious character, are more frequent.

HEGAR (Freiburg) pointed out that the drawings made from one of Freund's pelvic sagittal sections for the book on operative gynecology edited by himself and Kaltenbach harmonize with Bandl's views. He thought that B. S. Schultze had erred now and then. Bimanual examination easily gives rise to mistakes. Finally he called attention to the fact that there are not only flexions of the body of the uterus, but also of the cervix, and elucidated these remarks by drawings. As to the frequency of inflammations of the cervix and its surroundings, he is in accord with B.

FREUND (Strassburg) is of opinion that the case of Kölliker cited by Bandl cannot be utilized. In the first place, the sexual organs were not yet fully developed; secondly, the abdominal cavity had not been opened with the precautions particularly required in this instance. He, too, believes that inflammatory processes of the parametrium frequently follow those of the cervix.

BANDL (Vienna) also read a paper on

THE TECHNIQUE OF INTRAUTERINE TREATMENT.

There is a series of pathological processes in the uterus and its adnexa which often cannot be surely recognized by bimanual examination alone; moreover, a series of important questions—*e. g.*, whether a cause of sterility is present in the uterus or its surroundings, whether there is a catarrh in the cervix or the body, etc.—which very frequently cannot be decided by bimanual examination alone. In such cases, it is very desirable, after we have convinced ourselves by bimanual examination that no coarser alterations are present, to draw the uterus down with tenaculum or forceps to within about two fingers' breadth behind the in-

troitus vaginae. Sims, Hegar, Kaltenbach, and others have employed this procedure for a long time. B. himself resorts to it especially where it is necessary to answer the question whether intracervical or intrauterine treatment, or whether a bloody or other dilatation of the cervix would be advantageous. As contra-indications to the drawing-down of the uterus, he enumerates the still painful inflammations of that organ and its adnexa, decided pain or resistance on attempted traction, and the presence of adherent tumors perhaps appertaining to the tubes.

For the settlement of the above-named questions, moreover, examination with the sound of the uterus thus drawn down is indispensable. Performed in this manner, it is far less disagreeable for the patient and the uterus, and gives fuller and more certain information as to the healthy or morbid conditions of its mucosa and its canal than when examined *in situ*.

With the sound employed in this manner, it is possible to recognize more positively than by any other procedure to what distance upward the catarrh extends. If the cervix has first been thoroughly cleansed of mucus, and catarrhal secretion is obtained from the upper part of the uterus, either with the sound or by slight opening of Ellinger's dilator, catarrh of the body is likewise present. The latter, according to investigations on the cadaver, is of much rarer occurrence than cervical catarrh. In nulliparae, the latter extends usually as far as the internal os, and in parous women, as a rule, only to where characteristic cervical mucous membrane is still present, to Müller's ring.

Moreover, the cause, and often, too, the seat of a menorrhagia or irregular hemorrhage can be recognized with certainty only in this way. The examination, however, should not be made shortly before or after the hemorrhage. In normal cases, the uterine mucous membrane does not bleed in the least during gentle to-and-fro movements of the sound. Should this occur nevertheless, it indicates a morbid condition. Should menorrhagia exist in the former case, it is usually due to general causes, and no result is to be expected from treating the mucosa. The reverse is true in the other case.

For the performance of intrauterine treatment in the most certain and aseptic manner, B., about five years ago, has devised the following procedure which generally can be carried out without any assistance. If no contra-indication was present, the patients experience absolutely no pain after it.

The patient being in the dorsal position, the vaginal portion is engaged in an ordinary tubular or Sims' speculum and seized with any slender, long-stem tenaculum at the inner surface of the canal, about one-half centimetre above the external os; the point is firmly pressed into the tissue with the aid of the posterior edge of the speculum, or with one finger. If this be done, not a drop of blood escapes when the uterus is drawn down—a matter of importance if

the vulnerability of the mucous membrane is to be tested. The uterus having been thus secured, it is gently drawn down, when the long tubular or the Sims speculum is removed and replaced by a shorter, obliquely-cut one (measuring 7 cm. on the shorter, 9 cm. on the longer side). B.'s assistant, Dr. Heitzmann, facilitated this procedure by slipping a shorter over a longer tubular speculum and removing the latter after the vaginal portion had been seized. With the index-finger and thumb, the uterus, moderately drawn down, is fixed in such a way that the point of the tenaculum always presses slightly against the uterine wall, while the fourth finger of the same hand fixes the upper edge of the speculum. Into the



Sp, short speculum; *Cu*, liquid; *C*, canula.

speculum is poured any liquid appropriate to the treatment of the inner surface of the uterus—if we wish to proceed strictly antiseptically, five-per-cent carbolic acid solution—until the vaginal portion is entirely submerged in the fluid. As may be seen in the accompanying woodcut, while the left hand steadies the parts by the tenaculum and speculum, the right is free to use a sound, a dilator, a canula, curette, bistoury, etc.

B. employs the curette almost exclusively only in those very frequent cases where menorrhagia or irregular hemorrhage is present some time after labor or miscarriage. In them the organ, as a rule, has remained large, the mucosa of the uterus has not recovered uniformly. In such cases nothing is required, usually, but to remove some small, loosely adherent particles of tissue, or more commonly, decidua which has remained attached to the former

points of inflammation. If the uterus in such cases be dilated, the finger generally finds nothing that could be pinched or cut off. B. employs a narrow dull curette, 4 mm. broad, usually without preceding dilatation. He does not deny that dilatation has often a favorable effect on the involution and often is advantageous.

Before curetting, five-per-cent carbolic solution is poured into the speculum, and the curette passed into the uterus through the fluid. With each renewed introduction some of the carbolic solution penetrates into the uterine cavity, and with this we may often observe how the uterus contracts more and more. The effect of the procedure is positive, often instantaneous. It causes no pain to the patient.

The subsequent swabbing of the uterine cavity with liq. ferri sesquichlor. has been almost entirely abandoned by B.

For the treatment of chronic cervical or uterine catarrh he employs, in appropriate cases, a silver canula, 4 to 6 mm. thick, perforated at the sides and point; this can easily be fastened to a rod, as will be seen on reference to the above figure. Into the speculum is poured a ten-per-cent solution of copper sulphate—according to B., a very effective remedy in catarrhs and erosions—and through this is passed a canula of appropriate diameter into the cervix or, in catarrh of the body, into the uterine cavity, and moved slowly to and fro several times. The mucus adhering to the walls is detached thereby. That the fluid comes in contact with all parts of the wall is shown by the more or less active contractions of the uterus which ensue. Instillations of tincture of iron, the introduction of sticks of silver nitrate or other medicated bougies have been almost entirely dispensed with by B. for some years. In apparently very slight alterations of the mucosa he thinks it sufficient to cauterize the uterine cavity with Chiari's porte-caustique, or simply swab it with tincture of iron (by means of cotton, grasped in one of Bozeman's or a small dressing forceps). This, too, can be done more certainly and gently while the uterus is drawn down.

Frequently the effect is tedious, because, in catarrh of the uterus generally, its tissue is likewise diseased, the glands dilated, their efferent ducts narrowed. If these alterations still regress, this takes place but slowly. A sometimes disagreeable incidental result is slight hemorrhage after the procedure. This will be avoided if the canula chosen is not too thick. More active caustics are to be eschewed, for they too often give rise to cicatrization.

The procedure is employed by B. only with women who have a wide external os. Now and then Müller's ring requires to be dilated. In cervical catarrh it is not always necessary to draw down the uterus. The ordinary tubular speculum may likewise be used in the treatment. But then, in order to be able to introduce a canula, the corpus uteri must be pressed backward, from the abdominal walls, into the axis of the cervix which in such cases is nearly always retroposed.

(To be continued.)

REVIEWS.

TRANSACTIONS OF THE AMERICAN GYNECOLOGICAL SOCIETY. Vol. 7, 1882. Philadelphia: Henry C. Lea's Son & Co., 1883.

These Transactions are always welcome, however tardy their appearance. They have always hitherto reflected well the advances made in the surgery of women, and the present volume is fully on a par with its predecessors. Indeed, the papers are, with few exceptions, eminently practical, and all deserve careful reading. The annual address of the President, DR. EMMET, was devoted to a consideration of the Pathology and Treatment of Certain Lesions of the Female Urethra, and he points out clearly the advantages to be derived, both from the side of diagnosis and treatment, from making a button-hole opening into the urethra. If DR. JOSEPH TABER JOHNSON, in his paper on the Proper Use of Ergot in Obstetrics, holds rather extreme views on the subject when he says it would be better for the human race if ergot should be entirely abolished from the lying-in room; it must be granted that he has otherwise set the proper limitations to the use of the drug, and that his paper will have served a very useful purpose if from it many practitioners will learn the difference between legitimate use and unwarranted abuse of this most powerful and yet, within limitations, invaluable drug. DR. THOMAS M. DRYSDALE makes a further plea in favor of his well-known views in regard to the diagnostic value of the ovarian cell; DR. SUTTON reviews, in brief, the Treatment of the Pedicle after Ovariectomy, from the dawn of the operation till the present, and summarizes the conditions which, in his opinion, will insure success or lead to failure; DR. BARKER emphasizes the fact that leucorrhœa is frequently symptomatic of constitutional causes, eventually becoming a cause of local disease; DR. PARVIN's paper on Care of the Perineum is eminently practical in character, and so elementary, indeed, as to lead one to suppose it to be a chapter from his prospective work on "Obstetrics." It is to be regretted that he clings to the term "supporting" as a means of preventing rupture. The perineum must relax to allow of the birth of the child; why not, then, use the term "relaxation," since this should be the true aim of the obstetrician? The Value of Hysterectomy for the Cure of Uterine Fibroids, Compared with the Complete Removal of the Uterine Appendages, is the title of MR. J. KNOWSLEY THORNTON's paper. His conclusions are that the latter is far the safest operation, and is indicated, in all cases requiring interference at all, in preference to the former. The hysterectomy is only indicated where removal of the appendages has been tried and has failed. The two following papers deal with Extrauterine Pregnancy; the one by DR. GARRIGUES is a synopsis of the cases treated by electricity, including a successful personal case; the other, a report of twenty-one cases coming under the personal observation of the writer, DR. T. GAILLARD THOMAS, closing with some valuable deductions as to treatment. DR. BUSEY's paper on the Modifying Influence of High-Heeled Shoes on the Female Pelvic Organs is of value from a scientific standpoint, and, if placed within the reach of womankind, might be the outcome of practical good; DR. VAN DE WARKER's essay on the Mechanical Therapeutics of Versions and Flexions of the Uterus is a complete resumé of their supposed and, from the

author's standpoint, real action, concluding with a classification of pessaries and a thorough representation of their manifold and often curious shapes; finally, we have DR. J. COLLINS WARREN's new operation for the repair of rupture of the perineum through the sphincter and rectum; two more series of measurements of the uterine cavity in child-bed by DR. W. L. RICHARDSON; a review of surgical operations on the pelvic organs of pregnant women, by DR. MATTHEW D. MANN; a practical paper on Hyperemia of the Vesico-Urethral Membrane, with full report of the histories of five cases relieved or cured by the formation of a vesico-vaginal fistula by DR. W. H. BAKER. A graceful tribute to the memory of Professor James Platt White from the pen of DR. T. GAILLARD THOMAS.

This cursory survey of this volume must at least reveal the fact what valuable work the members of the Gynecological Society are doing, and how much the record of their work adds to the knowledge of the profession at large, and, in consequence, to the relief of suffering woman. The customary bibliographical index for 1881 is appended to the volume.

EGBERT H. GRANDIN.

TRANSACTIONS OF THE AMERICAN GYNECOLOGICAL SOCIETY FOR 1883.
Vol. 8. New York: D. Appleton & Co., 1884.

This volume opens with an interesting sketch of the life of Dr. Nathan Smith, constituting the annual address of the President of the Society. The paper on Superinvolution of the Uterus, by DR. JOSEPH TABER JOHNSON, which follows (by the way, Dr. Johnson seems to lead the van in these meetings), consists mainly in a brief analysis of four cases occurring in the writer's practice, and where he was unable to accomplish any more towards the relief of the condition than has fallen to the lot of other observers. In the discussion of this paper, a practical point for guidance in prognosis made by Dr. Barker is worthy of emphasis—namely, that as long as there is evidence of continuing ovulation, as shown by the menstrual moulins present at each period, we may hope, through judicious treatment, to restore the uterus to its normal size. Those cases are highly unfavorable where, together with superinvolution, arrest of ovarian development or cessation of ovarian function is associated. DR. SUTTON's essay on Cleanliness in Surgery is an earnest plea in favor of complete antisepsis, howsoever obtained, barring the use of the spray over an open wound in the abdomen; an antisepsis, however, depending as much, if not more, on minute cleanliness in each detail as on the promiscuous and free use of carbolic acid. The value of hot water in secondary hemorrhage is strikingly evidenced by a case which finds place in DR. ALBERT H. SMITH's paper on this subject. The object of DR. PALMER's paper on Dysmenorrhea was to call out an expression of opinion from the members from the side of treatment. Notwithstanding the fact that difficult menstruation has ever been a fruitful theme for discussion, exact ideas as to the pathogeny of certain forms of the affection are yet to be obtained before we can see our way towards rational treatment. Dr. Palmer is forced to admit that an obstructive dysmenorrhea does exist, though rarely, and, when such a case is found, the treatment indicated is incision. And this is an opinion towards which observers generally are tending. DR. REAMY reported three cases of a rare form of abdominal tumor, and presented one specimen. The rarity consisted in the fact that the tumors were omental, in the fatal cases, malignant; and thence the conclusion is drawn that, granting the identity as to site in the

three cases, the two which recovered might have become malignant had they not been treated. DR. JENKS' contribution on a new method of operating for fistula in ano, and DR. CAMPBELL's exposé of a rare condition—congenital fissure of the urethra with exstrophy of the bladder—are of a practical nature and deserving of study, as is also DR. LEE's paper on The Management of Accidental Puncture and Other Injuries of the Gravid Uterus as Complications of Laparotomy. DR. A. REEVES JACKSON asks the question if Extirpation of the Cancerous Uterus is a Justifiable Operation, and, after an extended analysis of the facts bearing on the operation at disposal, answers it in the negative, for the reason that, as yet, sufficiently early diagnosis cannot be made to insure complete removal of the growth by extirpation of the uterus, and that when the diagnosis is assured, it is too late to effect a radical cure. Whilst in these deductions he does not stand alone, his further proposition that the operation is highly dangerous and does not lessen suffering except where it kills, whilst true when limited to Freund's operation, cannot consistently be maintained against the vaginal method of extirpation. DR. EMMET's new operation for the repair of the perineum being now in print, can be fairly judged; DR. BYFORD's Remarks on Chronic Abscess of the Pelvis are of value pathologically, and from the side of treatment; DR. ENGELMANN's Contribution on Ergot, although containing absolutely nothing new, may be read to advantage by many a practitioner, and DR. MACRY's paper clearly traces the interdependence and the relationship existing between the pelvic organs. The volume closes with a memoir of James Dowling Trask written by FORDYCE BARKER.

It is impossible to close this notice without expressing the hope that future volumes of these Transactions will appear as promptly after the meeting as has the present.

E. H. G.

ABSTRACT.

1. Jungbluth (Aix-la-Chapelle): On the Treatment of Placenta Previa (*Volkmann's Sammlung*, No. 235).—As a general principle, says the author, the membranes which cover a fetus should not be ruptured until the os is nearly or quite dilated. Theoretically, this is true in cases of placenta previa, but practically it is not always so, since hasty delivery, according to most recent writers, is requisite *whenever* the bleeding becomes dangerous. Much of such teaching, however, is based upon the mistaken hypothesis, that in placenta previa an effective stoppage of hemorrhage can only be accomplished by the emptying of the uterus, without regard to the fact that other dangers which threaten both mother and child are slightly passed over as unavoidable ills of lesser magnitude. Dissatisfaction with methods in vogue induced the author to adopt the plan of treatment which he describes in this paper. The following advantages are claimed for his method: (1) uniform simplicity in

adaptability, and absence of danger in all cases of placenta previa centralis and lateralis, only those cases being excepted which continue to the fourth month of pregnancy and then suffer abortion; (2) immediate and permanent stoppage of hemorrhage during the period of active pains, until the os uteri is completely dilated, and avoidance of post-partum hemorrhage; (3) preservation of the fetal envelope until the os is entirely dilated, avoidance of rupture of the cervix, and facilitation of turning and extraction of a living child in cases in which such an operation is necessary; (4) maintenance, and even improvement, of the powers of the parturient woman, from the moment in which treatment begins, and lessening of the danger of collapse from anemia of the brain at the moment of the child's birth, an accident which frequently threatens. The potent means for accomplishing this consists in nothing more nor less than the efficient tamponade of the cervical canal with sponge tents. In spite of the almost universal prejudice against these instruments, the author claims that they can be so made as to be perfectly aseptic. The method is as follows: A portion of fine-mesh sponges, of different sizes, is beaten thoroughly with a wooden hammer, and then washed for eight or ten minutes with a two-per-cent solution of hypermanganate of potash. The washing is repeated in a two per cent solution of binoxalate of potash until the mass has assumed a yellowish-white appearance, and until no sediment remains in the solution. Finally it is washed in distilled water, until the addition of limewater fails to give an oxalic acid reaction. This method will also entirely remove the peculiar odor of sponge. The mass is then soaked for two days in a five-per cent solution of the purest carbolic acid. The separate sponges are next squeezed two or three times in a ten per cent solution of the best white gum arabic, and are strung lengthwise upon a piece of tin wire. Carbolyzed thread is then wound around them, and the wire is removed. They are next dried in a warm place, between leaves of blotting paper, and when absolutely dry, the surrounding thread is removed. Sponges thus prepared were found to be thoroughly aseptic after the most careful and accurate tests. Before they are used in a case of placenta previa, the vagina and vulva are first disinfected with a warm two per cent solution of carbolic acid. All clots are removed, and one or more tents, according to the nature of the case, are thrust sufficiently high into the cervix, and are allowed to remain, on an average, from six to eight hours. Should hemorrhage occur within an hour after the introduction of the tents, they must be replaced by others which will more effectually close the canal. The patient will be gaining strength while dilatation is going on, with rest and freedom from hemorrhage. After the tents have accomplished their work, the dilatation must be continued by means of others, with the same antiseptic precautions, which are to be retained from four to eight hours. Two series of tents will usually be sufficient, but a third should be employed if necessary to cause full and complete dilatation. After this last-mentioned stage has been reached, the hand is to be passed into the womb, pushing aside enough of the placenta to accomplish this, the feet are to be seized, the membranes ruptured, and the fetus turned and extracted as quickly as possible. The navel string being cut, complete removal of the placenta is to be effected, if nature have not already cast it off. The condition of the child will be the same, good or bad, which it was before the dilatation was commenced. When the tents are removed,

usually a few clots are found in the lower uterine segment. Should hemorrhage occur from the premature withdrawal of the tampon, the entire uterine cavity must be tamponed with three or four of the larger sponge cylinders, and, after six or eight hours, these may be withdrawn, one by one, the process being always preceded by disinfection of the vagina. The foregoing applies equally well when the placenta is central, and when it is lateral. If the head should present, after complete dilatation, and active pains exist, nature may accomplish the delivery, turning not being requisite. Frequent doses of ergot are useful to anticipate contingencies, and it should be given, if in the powdered form, in doses of eight grains. A detailed report of seven cases follows, in which the author carried out his method with perfect success. All the mothers recovered, three of the children were either dead at birth, or died within a few days. The history of these seven cases is an interesting study.

A. F. C.

THE SIMS MEMORIAL FUND.

To the Medical Profession and Others throughout the World :

The great achievements of Dr. J. Marion Sims call for some lasting testimonial than obituaries and eulogies. To him medical science is indebted for much brilliant and original work, especially in gynecological surgery. Those who have been benefited by his teachings and new operations, and such as have had the direct advantage of his personal skill are among the first to recognize and acknowledge this debt.

To him is due the honor of giving the first strong impulse to the study of gynecological surgery in America.

It is believed that the medical profession everywhere, the vast number of women who owe their relief from suffering directly to him, and those who realize the benefits he first made possible, will gladly unite thus to honor the man through whose original and inventive genius such blessings have been conferred upon humanity.

At the suggestion of many friends, therefore, the subjoined committee has been organized, and it is proposed that a suitable monument be erected to his memory in the City of New York.

To this end the active co-operation of the medical profession, and the many other friends of Dr. Sims throughout the world, is respectfully solicited. Contributions of one dollar and upward may be forwarded to the journal which has been constituted the treasury of this fund.—THE MEDICAL RECORD, New York.

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DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

TWO CASES OF MALPOSITION OF THE KIDNEY.

BY

WILLIAM P. NORTHRUP, M.D.,

Pathologist to the New York Foundling Asylum.

I DESIRE to record two cases of malposition of the kidney which occurred in female children. The permanent displacement is of such a nature as to be of possible interest to the accoucheur.

One history will serve for both; both were females; both three months old; both inmates of the New York Foundling Asylum; both died of chronic gastro-intestinal catarrh in wretched condition of emaciation; both were without symptoms referable to the urinary tract. In both the malposition was of the left kidney. Both were found in the same month.

Malpositions and abnormalities were as follows:

CASE I. (Fig. 1).—Suprarenal glands both in normal position, without reference to kidneys.

Right kidney in normal position, its vessels of normal distribution.

Left kidney. Its superior margin on a level with inferior margin of right, lying mostly in hollow of sacrum, perhaps one-eighth extending above the prominence. Kidney firmly attached by vessels and connective tissue.

Arterial Supply.—(1) Small artery given off from angle of bifurcation of aorta entering kidney posterior to and to the right of upper end. (2) Small artery arising on anterior aspect of aorta and just above the bifurcation, which reaches the hilum

after passing through a groove on anterior surface of kidney. (3) Much larger artery given off from internal iliac just below its origin (not seen in cut), which passes by a short course into the hilum.

Veins.—One leaves the hilum in the same groove by which artery (2) reaches it, passes under the bifurcation of the aorta, and joins the vena cava.

Ureter.—About half the length of the right, its arrangement normal.

CASE II. (Fig. 2).—Right kidney in normal position, surmounted by its suprarenal gland.



FIG. 1.

In each kidney there is a double hilum, or two distinct, each supplied with its own arteries, veins, and ureter.

Arteries.—One given off from aorta just above its bifurcation, itself bifurcates; one branch going to each hilum. This passes in front instead of behind the great vein.

Veins.—(1) One from the upper hilum going directly to the inferior vena cava. (2) An anomalous vein which sweeps up from the iliac fossa over the kidney, receiving a branch from each hilum and one from surface of kidney.

Left kidney. Position of suprarenal gland not noted. Kidney is situated one-half above, one-half below the prominence of sacrum in the median line.

Arteries.—(1) Small, arises from angle of bifurcation of aorta, passes to lower hilum through a deep groove on inner side of kidney (not shown in cut). (2) Moderate-sized, arising from anterior aspect of aorta just above its bifurcation, itself bifurcates, one branch going to upper hilum, one to extreme upper end of the organ. (3) Two small arteries given off from internal iliac, reaching the lower hilum by way of two deep grooves.

Veins.—(1) Small vein, companion of artery (2) above described, arises from two sources, upper hilum and extreme end of the kidney, passes behind the aorta, and joins the great vein just



FIG. 2.

above the junction of the two iliacs. (2) A vein from lower hilum, winding around in a deep groove, finds its way to the vein of the common iliac.

In the *Glasgow Medical Journal*, August, 1883, David Newman, in a thesis for his degree, reviews the literature of malposition of the kidneys. In 300 autopsies he found malposition in eight. Of these only one is of practical interest, the variation from the regulation position of a student's manual

being so small. "In the case referred to, the right kidney was situated about one-half inch to the right of the prominence of the sacrum, and the left in the iliac fossa two inches to the left of sacro-iliac synchondrosis." Right kidney was also malformed.

The bibliography given in Dr. Newman's article is very complete, and to it the reader is referred.

The two cases here reported occurred within a month of each other, early in my dead-house experience, and, like the boy who finds a horseshoe in the road, I've been looking for them ever since.

My report, then, stands thus: In 550 autopsies at the New York Foundling Asylum, marked immobile malposition of of the kidney has occurred in two cases.

AN ANOMALOUS KIDNEY.

BY

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THE following case recently came under my observation: History.—A. B., female mulatto, aged four months; was perfectly healthy until a couple of weeks before death, when she contracted pertussis. The paroxysms of coughing were unusually severe, and the case terminated fatally by convulsions. There was never any trouble with the urine, either before or during the illness.

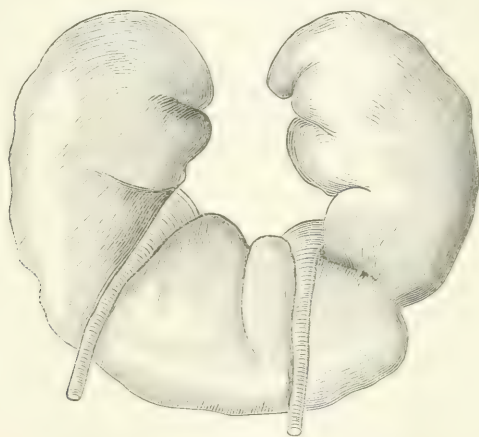
At the autopsy there was found a horse-shoe kidney, the appearance of which is well represented in the annexed cut. It lay in the normal position, with the concavity looking upward.

As may be seen, it consists of three separate kidneys, the middle one united to the two lateral by the extremities. There are two ureters, one passing from the right kidney in a normal manner, while the other passes from the junction of the middle and left.

On examination, the parenchyma of the triple organ was found to be continuous, there being no more pronounced line of separation between the bodies than the thin cortical layer enveloping each pyramid of Malpighi, demonstrating the fact that, when at an early period the three organs lay closely together in front of the bifurcation of the aorta, they coalesced; in other words they fused

together after the manner in which the separate lobules unite normally to form the kidney.

It is needless for me to describe the development and structure of the kidney, which is certainly familiar to every one acquainted with the rudiments of anatomy. I will merely recall a few points, that normally there are two of these organs situated one on each side of the vertebral column, in the lower dorsal and upper lumbar regions, behind the peritoneum. The right one is a little lower than the left, probably due to the proximity of the large lobe of the liver. That on making a



longitudinal section of an adult kidney, it is seen to be divided into a cortical and a medullary portion, the latter consisting of conical masses called the pyramids of Malpighi. The bases of these are fixed in the cortical substance, which, coming down between them, forms a layer of separation, and entirely envelops them except at their apices, which project outward into the sinus.

In the primitive state of the kidney it is composed of lobules which coalesce with the growth and development of the organ, and the gland is formed. Each lobule consist of one of the pyramids of Malpighi surrounded by cortical substance, which, when they unite, forms the partition between them.

Occasionally an anomaly occurs, consisting usually in the

union of two kidneys, and the resulting body is styled a *Horse shoe* kidney.

The most common variety is when the two organs are united at their inferior extremities with the concavity of the mass looking upward.

Very rarely junction may be effected at the upper ends, with the concavity looking downward. A fusion may also take place at the two hili, the extremities remaining free.

The bond of union is generally of the proper gland substance; it may, however, consist merely of condensed fibrous tissue.

Associated with the change in form, we sometimes find a change in position of the kidney; they may both lie on one side of the vertebral column, or may have descended into the true pelvis.

After a rather extensive search, I have failed to find any published account of just such an anomalous kidney as this; *its chief peculiarity being the fusion of three organs without interruption of structure.*

CORRESPONDENCE.

INTRACTABLE ULCER OF THE TONGUE CURED BY WEANING.

ABOUT the first of August, 1883, an infant, a male, aged twelve months, was brought by its parents to me for treatment. Nearly the entire under surface of the tongue forward of the frenum constituted a pearly white ulcer. A number of remedies were tried, including, finally, chromic acid, with only temporary benefit, until the latter part of October.

About that time I chanced one day to observe the child while it was nursing, and discovered that the tongue glided over the lower central incisors—the only teeth in the lower jaw—during the act, and so a constant source of irritation was the result.

At my request, the mother at once weaned the child, and with-

out any further treatment the ulcer healed in about two weeks.

Possibly these lines may assist some one to solve a similar mystery.

G. W. H. KEMPER.

MUNCIE, INDIANA.

ABSTRACTS.

1. Riehl: Pemphigus Acutus (*Wien. Med. Wochschr.*, No. 51, 1883).—Though Hebra denied the existence of pemphigus acutus on the ground of a rich experience, yet there seems no doubt of its existence, and even Kaposi admits it, and his assistant, Dr. Gustav Riehl, reports the following case, which is of interest as offering, perhaps, a new point of etiology and of differential diagnosis from other diseases accompanied by the formation of vesicles. A boy fourteen days old was brought to the clinic. The history was that on the sixth day a bulla as large as a hazelnut appeared on the neck. This soon filled, broke, became flat, and then was folled by others on the body. The child was nursed by the mother, and the family history was free from taint. Two days before admission, aphthæ appeared in the mouth. On admission, the boy was well-nourished, and showed on the brow, the cheeks, and lower extremities irregularly scattered bullæ, from the size of a pea to that of a dove's egg, filled with a clear, or wine-colored, or somewhat cloudy fluid, with thin walls, which broke on the slightest pressure. Between the bullæ were many broken ones covered with the remains of the thin walls or with crusts. On the sides of the neck and front of the body almost the whole epidermis had been loosened by the confluence and great number of the bullæ, and presented the appearance of a burn of the second degree. The edge of this surface was made up of segments of circles, convex outward, and surrounded by small halos of active redness of the skin. The isolated bullæ also showed these erythematous, scarcely raised edges. In the groins and by the scrotum, the skin was red, slightly swelled, in a state of eczema intertrigo. There were a few aphthæ in the mouth. The internal organs were normal; there was no fever and pain; only on touch. The treatment was simple—powder of starch, lime water, and oil, aa on the excoriated parts, etc. I have given the details of the case thus to show—as did also the further course—that there was no doubt of the diagnosis of pemphigus acutus neonati (pemphigus benignus.)

The contents of the bullæ reacted alkaline, contained epithelium cells and a few white blood-corpuscles. In the walls, as well as in the portions of epidermis dried up with the secretion, there were found a large number of *fungous elements*, both the arrangement and amount of which show they could not have been accidental. They were found in every preparation. First, there were larger or smaller irregularly shaped heaps of gonidia (containing from ten to one hundred). These were *pushed in* between the layers of the epidermis, mostly situated by the openings of

follicles, but also sometimes independent of the latter. Sometimes, the hair canal or the sweat duct was completely blocked by them. Sometimes from two to twelve of them were arranged in chain form. Threads of mycelium were also found, either isolated or inclosed in the gonidia, though in much less abundance than the latter. The gonidia resemble those found in trichophyton tonsurans, but their great number as compared to the mycelium and their peculiar collection in heaps reminds us less of the herpes tonsurans than of pityriasis versicolor; but the author believes the points of difference from each of these are so great that we must consider these as fungous elements of a separate species. There was such an abundance in every preparation made (over twenty) that accidental uncleanness may be excluded (schimmel-pilze or mould-fungus), and the growth into the follicles in fresh specimens shows they existed there before the development of the bulla.

It is known that vesicles and bullæ form on children's skins from very slight irritation (eczema bullosa). In one case at the clinic, a herpes tonsurans spread rapidly from a small spot on the neck, over the back, in the form of large bullæ, reminding strongly of pemphigus, though there was an abundance of the fungus considered characteristic of trichophyton tonsurans.

The author, therefore, believes that in the case reported the fungus was the exciting cause, though there is no strict proof of its transmission from or production of other cases of the same sort. The occurrence of pemphigus in epidemic form and the limitation of it occasionally to the children cared for by one midwife would seem to show that it is a contagious disease. Its feverless course, its relative harmlessness, its strikingly slight constitutional disturbance, make it probable that the virus is limited to the skin. In spite of all these arguments for contagiousness, the contagium has not yet been found, and all attempts at inoculation with the contents of the bullæ have failed. The supposition of a fungus in the epidermis, as found in this case, would explain all the symptoms of pemphigus, and clear up many heretofore cloudy points. Thus, the feverless course, the spontaneous disappearance of the disease after the gradual decrease in number and size of the bullæ—analogous to the course of herpes tonsurans vesiculosus—the frequent transmission of the disease by midwives and nurses, the negative results of inoculation with the contents of the bullæ, even the fact (mentioned by Bohn as arguing against contagiousness) that in adults attending pemphigus children vesicles occur on the breast, lips, fingers, tender places, but never as a general eruption: all these are explained by the presence of a fungus in the epidermis. The tender skin of new-born children, especially in the period of desquamation, offers a soil for rapid growth and extension to such a fungus, and is easily irritated by it, while in the adult it can only find place on tender or macerated skin, and so only cause localized eruption.

The author does not claim to have at all established his theory by this one case, but his researches should certainly lead to others, both in the line of microscopic study of the epidermis in the cases, and also further attempts at inoculation of the fungus found in the epidermal layers, rather than of the contents of the bullæ.

J. F., JR.

2. Hartigan: Trismus Nascentium (*Amer. Journ. Med. Sci.*, Jan., 1884).—A discussion arose in Washington in 1877 over numerous cases of so-called trismus nascentium, and Dr. J. F. Hartigan began then to investigate the matter with thoroughness. The first point noticed was the uniformity of the post-mortem conditions—extravasation of blood and congestion of posterior part of brain or spine, or both. The position of the bones was next noticed, and the question arose, Did the almost uniformly existing depression of the occipital bone cause the disease? This theory was advanced thirty years before by Dr. J. Marion Sims, and he held it up to the time of his death. The author's object is to endeavor now to establish this theory, with some additional facts. He devotes a large part of his article to a very exhaustive resumé of the literature of the subject, which only goes to show how varied, how contradictory, how strained, often how absurd have been the causes given for this deadly disease, for all the authors given agree in one point at least, namely, that the trouble, trismus nascentium, tetanus nascentium, nine days' fits, is almost uniformly fatal. In looking over the long list of quotations and opinions, I find that there is no cause advanced by any author which is not flatly, and apparently on good grounds, contradicted by some other writer. Perhaps we should except from this the author's own theory, that of Dr. Sims. And even of this, Dr. J. Lewis Smith says that if Dr. Sims' mode of explanation was correct, the convulsions would occur sooner; compression of the medulla would certainly be followed by immediate and marked symptoms instead of an immunity for four or five days. He writes: "After its publication" (Dr. Sims' paper), "cases were related in which there was no occipital depression. In my own, it was sometimes noticed, but in no instance did the depression seem to be a cause, but a result, and it became more marked as the disease advanced. The correct explanation is probably as follows: If the new-born infant becomes emaciated, the volume of the brain is diminished like that of the trunk or limbs, and the sinking of the occipital bone simply corresponds with the amount of waste in the cerebral substance. In fatal tetanus nascentium, emaciation is very rapid. Viewed in this light, the occipital depression, so far as it has any effect, must be regarded as conservative." Other causes, such as filth, bad ventilation and hygienic surroundings, climate, heat, cold, race, smoke (Dr. James Clark), contagion, night air, exhalation from the ground, use of liquor by parents, and many others may all be regarded as positively disproved. There remain the two theories of origin; first, from the umbilicus, second, from pressure of the bones. The arguments against the former are strong. They are: First, the absence of any anatomical lesions which point to origin here; second, the occurrence of cases of trouble with the umbilicus in which there is no trismus; third, the contradictory statements of authors as to what conditions of the umbilicus and what methods of tying the cord are productive of the trouble; fourth, an argument, made by Dr. W. L. Sutton, upon the period of commencement of the disease after tying the cord compared with the period of commencement of tetanus after a wound, and other arguments to disprove the analogy with traumatic tetanus; in short, a weight of evidence from the most numerous and varied sources, all tending to show that while there may be such a thing as tetanus due to trauma of the umbilicus, yet this is not trismus nascentium, and the latter can scarcely ever be traced to

navel disease. It is surprising to one who has been accustomed to think of these cases as due to some carelessness with or some affection of the umbilicus to see what a mass of argument and opinion the author has collected against this theory.

The second theory, that of Dr. Sims, has also found opponents, and, perhaps, one strong argument against it is that it has found so few supporters. The author, however, has found a few writers who have believed in it, and *been successful in treatment according to it*, which is a great point when we consider how fatal the disease is.

In his second paper, published in 1848, Dr. Sims modified some of his statements as to deficient ossification, etc., but says the position is fully sustained: "That trismus nascentium is a disease of centric origin, depending on a mechanical pressure exerted on the medulla and its nerves; that this pressure is the result, most generally, of an inward displacement of the occipital bone, often very perceptible, but sometimes so slight as to be detected with difficulty; that this displacement of the occiput is one of the fixed physiological laws of the parturient state; that when it exists for any length of time after birth it becomes a pathological condition, capable of producing all the symptoms of trismus nascentium, which are relieved simply by rectifying this abnormal displacement, and thereby removing the pressure from the base of the brain." He also describes cases of "trismoid," or chronic trismus in which the displacement is slight, and cases of lateral displacement, in which the occipital bone is above the parietal, the latter pressing inward. Now, Hartigan's experience goes to substantiate these theories. He has divided his cases into three classes: 1st. Those that recovered under the postural treatment. 2d. Cases in which post-mortem examinations were made, many of which were seen before death, but too late to render any assistance. 3d. Those whose history was obtained after death, the majority also having been seen before fatal results ensued, but where post-mortems were not permitted. There is also a statistical table of 229 deaths occurring in the first month. About 150 autopsies were made, but only 43 are reported. It would add interest to give some of the author's cases, some of his "postural cures" being really remarkable, but there is no space. His tables show that the disease prevails equally in warm and cold weather, about equally among males and females (disproving of the latest theory of adherent prepuce as a cause), and at ages varying from birth to twenty-eight days. The shortest duration of the disease was one hour, the longest thirty days. In discussing his cases, the author first disposes of the identity with tetanus, first by the arguments already mentioned, then by the presence of fever in trismus and its absence in tetanus, and then by the difference in post-mortem appearances. There should be no confusion of this disease with eclampsia or with congestion of the brain or apoplexy. Then follows an elaborate argument against the umbilical theory, piling up even more evidence than in his literary resumé, showing that there are no nerves in the cord (Delafield), and that reflex irritability is therefore impossible.

The other causes having also been disposed of in a very able manner, Hartigan arrives at the object of his paper, "the establishment of the true cause and the prevention and cure of trismus nascentium." Experiment and experience show that pressure on the brain causes convulsions, their duration and frequency depending on the degree of pressure. In

acute trismus, when the convulsions continue a certain length of time, extravasation occurs and the case is hopeless. Where one side is overlapped the disease is milder, and when the displacement is hardly perceptible the symptoms are apt to continue many days. Before birth the child's head is cushioned in the liquor amnii. At birth and for some time afterward the cranial bones are not united, are thin and without diploë. They may be easily displaced by the mother's allowing the child's head to rest on her arm during nursing, or by placing it on its back on a hard cushion or with an old quilt or a bunch of clothes wadded up and stuck under the occiput. In one of Dr. Sims' cases the child would not nurse, the mother placed its head on her arm, and when it fretted and would not take the nipple she pressed the head against the breast, only increasing the difficulty. The doctor took the head in his hands, compressed the parietal bones and relieved the pressure on the occiput, and it nursed ravenously. Pressure was made on the occipital bone, and instantly the old trouble returned. The modern baby carriage is a fruitful cause of sickness, often called diarrhea, tabes mesenterica, meningitis, cholera infantum, etc., but in reality due to pressure on the posterior brain. Many interesting and remarkable cases are detailed. It is not necessary to the development of trismoid that the bones should be visibly displaced. Symptoms may arise from continued decubitus, to subside when the child is taken up and recur again when it is laid down. Dr. Baldwin has claimed that the variations in the post-mortem appearances are not consistent with Dr. Sims' theory, but the author explains these by the varying degrees of pressure. In all cases there was congestion of the posterior brain or cord or both. In rapid cases there were coagula in upper part of canal in the cavity of the arachnoid—these coagula being sometimes absent in cases so rapid that death was really caused by asphyxia, as shown by congestion of the lungs, etc.—while in these latter cases, as well as in many of the others, there was a varying quantity of reddish, gelatinous lymph in the same location (Meigs' and Pepper's proliferation of connective tissue?). In some cases, the postural treatment (which needs no explanation) is not sufficient. The depressed bone must be raised. Some successful cases have been recorded by Drs. Harrison and Hart. Dr. Sims used a shoemaker's awl. Dr. Harrison cut down on the edge of the parietal. The difficulty is to retain the bone in place after it has been raised. The fronto-parietal position should be employed. Hartigan believes that many still-births are due to this same occipital pressure. He calculates that 25,000 children die annually in the United States from this disease, believes that many cases may be saved by his plan, while it is universally admitted that almost none are saved by other methods, considers that this theory will yet be one of the bright stars in Dr. Sims' reputation, and closes by quoting from the latter: "If I am wrong, contemporaneous observers will prove it. If I am right, future generations will feel it."

J. F., JR.

3. Monti: Vienna Poliklinik (*N. Y. Med. Jour.*, No. 261): This short report of three cases from Prof. A. Monti's poliklinik is so characteristic of the man, his clearness, positiveness, and precise method of teaching, that I give the article entire as reported in the *Journal*:

Congenital Cyanosis—Bronchial Catarrh—Apthous Stomatitis.

CASE I.—You have here, gentlemen, a child which, from the mother's

account, has been cyanotic from its birth. He is four years old, yet he is scarcely as large as a child of two. On careful examination, you see the marked blueness of the face, particularly of the cheeks. Even the tongue and mucous membrane of the nose and mouth are blue; hence this is a general cyanosis, not confined to the skin. The hands are congested, and there is a peculiar swelling of the tips of the fingers and toes.

When the child cries there is a general venous congestion. This is a very striking and interesting case, but the interest does not lie in the cyanosis alone. The child is unable to walk or talk; his head is abnormally small—in fact, microcephalic; the fontanelles are not yet closed. The penis is only as large as that of a new-born infant. The testicles are present, but are remarkably small. In short, the child is an idiot. This is undoubtedly a disease of the circulatory system, and probably some defect of the heart. It must be *congenital*, for not otherwise could such extreme cyanosis result. By careful percussion, I find that the heart is not enlarged. This is very important, for in acquired heart disease the heart would be enlarged, while in this case, on the contrary, it actually seems to be rather smaller than normal. The apex-beat is not definitely located. On auscultation, you will hear a loud, blowing, systolic murmur at the base. Hence we must infer, first, that there is no enlargement; secondly, that the apex-beat is diffused; thirdly, that the ventricular tone is normal, and, lastly, that there is a systolic murmur at the base. Both pulmonic sounds are accentuated, and there is a blowing murmur with the first aortic sound. Now, I would ask. What have we here? Is this a case of open foramen ovale, of congenital pulmonic, or aortic defect? Is the septum wanting, or does the ductus Botalli remain patent? We cannot definitely say; all that we can tell is that there is a murmur at the base, which may be due to an open foramen ovale, or to a defective septum. The former is more probable. For us, as practising physicians, it is enough to say that the child has some congenital defect about the heart. The anomalous appearance of the head and sexual organs is interesting, but this has no connection necessarily with the other conditions. The prognosis in these cases of cyanosis is most unfavorable. Such children usually suffer from bronchial catarrh, and are liable to die suddenly from acute congestion within the first four or five years of life. As to the prognosis in the present case, I need say nothing; it is very bad. We can do but little in the way of treatment except to give tonics, regulate the diet, and administer stimulants as required.

CASE II.—This child has a cough and, as the mother says, seems to suffer from pain in the abdomen. It is not unusual in pneumonia, either of the upper or lower lobe, for the pain to be referred to the belly. In pleurisy in children the hypochondrium is generally the seat of pain. By further questioning, I find that two days ago the child suffered from pain in the breast, and dyspnea. On examination, you see that the face is pale, the general surface temperature somewhat elevated. The respirations are not much accelerated, and there is a slight cough. Auscultation of the right back shows roughened respiration, with moist râles; the same is heard on the left side. You see that I listen to the chest first because the child begins to cry if I percuss. I also employ immediate auscultation, as children are less terrified than if they see a stethoscope. Many different instruments have been devised. We use here a stethoscope invented by Rousseau, which has a small rubber bulb attached to the mouth

of the instrument, so that the air can be exhausted. Two long flexible rubber tubes connect it with the ears. The advantage of the instrument is that it adheres to the skin and is not detached by the child's uneasy movements. As I said before, we find here roughened respiration, and râles, but without dulness on percussion. Hence this is a catarrh of the bronchi. It is important to decide whether the large or small bronchi are affected; if the small, the respirations will be very frequent. This is not the case here. The respirations will be labored in capillary bronchitis, and there will be movement of the *alæ nasi* with action of the muscles of the neck. This symptom is also absent in the present case. The treatment is simple. The child should be kept in bed and have its chest enveloped in wet compresses. There is no specific treatment. You can give some simple expectorant, and, if there is elevation of temperature, a teaspoonful of salicylate of sodium (a five-per-cent solution) may be given every three hours as an antipyretic.

CASE III.—This is a case of aphthous stomatitis. It is a disease most often seen in summer, and seems to be due to some fermentative process. It is often preceded by a considerable elevation of temperature (39° – 40° C. [= 102.2° – 104° F.]), leading to the inference that pneumonia is threatened. Then comes salivation, attended with heat, redness, and swelling of the buccal mucous membrane. The child loses its appetite simply because the presence of food in the mouth causes pain. After twenty-four hours the fever declines, and, simultaneously, small vesicles appear on the mucous membrane of the lips, tongue, and gums. On the first day five or six vesicles are seen, on the second as many more, and so on. By confluence of these, large pustules are eventually formed. In this case you can see all of these different stages. Salivation is present. Here are vesicles on the lip and tongue, and on the roof of the mouth is a large purulent spot. This is not diphtheria, but confluent aphthous stomatitis. The child infects itself through its saliva, and, in a family where several children use the same glass or spoon, the disease spreads. Hence aphthous stomatitis is infectious. The prognosis is favorable, though, if the affection lasts for twelve or fourteen days, the child may become emaciated through loss of sleep and appetite. In some cases the larynx may be affected, leading to edema of the glottis and a consequent diagnosis of diphtheria.

The treatment is simple. Attend carefully to the diet, giving nothing but cold milk until the soreness of the mouth decreases. In no disease is the application of cold more useful. Give the child small pieces of ice, or syringe out the mouth with ice-water. This treatment, according to my experience, shortens the attack. Mouth-washes are often applied with a rag or brush, but wrongly so, for every mechanical injury is harmful, and, by roughly rubbing the mucous membrane suppuration is induced and the disease prolonged. A solution of chlorate of potassium (1 to 100) may be given in doses of a teaspoonful every two or three hours. This has only a local effect, the idea that it acts through the blood being erroneous. The mouth may be syringed out with a similar solution every four hours. Benzoate, or salicylate of sodium (a five-per-cent solution) is also useful. A solution of corrosive sublimate (1 to 500), applied with a camel's-hair brush to the gums, causes a rapid dulling of the sensibility in cases where the pain is great. The separate spots may

be touched with the solid stick of nitrate of silver. It is too painful when the whole mouth is thus pencilled.

4. Gadet de Gassicourt: Diphtheria of a prolonged Form (*Revue Mensuelle des Maladies de L'Enfance*).—What is to be understood by a prolonged form of diphtheria? It is not exactly chronic diphtheria. The disease begins in the usual way, appears after some days to be about to cease, but instead of this there seems to be a tendency to a continuous new development of pseudomembranous deposit, which may last for months or even years. This form of the disease is dangerous only when it reaches localities where the extension of the membrane does positive harm. On this ground the author divides his cases into those in which the membrane is not in the larynx but in other parts of the body, and those in which the membrane forms in the larynx itself (I. Diphthéries à forme prolongée sans croup et II. croups à forme prolongée).

I. In a case of the first kind the development of the pseudomembrane continued on the nasal mucous membrane for forty-five days. Then there was a period of twenty-seven days of diphtheria in the usual form, which passed over, leaving the girl, a child of two and a half years, apparently well, except that the formation of membrane in the nose continued eighteen days longer. In the case of an assistant of the "Hôpital des enfants malades," the diphtheritic process was also localized in the nasal cavities, and continued nine months. The young man travelled all over Europe in search of cure, but did not recover till he again returned home.

II. Localization in the larynx. The author divides the cases of this class into three groups: (a) Prolonged course but cure without tracheotomy; (b) long duration of the process without indication for tracheotomy; and (c) continuation of the process and further formation of membrane after the operation.

He makes no explanation of the continuance of the disease in these cases, and suggests nothing new in the way of treatment. J. F., JR.

5. Woakes: Etiology of Diphtheria—its Contagium and the Occurrence of sudden Death (*Lancet*, xi. & xii., 1883).—Carrying out an opinion expressed in the International Congress in London, Edward Woakes claims that diphtheria does not consist of a specific disease of the mucous membrane with secondary constitutional infection, but of a disease of the sympathetic nervous system. Starting from the physiological fact that every irritation of the afferent fibres of the sympathetic ganglionic system projects itself reflexly in the path of the vaso-motor fibres, and manifests itself in the locality supplied by them by a contraction or dilatation of the vessels, which in its turn has a determining influence upon the function and nourishment of this locality, Woakes sees in a disturbance of this regulating function of the sympatheticus the sole cause of diphtheria. Woakes has reached this peculiar understanding of the diphtheritic process through observing that he never could bring the outbreak of the disease into connection with any zymotic virus, but that, on the other hand, changes of climate and conditions of weather were of great influence. Sometimes it was intense cold; sometimes continued heat; then again a strong East wind or damp, cold weather, which preceded the outbreak of the disease. A second circumstance which seemed to strengthen his view, was the outspoken disposition of certain families to diphtheria without the existence of anything peculiar in their sanitary

circumstances. It is these two factors, family disposition and precedent abnormal conditions of the weather, [which render the organism liable to diphtheria, the first depending] probably upon an hereditary excitability of the vaso-motor centres, the latter causing a weakness of the vital force, especially that of the nervous system. In consequence of this weakening of the vaso-motor nerves and their centres, the individual is not in condition to withstand further causes of disease.

Sporadic cases are easily explained in this way. As soon as an individual who unites in himself these two predisposing factors is exposed to a new irritation, for example, cold, which, as is so often the case, strikes the sympathetic nerve-fibres of the upper cervical ganglion, the corresponding vaso-motor fibres respond to this irritation with dilatation of the vessels, and upon the corresponding mucous membrane there result hyperemia and exudation, in one word, catarrh. The intensity of this catarrh depends alone upon the condition of the sympathetic system weakened by the two factors mentioned. If this loss of nerve strength be slight, the vessels soon reattain their tone, and the catarrh passes away; but if the individual has already lost much of his nerve force, the simple catarrh assumes a phlegmonous form, with a predominance of edema, as the expression of greater vascular paralysis. If, however, the patient's nervous force is so far lost that the ganglion centres are unable to oppose any regulating contractive action to the injurious irritation, there then follows the highest degree, complete vascular paralysis, and inflammation with exudation. There is, therefore, only a quantitative and not a qualitative difference between simple catarrh and inflammation with a false deposit. The most import process in this highest degree of vascular paralysis is the surprisingly rapid formation of new cell elements. The transudation corpuscles thrown out in the subepithelial layers cause a rapid proliferation of the connective tissue-cells, especially those of the cell layers in the walls of the pharynx. These new cells, formed in masses, wander in direction of the least opposition, and soon reach the basis membrane, where they organize themselves into genuine diphtheritic membrane. In regard to the question of contagiousness, which Woakes considers undoubted, he holds the view that the contagion is not something undetermined and foreign to our organism, but a perfectly determinable tissue element, existing in normal conditions in our bodies, but altered in its condition through the new circumstances in which it originates. This complete change in the biological condition of a normal tissue element explains the contagiousness of diphtheria. The new-formed cells run in a few hours a course which normally would occupy years. Scarcely have they originated when they are forced by new cell formation to the surface of the membrane, and cast off before they have gained the power of proliferation or organization. If such cells reach a new soil which is related in its histological condition to the mother soil from which they originated, they are, according to the hypothesis of Woakes, able not only to develop themselves further, but also to arouse in their neighborhood the same stormy development of cell elements with rapidly following destruction, insofar as the individual offers the predisposing condition of vascular paralysis. If this latter be not present, a simple catarrh will be the only consequence of diphtheritic infection. This explains why from the same source of infection in one case there originates the severest diphtheria, in another a simple angina.

This hypothesis explains the occurrence of sudden death in diphtheria. The weakening of the vaso-motor force is felt not only in the vessels of the mucous membrane, but also in those which nourish certain nerves. In consequence of this paretic dilatation of the nerve vessels, the motor fibres become affected. This the author has seen in the fibres of the vagus. The more intense the diphtheritic disease is as regards the loss of power in the ganglion system, the greater will be the extension of paralysis, especially in the vagus, until the branches leading to heart or lungs are affected, and death follows through cardiac or pulmonary paralysis. The paralysis, however, is not central, as is generally considered, but peripheral, caused mechanically by the pressure of the nerve vessels, in consequence of the greatly dilated vasa nervorum. Woakes holds the same view of diphtheritic paralysis of the palate. It is never of a central nature, but always the result of a high degree of dilatation of the nerve vessels and consecutive exudation in the nerve sheaths which causes compression of the motor fibres.

J. F., JR.

6. Percy Kidd: Pathology of Diphtheritic Paralysis (*Lancet*, II., 1883).—In opposition to the purely hypothetical view of Woakes in regard to diphtheritic paralysis, Dr. Percy Kidd regards the seat of the affection as central and spinal, and supports his view by pathological anatomical examinations which agree entirely in their results with analogous cases reported by Vulpian, Abercrombie, and others. The lesions occurred in the anterior horns of the spinal cord and consisted of a change of form in the motor ganglion cells, and a change of structure in their cell protoplasm. The affected cells were mostly round and without the ordinary processes. The cell protoplasm appeared indistinct, the nucleus was wanting or scarcely visible, or more rarely the contents of the cells were granular and showed a distinct nucleus. Both changes led to atrophy of the cells, so that in certain portions of the microscopical sections the number of motor cells was greatly diminished. The lesion appeared limited to certain circumscribed portions which corresponded to the locality of muscular paralysis during life. The neuroglia did not appear to be involved.

ARTHUR BENSON (*Brit. Med. Journ.*, 1,159) reports a case of diphtheritic paralysis of the muscles of the eye in the fifth week of diphtheria, the paralysis continuing seven weeks. In regard to the seat of the lesion, he considered it to be central, in the brain or spinal cord, because if, according to the sympathetic theory of Dr. Hughlings Jackson, the ganglion lenticularis had been diseased, there would also have been an alteration of the pupil. The localization of isolated bilateral paralysis of accommodation must be in the accommodation centre established by Hensen and Völker in the posterior portion of the floor of the third ventricle.

J. F., JR.

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ORIGINAL COMMUNICATIONS.

A NEW METHOD OF PARTIAL EXTIRPATION OF THE CANCEROUS UTERUS.

BY
ELY VAN DE WARKER,
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With six woodcuts.

THE history of the surgery of women teaches us that success has followed failure only as the result of wise and careful experiment. Our lesson from this is that, however discouraging the surgical treatment of malignant disease of the uterus may be, fortune may have in store for it a future of better promise. We are, therefore, encouraged to enter the field with new methods, and to go on accumulating experience.

I have had, in common with every physician, the usual experience in the treatment of these cases. It is needless to review this, but from the present status of the surgery of this disease we may learn something. At this period the treatment is narrowed down to three methods: total extirpation of the uterus through the vagina, vaginal amputation with supra-vaginal excision, and my own method of potent chemical cautery. The first is a method attended with such a ratio of direct fatality and with such mechanical difficulties in the way of its performance that ovariectomy is safe and simple in comparison.

The second is simply a survival of the antique. To the surgeon who hopes to gain a more or less permanent arrest of the ingrowth of malignant uterine disease by amputating the cervix and an excision of the tissue of the organ to the os internum, the past has taught but little. There is a period in the course of malignant invasion when the disease is a simple focus. If this stage could be detected with certainty, and at the critical movement this focus eradicated, the knife, or the *écraseur*, or the galvano-cautery would show a better record. Let us conceive further that this beginning focus has extended through a wider zone, but within limits well defined by the palpable vaginal portion; here again these older procedures would give possibly immunity for years from the advance of the disease, if not a radical cure. But my experience has been that when the disease has passed above the vaginal junction, amputation and uterine excision, no matter how thorough, was useless to stay the disease, but when confined to the vaginal portion such a happy result was possible because the operation had extended beyond the frontier of malignant action into absolutely healthy tissue, and a disease yet purely local, but potentially malignant, was completely extirpated.

We all know how rare it is to see these cases while the disease is thus limited. I may say that I have never seen a case in this primary and fortunate stage for simple removal; that other surgeons have, I think, is proven by the rare instances of apparent cure.

In this direction I claim that surgery by the knife has reached its limits. We ought to look for no better results in total uterine extirpation than in total mammary extirpation, and the figures so far as they have gone prove this. The difficulties of this operation, and its fatalities which equal a third of the recoveries, are encountered for what, in every case of uterine carcinoma, is simply an experiment with the laws of chances combined against it, for by no means of examination yet known can one assure himself that nodes of cancerous infection do not exist in the parametrium which would defeat every effort for removal. Amputation of the cervix by the knife and scissors, the galvano-cautery, the gas cautery, the local application of caustics to the cervix, be they bromine, fuming nitric acid, or any chemical of equal potency, the application of styptics, or

alteratives are only mentioned as measures that are worse than useless and ought in our present knowledge of the subject to be abandoned. In view of this I believe I am justified in saying that the knife has reached its ultimate limits, and if we are to advance further in treatment equally radical it must be in other directions.

Dr. Sims, in an article in the *AMERICAN JOURNAL OF OBSTETRICS*,¹ speaking of a noted empiric, says in his usual candid way, "He taught the profession this truth, which we would not accept from such a source, that better and more permanent results followed the use of caustics, and a consequent sloughing, than followed the use of the knife with healing by the first intention." I began the use of caustics nearly ten years ago, but my experience was such that I abandoned the plan, and for several years did nothing for these cases. My plan was to amputate the cervix and excise into the body of the uterus as far as I could go with the knife and scissors, and then complete the operation by burning out the excavation with the actual cautery, or, when that instrument came into more general use, with the bulb of a Pacquelin gas cautery. I worked through a Sims' speculum and protected the anterior vaginal wall with thin pieces of wood. I cauterized freely, stopping several times to let the cavity cool, as it would get so hot I could not hold my finger in it. But, as I said, I abandoned the method after an experience of several cases. I learned one thing, however, that has been of use to me since. No matter how thorough I might be in the use of the actual cautery, the slough that resulted was extremely thin, the cautery had no power of penetration, as the eschar, too quickly formed, protected the deeper tissues. I was convinced that while cautery was the method that ought to be employed, the actual cautery was not the agent, but I did not see my way to any other form.

Here the matter rested until I read Dr. Sims' article. I was then encouraged to go on after his method, and operated in several cases. It was better than anything I had yet tried, but on the whole was a failure. Several months of loathsome discharge and loss of blood were saved to the patient, but no real arrest was given to the disease. The slough that resulted

¹ Vol. xii., p. 451.

from the use of the chloride of zinc solution of the strength prescribed by Dr. Sims (3 v. ad 3 i.), was very thin, rarely exceeding one-sixteenth of an inch in thickness, showing that we were yet working upon the surface, and were without the power of penetrating into the depths, much less through the zone of malignant action. The idea occurred to me to apply to the uterus caustics of a power to give the same depth of slough that we would operate with on a cancerous breast. Instead of producing a slough of a quarter of an inch, why not slough out nearly the whole uterus if necessary? And this I believe we may do as safely as we would the whole mammary gland.

To the late Dr. Sims belongs the credit of teaching us how to apply a caustic solution on pledgets of absorbent cotton, literally wrung dry of superfluous fluid, a method that is equally applicable to any watery caustic solution. Thus we have the means of applying these potent agents with the certainty and accuracy of the knife. I am not aware that any one can dispute this simple and efficient device with Dr. Sims.

I make my operation as follows:

The first step consists in amputation of the cervix uteri up to the vaginal junction. The cervix is seized in some portion of its periphery where the tissue is sufficiently firm to hold, with a double vulsellum, and the part cut away with a scissors curved on the flat. If the part is very indurated and thick, a knife with a suitable handle is the better instrument. From this point the tissue of the cervix is excised, usually as high as the os internum, in the form of an irregular triangle, with the base at the vaginal junction. We may do this with either the knife, scissors, or curette, whichever is most convenient. In case the tissues in the cavity of the cervix are very friable, a curette will answer every purpose. With this instrument we may follow the spongy tissue down into depressions of the firmer parts, and then using the scissors, smoothe off the irregularities to a general level. In case of considerable depth of indurated tissue in the cervical cavity, a knife that may be placed at different angles is necessary. Hemorrhage has never given me any serious trouble at this stage. In several cases there has been free loss of blood, which ceased as soon as the cavity was thoroughly cleaned out.

I believe that in the majority of cases packing the excavation, after the manner of Sims, with iron cotton as a precaution against hemorrhage, is not necessary, and that we might proceed at once to the second step of the operation, as the zinc solution is in itself a powerful astringent. In two instances I did so, but it appeared that the application of the caustic was more painful and the resulting slough was not so thick as when preceded by a packing of the subsulphate of iron solution. A further experience is necessary to confirm this; but it seems a reasonable explanation that the sensibility of the surface would be lessened by both the pressure of the cotton packing and the effect of the powerful astringent upon the severed nerve fibres and capillary-vessels, while the chemically hardened surface would permit a deeper penetration of the caustic solution, as in histological work a properly hardened preparation will select and take a stain better than a fresh.

With these two exceptions I have always packed with the iron cotton, one of subsulphate solution to three of water. I cannot help regarding it as one of the steps of the operation requiring the greatest care and as not without danger. The directions given by Dr. Sims for the prompt removal of the cotton on the first suspicion of blood-poisoning must be carefully followed. He also reports two cases in which the thinned and weakened uterine wall was ruptured by too great force used in introducing the cotton. When I have seen how thin we may wear down parts of the uterine wall by using the curette, another explanation of this occurs to me; that the thin and friable uterine shell may, under the powerful influence of the astringent, give way at its weakened point from the shrinkage of the walls over the unyielding mass of cotton. I pack the uterus with masses of absorbent cotton about the size of a chestnut, wrung nearly dry from the iron solution, made of one part of subsulphate of iron to three of water. I find that if the cotton is used in small, separate masses, the cavity may be packed without the use of any force, as larger or smaller pieces may be chinked in where they are needed. In removing the iron dressing, these small pieces may be taken away separately with much less force than large. Very little packing is required in the vagina, as the iron cotton keeps its place very firmly, while filling the vagina adds very much to the discom-

fort of the patient and prevents the free action of the bladder. The use of carbolized or aseptic cotton in the vagina in no way prevents decomposition, as it is placed where it can do no good. We can, in a measure, avoid blood-poisoning, which is so liable to follow the decomposition of blood-clots saturated with iron, by carefully removing all clots from the uterine excavation, and if we find the blood oozing from between the masses of cotton, remove all the dressing and repack, rather than insure against hemorrhage by imprisoning the exuded blood by packing the vagina, as the blood is sure to decompose and prove the source of a vile odor, if not of absolute danger.

The dressing ought to be removed by the second day. This I can do more readily with a tenaculum with a short, right-angled hook than with Dr. Sims' cork-screw instrument. As mentioned above, here the chief advantage arises of using small masses of the iron cotton, as each one may be caught up on the tenaculum, and removed separately without any force. The patient should be placed upon the table in a good light, as the vagina will be found discolored by the iron, and very much contracted. When the vagina and uterine excavation is properly cleaned out, the patient is ready for the real operation, compared to which all that has gone before is subsidiary and of minor importance. The reader is entitled to object, if it is of so little consequence to the real purpose of this method, why do it? It is done for various reasons. First. That we may make an excavation in which to pack our caustic; secondly, to remove as much of the diseased material as possible by a rapid operation; and thus, thirdly, to save time. The operation I have just described is more radical than that usually made for epithelioma of the neck. It is not rare to find that the greater part of the supra-vaginal cervix has been shelled out, leaving a thin-walled cavity reaching to nearly the os internum.

I make use of two strengths of the zinc chloride solution, one of 3 v. to the ounce of water, and one of equal parts of the chloride and of water, by weight. Before proceeding to pack with the caustic cotton, I have everything prepared. The zinc solutions, about an ounce of each, in salt mouth bottles, marked so that there may be no doubt which is the stronger. A pomade of bicarbonate of soda in vaseline, about

one to three, and a thirty-per-cent solution of the same salt in a goblet. An assistant accustomed to hold a Sims speculum is very necessary. After the dressing of iron cotton is removed, I clean and dry the vagina and excavation with absorbent cotton, and then carefully protect the labia and vagina with the pomade of bicarbonate. This is important, as the comfort of the patient for two weeks or so depends upon protecting the labia, and, especially, the urinary meatus, from the action of the caustic. I have found these parts the seat of slight sloughs after I had exercised the greatest care to protect them. Since using the vaseline pomade, I have had no further trouble. The question now comes up, How are we to determine whether we will use the weak or one-hundred per-cent solution? I determine this point at the completion of the preliminary operation. By introducing a blunt sound to the fundus of the bladder, and with the finger passed into the uterine excavation, I endeavor to feel the sound through the intervening part, and thus estimate the amount of tissue left for the caustic to act upon. (Case III.) The posterior relations of the cavity I judge of in like manner, by passing the sound into the uterine excavation, and with a finger in the rectum, if the interlying part is very thin, the sound may be felt. It is better to do this before the excavation is hardened and contracted by the iron. If, then, we have approached quite near the surface of the uterus, so that a slough in excess of a quarter of an inch in thickness is liable to result in perforation, it is prudent to use the weak solution after the upper and thicker-walled parts of the excavation have been packed with the strong solution. If the vaginal wall was involved and worked down with the curette, it is possibly better to use the weaker mixture for fear of perforating into the bladder or rectum, although I have applied the one-hundred-per-cent solution upon the vagina without accident. (Case II.) Usually, we need not hesitate to expose the surface operated upon to the full strength of the caustic. After the packing is completed, the surface of the zinc cotton and about an inch of the upper vagina is filled with absorbent cotton saturated with the bicarbonate of soda solution, by which any of the chloride of zinc that may filter out is decomposed.

In some instances, the pain is not severe. One or two full

doses of morphia, hypodermically, are sufficient to bridge over the period of pain, which usually does not exceed ten hours. In about two or three days, we may remove the cotton from the vagina, and, if we can do so without force, from the uterine excavation. If the zinc dressing is thoroughly cemented down, it is better to wait a day or two longer. When it is removed, a white, firm, cement-like surface, which is the slough firmly adherent to the excavation, is brought into view. The slough will separate in from five to ten days. No force at any time should be used to detach the slough, but it must be allowed to exfoliate spontaneously. If the one-hundred-percent chloride solution has been used, it is thrown off in a single piece, an exact cast of the cavity. There is no danger of blood-poisoning during this stage, as the chloride is a perfect disinfectant. While the slough is separating, we may aid the process by a free douche of carbolic acid solution, and which may be continued during the granulation process.

It is during the sloughing stage that we may be annoyed by hemorrhage. I guard against it by confining the bowels for four or five days, as then there will be no cause for expulsive effort upon the bed-pan. The patient is cautioned against sitting up in bed or making any considerable effort to help herself. It is better to empty the bladder through the catheter at this time, as patients get to be very careless if allowed to help themselves. On several occasions, I have seen hemorrhage follow directly after using the douche. This, I think, was owing to the force of the current into the excavation. None but the gentlest stream must be used, and, if hemorrhage should follow, stop the injection for a day or two. My experience is not sufficient to state it for a fact, but it is a fact, so far as it goes, that in cases in which the destruction by the caustic of the muscular tissue of the uterus has been extensive, there has been less hemorrhage than in cases in which a considerable part of the substance of the organ was intact. In one case, in which I had reason to believe, from the shape and quantity of the slough and the depth and breadth of the excavation, that the entire uterus had been shelled out of its peritoneal envelope, there was only a slight hemorrhage on the sixth day caused by the patient getting up and sitting upon the commode. The hemorrhage ceased spontaneously. I have

never been obliged to pack the vagina to arrest hemorrhage. An injection of a weak subsulphate of iron solution has always promptly arrested it. It is not a good form of astringent to use, however, as the bed is liable to be damaged. Vinegar or a solution of alum would be just as efficient, and without this disadvantage. The use of vinegar would not constrict the vagina, and thus allow one to pack in case this became necessary—a very difficult thing to do after an iron injection.

Cicatrization is completed in from two to four weeks, leaving a greatly contracted cavity lined by a pale, soft, velvety membrane free from odor or discharge. The cavity continues to contract for some time after the granulation process is completed (Case II., Fig. 6). Contractions such as this would follow no agent other than a caustic. It is an endowment of cicatricial tissue from this cause to undergo this progressive shrinkage as the deformities from severe burns prove.

The following cases will illustrate practically the various steps of the operation.

CASE I.—Mrs. B., widow, of Oneida, N. Y., forty-nine years old, the mother of three children, the youngest twenty-five. First menstruated at thirteen years and passed change of life at forty-five. Always enjoyed good health, but was never robust. Early in November, 1882, she was brought to me by her physician, Dr. Cragin. For six months previously she had noticed an offensive discharge with considerable itching and irritation of the labia, with a rapid decline in strength and flesh. For last two months she had been losing blood from the vagina, at times quite freely. Some sacralgia, but otherwise quite free from pain. An examination showed that a friable mass the size of a hen's egg was attached to nearly the entire periphery of the vaginal cervix, anterior to which a trace of the cervix could be felt intact. The growth extended downward for nearly an inch upon the posterior vaginal wall. The serious nature of the disease was stated to her friends to whom I explained what I proposed to do to arrest it if possible. The patient consenting, she was admitted to my private hospital and on Nov. 11th, assisted by Drs. Cragin, Stanton, and Miss F. A. Adams, medical student, the mass was removed up to the solid line *a*, Fig. 1, by the scissors and curette. The excavation was packed with iron cotton, which was removed in two days and the caustic cotton, one-hundred-per-cent solution, was applied. There was very little pain, a single hypodermic injection of ten minims of Magendie's solution being all that was required. On the seventh day from the application of the caustic the slough shown in Fig. 2 was found lying in the vagina. It was firm, white, and perfectly free from odor.

From an examination of Fig. 1, it appears as though the point *c* on the posterior wall of the vagina was especially liable to perforation, as the curette had already made the part exceedingly thin, and in packing it was exposed to the full strength of the caustic; but it did not occur, and in fact at this point the slough was about one-fourth the thickness of that in the excavation of the uterus. The explanation of this is, to me, very simple. The degenerated tissue of the epithelioma is of a low grade of vitality, and can offer no resistance to a chemical caustic; on the contrary, normal surfaces stand at



FIG. 1.

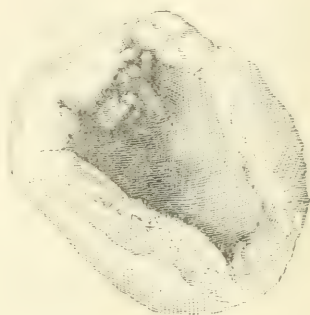


FIG. 2.

the opposite pole of resistance, and further, the inflammation excited in it by the cautery opposes a wall of hyperplastic tissue to its further invasion. Thus the caustic action was limited at this point, and throughout the uterus. In my former work I have observed this: under the action of the actual cautery we lose this advantage, normal and diseased tissue being destroyed to the same extent, the eschar giving equal protection to each. The dotted line *b*, Fig. 1, represents as near as I could measure it at the time, the limit of the destruction of the uterine tissue. The size of the slough shown in Fig. 2, which is from a ferrotype, actual size by measurement, will enable the reader to estimate how nearly correct the line *b*, Fig. 1, is. The photograph was taken after it had been some time in strong spirits by mistake, which had caused considerable shrinkage. All the lower portion is cut away, the specimen figured representing the part that corresponded to the fundus uteri. If we

compare the size of the slough to that of the normal uterus, and allow for the amount of tissue removed at the preliminary operation, the conclusion seems inevitable that in this case the entire uterus was sloughed out of its peritoneal covering. This had been done without damage to near parts, high temperature, or severe pain.

The specimen shown in the figure has been further studied. Careful microscopic sections have been made by Dr. A. C. Mercer. Fig. 3 is a drawing from one of these, stained with logwood and mounted in Canada balsam. The margin *a* corresponds with the outer, convex surface of the slough, and *b* with the inner surface or cavity. It therefore represents a transverse section of the uterus a little above the level of the os internum. From it we may learn something of the process by which the caustic works. It destroys vitality, but does not disintegrate. Each histological element is preserved intact in its exact relations, hardened and fixed as though it had been prepared for microscopic section in a solution of chromic acid and spirits. The chloride has the power of penetrating the debased tissue of the epithelioma just as one histological element has the power of selecting one stain and rejecting another, it then destroys the vitality of the part by coagulating and solidifying its fluids by its marked affinity for water. The chloride would affect normal tissue to the same extent and manner if by simple contact it could penetrate with equal facility. When brought in contact with normal tissue it excites inflammation of sufficient high grade to form a line of demarcation, and at which point separation occurs between the healthy and necrosed portions. I have compared this selective power of the chloride to double staining in histology which served as a figure to illustrate the process, but of course it could not be used as an actual explanation of what goes on in vital tissues, one normal and the other undergoing retrogression. The vital action must be nearly this: The chloride excites inflammation through the cancerous tissue, but of a low, non-resisting grade, like the low, sluggish inflammation that keeps just in advance of the march of senile gangrene, and the local death is not arrested until a line of high grade inflammation is reached capable of interposing a margin of hyperplastic tissue. In the case before us, the figure shows a line

of demarcation as sharply defined as though made by a knife. If we divide a carcinoma of the uterus into the three zones of Hart and Barbour,¹ we have, first, a layer in the process of breaking down; second, a zone of purely malignant tissue intact; and third, one of normal tissue more or less invaded by the debased cellular elements of the disease (advancing carcinoma). An examination of the section shows us what has taken place.



FIG. 3.

Beginning at *b*, Fig. 3, we are at the point where the preliminary operation ended, and the caustic action began. That is, I had passed through the first zone and into the second, and the chloride solution had penetrated the remnant of the second, through the third zone into, slightly, the underlying normal tissue. We see in the cut the second zone with its profusion of cell growth and its scanty connective and muscular elements terminating somewhat abruptly at *c*, where we have a beginning

¹ Manual of Gynecology, p. 449.

normal connective and muscular tissue sparsely infiltrated with cells, terminating at *a* in normal structure.

Mrs. B. is at this date, January 18th, 1884, free from disease.

CASE II.—Mrs. P., forty-five years, one child twenty-six years. She ceased to menstruate at forty-four years. An excitable and nervous brunette. For about a year previous to seeing her, she had noticed an offensive brownish discharge, and latterly an occasional small hemorrhage. An examination showed the uterus in a state of fixation, the cervix gone quite up to the vaginal junction, and leading up into the cavity of the cervix a ragged excavation, nearly an inch in diameter, and over an inch in depth, which bled freely.

On Feb. 19th, 1883, assisted by Dr. Stanton and F. A. Adams, I removed, by the scissors, the remnants of the vaginal portion, and cleaned out the cavity with a sharp curette. The walls were very friable, and in a short time I had an extensive excavation before me. While using the curette well up on the anterior wall of the cervix, I observed I was getting dangerously near the bladder. I laid aside the instrument, and introducing a sound into the bladder, I used my finger-nail to break down the tissue. It was evident I was working my way through a deep cavity upon the sound. My fears were confirmed by a slight gush of bloody water into the vagina. I stopped work in this direction, in some alarm, but completed the operation upon the remainder of the cavity in a very thorough manner. The uterus was packed with the iron cotton, which I removed on the second day. In the interval, I determined upon the course I should follow. As it seemed evident, whatever I might do, I could not place the woman in a more desperate condition, I determined to proceed with the use of the caustic, in the hope that by the resulting contraction, which I had a reason to expect in the repair process, the opening into the bladder would be nearly, if not quite, closed. I used the one-hundred-per-cent solution of the chloride. The patient, who was very intolerant of pain and accustomed to the free use of morphia, made no more complaint than usual. On the 27th, she had a spurt of hemorrhage, while upon the commode, as she insisted upon getting up, but it ceased in a few minutes spontaneously. As the urine escaped from the vagina, a catheter was retained in the bladder, through which all the urine came. On March 4th, an enormous slough came away without any force being used. Granulation proceeded very rapidly, and at the time she left the city, near the end of April, about all the urine came through the urethra when sitting up, and when lying, she could retain the urine over an hour. I saw her for the last time at her home at Jamesville, under the care of Dr. Knapp. The vagina ended in a small cul-de-sac, and was pale and soft.

As this case demonstrates what we may do with the potent

caustic in a case so desperate that it was clear no other means could have been employed to give even a temporary relief, it appears worthy of illustration.

Fig. 4 shows the general condition of the parts at the completion of both operations. The solid line *a* defines the limits of the work with the curette, with the opening *b* into the bladder, which is shown collapsed at *d*; *e* is the opening of the urethra, *v* is the vagina also collapsed, *r* is the anterior rectal wall.¹ The dotted line *c* represents the extent of the excava-tion after the separation of the slough. It may appear to the reader an exaggeration, but if he will compare the engraving of the slough, after a ferrotype, actual size, it will be seen that



FIG. 4.

I have in no way magnified the dimensions of the cavity from which it was cast off.

The slough, Fig. 5, has retained the shape of the uterine cavity. It would have been a very beautiful specimen if the lower portion had not been injured by the action of the urine. It was laid open at its upper part, and shows its thickness at A.

Figure 6 exhibits the size and shape of the upper vagina, and what was left of the uterine cavity *a*. Located somewhere at *b*, was a minute opening into the bladder. The cut gives

¹The profile is copied from Hart, "Structural Anat. Female Pelvic Floor," Pl. ii., Edinb., 1880.

no more than a correct idea of the enormous contraction that attended the repair process. The surface was smooth, soft, and pale. For several months previous to the operation, it was difficult to feed the patient. She suffered extreme pain throughout the abdomen and down the limbs, for the relief of which she was taking daily a large amount of morphia. The pain continued to the last, especially through the abdomen. Her



FIG. 5.

nutrition in no way improved, and latterly was seriously impaired by obstinate vomiting. She died exhausted about the middle of September, 1883. To the last, there was no return of the disease in the vagina, but I am satisfied that it had in-

vaded some of the abdominal organs. I believe that the operation materially prolonged her life, kept her clean and free from odor, and saved her and her friends all the disgust and misery that attend the last stages of uterine cancer.

CASE III.—Mrs. W., aged sixty-one years. Has had seven children, the oldest thirty-eight, and the youngest twenty years. Early in fall of 1882, had bloody discharge and pelvic pain and fulness. She was brought to me by her physician, Dr. A. K. Hale, of Adams, Jefferson County, N. Y., and admitted into my private hospital. An examination showed the vaginal portion somewhat enlarged, but not indurated, the margins of the os externum showing friable tissue, for a depth of about three-eighths of an inch, upon the external surface. The diagnosis of malignancy was made, and on August 26th, 1883, aided by my usual assistants, and in the presence of



FIG. 6.

Dr. Hale, the vaginal portion was scissored away and the curette freely applied to the cavity of the cervix and body. What appeared before operation a very simple, was now seen to be a very serious case. A large share of the cervix broke down under the curette to a point above the os internum. The anterior wall of the cavity of the cervix was entirely scraped away, the curette passing into the connective tissue between the uterus and bladder. The cavity was packed with iron cotton, which was removed the following day, and the caustic applied, the upper portion being packed with the one-hundred-per-cent zinc cotton, while the perforated portion of the cervix was filled with packing of the weaker solution. While the slough was separating, several hemorrhages occurred, which always followed the douche. In all other respects, Mrs. W. did very nicely and made a rapid recovery. She is reported by Dr. Hale as still well.

These cases illustrate all of the descriptive portions of this paper. They serve to demonstrate what may be done with potent caustics applied on cotton. I believe that further ex-

periment with this agent will develop a new field in pelvic surgery. I feel assured in saying that no other method yet suggested or tried is equally thorough and radical, except total extirpation of the uterus.

A DIFFERENT METHOD OF TREATING A CASE OF FRESHLY RUPTURED PERINEUM.

BY

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So much has been said and written about ruptured perineum, that I doubt the propriety of again bringing before the profession such a hackneyed subject, but as my method of managing these cases has been somewhat different from that usually employed and recommended, I thought I had excuse enough to write a few words on this question.

When I started out in practice, I was an enthusiastic advocate of the immediate repair of a lacerated perineum. It was then, as now, my misfortune to be unable to prevent, in some cases, a tearing of the perineum, and in such cases I always sewed it up immediately. But I soon found that some cases would heal readily and others would not. Careful investigation showed that the cause of failure, in most cases, was that the swelling and tumefaction which are found in cases of prolonged labor would subside, and that the wound would gape, no matter how tight I would draw the sutures. In cases of edema of the vulva, the same result would follow. Other physicians have also found this to be the cause of failure. It occurred to me at the time I first had impressed on my mind the true cause of non-union that, if I could get rid of the infiltration I ought to have generally a good result. The best remedy to remove swelling is—time. If I would wait for twenty-four to thirty-six hours the swelling would have subsided, I could introduce my sutures, bring the raw surfaces well together, and need not fear gaping and non-union or healing by granulation.

It is more than ten years since I first tried this method on a French Canadian woman, with good results; I have no record of the case, however.

On April 12th, 1876, I delivered Mrs. D., aged thirty-two, of her first child with forceps, and ruptured the perineum up to the rectum. I waited twenty-four hours, then, assisted by a student, sewed the perineum, and got a most excellent result.

Since then I have had quite a number of cases, and have treated them all in the same manner, one case being that of

Mrs. T., aged twenty-two, with her first child. She was suddenly taken with puerperal convulsions one morning two weeks before the expected time of her confinement. I was immediately called (on May 19th, 1881), and found the lady semi-comatose and having attacks of convulsions every half-hour. Large doses of chloral controlled the convulsions and brought on sleep. The os uteri was just commencing to open. I waited for twelve hours, when the os was open, and as symptoms of convulsions again appeared, although Mrs. T. was still semi-comatose from the effect of the chloral, I applied the forceps and delivered her of a living child. During delivery, however, I could not prevent laceration; although I was on the point at one time of making lateral incisions, but desisted, as I would rather run the risk of rupture and sew it up. There was great swelling, so I waited for thirty-six hours, and then, assisted by Dr. Erichsen, put her under chloroform and sewed the rupture. I found a few gray gangrenous spots, these I nipped off with the scissors before bringing the sutures together. The result was good, the wound healed, and Mrs. T. is now again pregnant.

Other cases in my practice, where more or less rupture occurred, offer nothing particular of note until I was called August 15th, 1882, to attend:

Mrs. G., aged twenty, with her first child. She had been in labor about twenty-four hours, under the care of a midwife, and as no progress seemed to be made, I was sent for, and found the patient weak and exhausted, the perineal and other muscles very rigid, the child large, head just engaged, pains poor and inefficient. The forceps were indicated, and putting the patient under chloroform, I applied them. Working slowly, I had the woman delivered in the course of an hour of a dead child weighing thirteen pounds. But in spite of my slow work, the perineum ruptured through the sphincter ani into the rectum. I waited twenty-four hours, and assisted by my colleague, Dr. N. W. Webber, put her under chloroform and sewed the tear in the usual manner, using three silver-wire sutures. As the parts were still somewhat swollen, I drew the sutures quite tight. Still the next day I found a slight gaping at the upper part of the wound, where the third suture was inserted, that is, nearest the

fourchette. The second day the gaping was still more marked, although the lower part of the wound seemed to be healing by first intention. On this second day I twisted the suture where the gaping was, and brought the wound pretty well together, although not perfectly. I repeated this procedure the third (next) day, getting the wound still more together. I ordered carbolized injection and prescribed quinine, opium, iron, etc. The bowels were kept confined for eight days, when a dose of castor oil and a rectal injection caused a movement. On the ninth day I removed the sutures, and found the wound healed by first intention, except where the gaping had existed, where it was only partially healed, but was healing by granulation. In spite of an attack of acute metritis, the ultimate result was good; she has complete control of the sphincter ani, uterus in place and of normal size three months after delivery, when she again became pregnant, and I delivered her August 31st, 1883, of a nine-pound boy. This labor commenced at 5 P.M., and at 7.45 P.M. the child was born, without instrumental aid; the perineum remaining intact.

This last case taught me a good lesson, as it showed that, if I could tighten the sutures the second and third day after sewing the wound, I would get better result, when gaping occurred, by tightening the sutures as often as necessary. The best plan seemed to me to slip over the wire when inserted a number of perforated shot and compress the last one; then, if gaping occurred, to draw out the silver wire with the last shot (thus bringing the wound together), then to compress the next shot, and cut off the projecting wire. This process to be repeated as often as needed. I delayed the writing of this article until I had tried this way of treating a case of freshly ruptured perineum, but fortunately for my patient (and as I thought unfortunately for me), although I had my share of confinements to attend to, I did not come across a case of ruptured perineum until I was called August 6th, 1883, to

Mrs. S., aged thirty-six, in her first confinement. She had been in labor for twenty-four hours under the care of a midwife. The head was well engaged, but the pains had nearly ceased, and the perineum was rigid. I put her under chloroform and applied forceps. In little less than an hour I had her delivered of a living boy weighing eleven lbs., but the perineum was ruptured through the sphincter and into the rectum. I intended to sew it up, but on account of lack of proper shot, the holes being too small for my wire, I had to desist and wait until the next day, when I sewed it without the shot being used, as I intended. I got a good result as I thought, until the fifth day, when I found a vagino-

perineal fistula. The sutures were removed on the tenth day; the result was good, although the fistula was still present, but it healed in the course of three weeks without any other treatment than carbolized vaginal injections.

My next case occurred October 31st, 1883, being

Mrs. D., aged thirty, first confinement. She was pregnant with twins, and as a consequence very large, had frequent attacks of neuralgia of the womb of a malarial origin, and twenty-four hours preceding the onset of labor had the most severe attack I ever saw, and which was but slightly modified by treatment. October 31st, at 9 A.M., regular labor pains came on, the os gradually opened, and by 10 o'clock was thoroughly dilated. During my absence one bag of water had ruptured. When I arrived at 11 A.M., I found a brow presentation; fronto-iliac anterior left. On account of the absence of amniotic fluid and the powerful contractions, it was impossible to change the position, as the head was already engaged. The head descended, but as the uterine pains lessened in force, the head finally became fixed, and as the woman was weak, I applied the short forceps and delivered her of a living boy weigh-

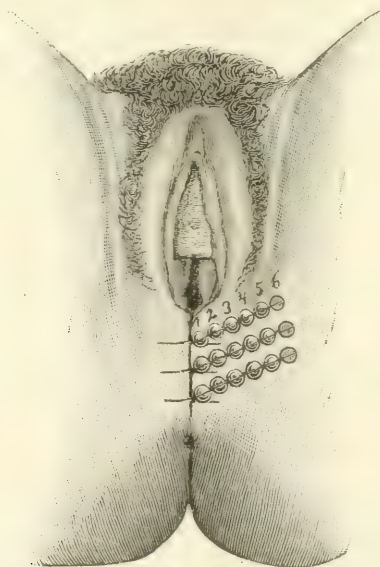


FIG. 1.

ing 7 pounds 1 oz. At 4.30 P.M., although extraction was slow, still the perineum ruptured up to, but not into the sphincter. The second child presented in the first position. I ruptured the membranes and applied the forceps and delivered her of another living boy weighing 6 pounds 15 oz., the two children together weighing just 14 pounds. I then removed the two placenta.

The woman was quite exhausted, and not having a competent assistant, it being also dark, I thought it advisable to wait till morning and then sew the perineum. I went the next morning at 9 A.M., and assisted by Dr. Erichsen, put her under chloroform and introduced three silver sutures in the usual manner, although the parts were still much swollen. After having thoroughly cleansed the wound, I brought the two ends of the lower or first suture together and slipped over them six larged perforated shot; these were forced down as much as was thought necessary, then the last (sixth) shot compressed, and the wire cut close up to the shot. The second and third sutures were treated in the same manner, as the accompanying cut will show. Twelve hours later I tried to tighten the sutures, but found still too much swelling. The next morning, that is, twenty-four hours after the introduction of the sutures, I took a shot compressor and firmly grasped shot No. 5 and with a pair of forceps took hold of shot No. 6 and pulled it and the two ends of the suture out as much as I could, then compressed shot No. 5. The accompanying cut will show it better than I can explain it (Fig. 2). I then cut off the wire just outside of shot No.

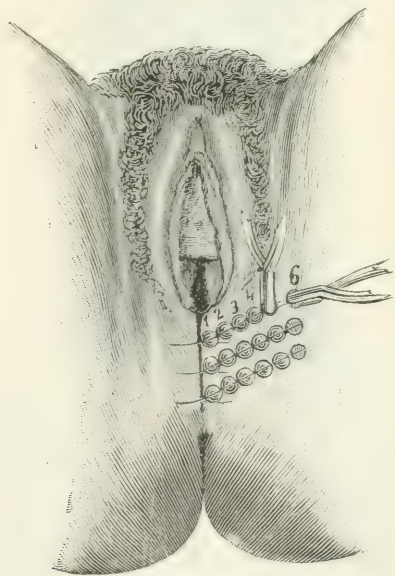


FIG. 2.

5. The two other sutures were treated in a similar manner. I could pull out the wire about one-half inch, which, as said, I cut off. I now had 5 shot remaining (Fig. 3). In the evening I repeated this procedure, that is, I took hold of shot No. 4, pulled out shot No. 5, and compressed shot No. 4 and cut off about one-quarter inch of the wire. I repeated this next morning, pulling

out again about one-quarter inch and cutting it off. The swelling had now all subsided, the wound being well united, it was not necessary to repeat the manipulation. I now had remaining on the wire three perforated shot, the last, No. 3, compressed, the others loose. I had prescribed quinine, two grains every two hours for the first day, and a mild diet. The third day I ordered castor oil, which moved the bowels freely. Then I prescribed nitro-muriatic acid and pepsin (which she took for three weeks), and a more nourishing diet.

Neither in this nor in any of my cases did I draw the urine with a catheter, but always allowed them to pass the urine in a bed-pan or chamber.



FIG. 3.

Mrs. D. got along well, although I had to repeat the cathartic on the sixth day. On the ninth day I removed the sutures by simply cutting the wire between shot Nos. 2 and 3. Shot Nos. 1 and 2 could now be removed and one end of the silver wire grasped with forceps and removed (Fig. 4).

The result in this case was very good; the perineum was apparently perfectly restored, and on the fourteenth day I allowed my patient to get up for a few hours at a time. At present she has her hands full nursing and feeding her two boys.

I never had a case where I could so easily remove the sutures as in this case, except in some cases where I had used perforated lead plates, which, however, for various reasons, I do not use at present. It seems to me a good plan to use the per-

forated shot also in a case of ruptured perineum which is of long standing, and where the parts must first be denuded, as the facility with which the wire can then be removed is of great advantage in such cases.

The disadvantage and objection to my first plan is, that we must put the patient under an anesthetic often the second time in twenty-four or thirty-six hours, but the advantage is that we more often get good results. Against the second plan I can think of no objections.

I should be pleased if other physicians would try this

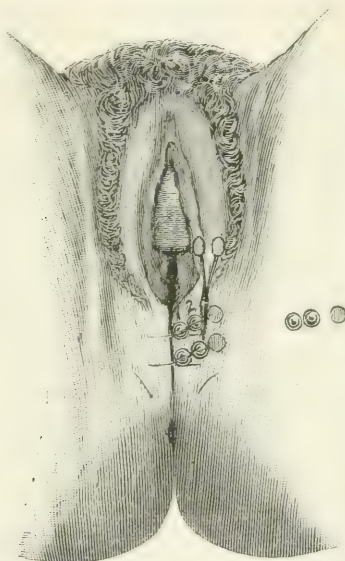


FIG. 4.

method and report their results, as only after extended trial can this method be placed in its proper position.

It seems to me not going too far if I conclude :

As the cause of failure in some cases of non-union is the tumefaction and resulting gaping of the wound, if we either wait with the sewing for from twenty-four to thirty-six hours until the swelling has subsided, or if we use the method I have described (or a similar method), which allows the tightening of the sutures when needed, we ought to have few failures.

AN INQUIRY CONCERNING THE RELATIVE INFLUENCE OF THE SEX OF THE FETUS IN UTERO, ON THE MENTAL, PHYSICAL, PHYSIOLOGICAL, PATHOLOGICAL, AND DEVELOPMENTAL CONDITION OF THE MOTHER DURING GESTATION, LACTATION, AND SUBSEQUENTLY.

BY

JOHN STOCKTON-HOUGH. A.M., M.D., Mag. Chem.

(Continued from p. 133.)

8. *Influence of Sex on Production of Edema of Vulva and Members.*

THE curious and popular book of Albertus Magnus, "De Secretis Mulier," is full of wisdom on the subject of the signs of conception, by which a male may be distinguished from a female, as the following words of his translator¹ abundantly testify: "We shall now proceed and communicate to the world what are the signs or indications of a man or woman child, and what I shall here lay down may be depended on as certain. If a woman has conceived of a boy, the color of the face will be red, and her motion will be light and easy. A second sign of a boy is if the belly swells on the right side and grows round and plump. Thirdly, if the milk of a woman's breast be thick and well digested, so that when it is dropped upon anything that has been well rubbed it keeps close in its body and does not separate or divide itself, then you may depend upon it that such a woman has conceived of a boy.

"Fourthly, if the milk so digested, or if a drop of blood drawn from the right side be put into clear water or into urine and sinks directly—that is, perpendicularly—to the bottom, this is a sign of a male child. But if it floats or swims at the top or upon the surface of the water, this is a certain sign of a female."

[Some of these tests are resorted to at the present time in Italy in selecting a wet-nurse to test the quality of the milk—not by old women, but by professors of obstetrics, such tests having been witnessed by the author in 1874.] The right breast is said to be larger in conceptions with males.

¹ John Quincy, M.D., Translation of Albertus Magnus, 8vo, London, 1725, pp. 83-85.

“Sixth, when a woman, as she begins to walk, moves the right foot forward and not the left, it is a male child, and the contrary when it is a female.”

Aristotle, B. vii., says that “those that are pregnant with a male fetus, usually pass through the time more easily, and retain a better color throughout. If a female is conceived, the contrary is the case; for they are generally more discolored, and suffer more during the period of gestation. In many cases the legs swell, and a swollen condition of the flesh is also common. In some women, however, the condition is the contrary; pregnant women are apt to have all sorts of fancies; these are strongest when a female is conceived.”

Pliny, N. Hist., B. vii., Ch. 5, says that with a female conception “the mother experiences an almost insupportable weight, there is a slight swelling of the legs and groin, and the first movement of the child is not until the ninetieth day.”

Concerning the swelling of the legs and groin mentioned by ancient authors, it is clearly a difficult matter to decide whether it occurs more frequently in case of gestation with girls than with boys, as one is seldom called upon to treat such a condition in the human female. However, in the case of phlegmasia alba dolens it would be interesting to determine if this condition were more frequent in female births than in those of males. As tending to confirm the alleged swelling of the groin in female conceptions, M. Lemoal, of Rennes, France, has observed that the “bearing” or vulva of cows was perceptibly more swollen in the case of those that afterwards gave birth to female calves than in the case of those that gave birth to males, so much so that he was always able to predict the sex of the calf by observing the “bearing.”

9. *Influence of Sex of Fetus on Inoculating the Mother with the Constitutional Peculiarities of the Paternal Organism.*

It is a well-known and undisputed fact that the fetus in utero inoculates the mother during gestation with physical peculiarities of the male by whom she was impregnated, and is liable to imprint one or more of such peculiarities on her subsequent offspring, even when by another father.

Now, we wish to determine whether the male or female fetus has the greater influence in thus inoculating the mother.

The generally-received opinion, as taught by Aristotle, Hip-

pocrates, and other authors—and elaborated in an interesting article by the author of this paper,¹—is that the female children resemble more their father than their mother, and the males incline more to their mother than to their father.

Presuming this to be true, we would infer that, as females resemble their father more than their mother, they would engraft on the mother more of the peculiarities of the father than would a male conception. The greater extent of placental connection in the former case would also favor this view, though the greater length of gestation with males might interpose a modifying influence. I am therefore inclined to believe that female conceptions impress the mother more than the male with peculiarities of the father, as we have shown that the former impresses her physically more.

10. *Influence of Sex of Fetus on the Duration of Gestation.*

Professor Hamilton, in his paper,² has stated that “he has ascertained by facts which are incontrovertible, that the more calves the cow has had the longer is the duration of her pregnancy,” while Tessier declares with equal pertinacity that he has ascertained beyond doubt that the duration of gestation “is not affected by the age, constitution, or food of the animals; nor by the size, strength, or sex of the fetus,³ but more recent investigations have at least rendered doubtful the truth of his conclusions in these latter respects.⁴

Sir E. Home mentions the fact that where animals of different species breed, the period of gestation is the longest time of the two; “the mare covered by an ass goes eleven months; and the ass covered by the horse goes *eleven months*, although *ten is her usual period.*”⁵

On the authority of Herman Von Nathusius,⁶ cited by Darwin,⁷ we are told “that Merino and Southdown sheep, when both have long been kept under exactly the same conditions,

¹ The Law of Transmission of Resemblance from Parents to their Children. N. Y. Med. Rec., 1874. See Dr. Harvey, Monthly Journ. Med. Sci., 1849-50; also, Sedgwick, Med.-Chir. Trans., 1861-2.

² Philos. Trans., 1822.

³ Montgomery, Signs, etc., of Pregn., p. 532.

⁴ Mém. de l'Acad., p. 15 et seq.

⁵ Prac. Obstet., p. 109.

⁶ Animals and Plants under Domestication, Vol. 1, p. 97.

⁷ Transl. in Bull. Soc. Imp. d'Acclimat., tome ix., 1862, p. 723.

differ in their average period of gestation, as seen in the following table:

Merinos,	150.3 days.
Southdowns,	144.2 “
Half-bred Merinos and Southdowns,	146.3 “
Three-quarters blood of “	145.5 “
Seven-eighths “ “ “	144.2 “

“Nathusius remarks that, as Southdowns grow with remarkable rapidity after birth, it is not surprising that their fetal development should have been shortened.”

It appears that domestication and high breeding shortens the period of gestation, according to these two observers.

Mr. F. Burke,¹ in reciting the results of M. Tessier's experiments concerning the period of gestation in cows, says: “In most cases, therefore, between nine and ten months may be assumed as the usual period; though with a bull-calf she has been generally observed to go about forty-one weeks, and a few days less with a female.”

Of twelve cows observed by Dr. Nicolls,

1 went	276 days.
1 “	279 “
2 “	282 “
1 “	283 “
1 “	285 “
2 “	286 “
1 “	288 “
1 “	293 “
1 “	295 “
1 “	303 “

12

“It will be observed here that six, or one-half of the whole number, exceeded the ordinary period of 285 days; and of these six calves four were bulls. Dr. N. observes that ‘according to this record the bull calves were carried from one to three weeks longer than the heifers.’

“With regard to any influence or relation between the sex of the offspring and the prolongation of the gestation, Lord Spencer thinks there is some foundation for the opinion; since

¹ British Husbandry, Vol. ii., p. 438.

it appears that from the cows whose period of gestation did not exceed 286 days, the number of cow-calves produced was 233, and bull-calves 234; while from those whose period exceeded 286 days the number of cow-calves was only 90, while the bull-calves was 152.

"Lord Spencer having subsequently bought another bull, found that of sixteen cows impregnated by this bull, which was aged, the average period of gestation was $290\frac{1}{4}$; and of fifty-nine other cows in calf by this bull, the average period was 288 days; or, taking the whole seventy-five cows, the average was $288\frac{1}{2}$, an excess of about four days over the ordinary period. His lordship also observes that while of the 764 cows first kept account of, 185 went less than 281 days, *not one* of the seventy-five cows in calf by this bull did so. And again, while only one-seventh (111) of the same 764 went above 289 days, between one-third and one-half (twenty-nine out of seventy-five) of those covered by this new bull went above 289 days."¹

Mr. C. N. Bement,² of Albany, N. Y., published the result of his observations on the period of gestation in cows, in 1845, which has been tabulated by Dr. Beck.³

				Average period of gestation.	
In 1839 three cows produced heifer calves, 284 days.					
1840 six cows	“	“	“	287	“
1841 eight cows	“	“	“	286	“
1842 four cows	“	“	“	284	“
1843 five cows	“	“	“	282	“
1839 eleven cows	“	bull	“	280	“
1840 seven cows	“	“	“	299	“
1841 three cows	“	“	“	293	“
1842 nine cows	“	“	“	287	“
1843 six cows	“	“	“	282	“
62 cows average	.	.	.	285	“
36 cows, average time with males,	.	.	.	288	“
26 “ “ “ “ females,	.	.	.	282	“

The shortest period was 213 days, and the longest 336, a difference of 123 days.

As the cow is in heat every two weeks, and varies on an

¹ Montgomery, Signs, etc., of Pregnancy, p. 531.

² Cultivator, July, 1845; cited in Amer. Jour. Med. Sci., Oct., 1845.

³ Medical Jurisprudence, 1850, vol. i., p. 576.

average six days in the duration of gestation of males and females, and as the human female is in heat every four weeks, and varies about twelve to fourteen days in the average period of gestation for males and females, we can readily see the analogy, and deduce the law, that is, the difference in the duration of gestation with a male fetus is one-half the intermenstrual period longer than with a female.

The period of gestation in the same woman with a fetus of the same sex may vary from several circumstances, among which I may mention: age, state of health, number of pregnancy, whether living with her husband or not, frequency of intercourse, emotions of fear, grief, joy, etc.; age, physical condition, etc., of the father of her child.

This difference in the duration of pregnancy, where it goes beyond the usual period, is clearly independent of the sex of the fetus, though we have shown that in cases of protracted gestation there is usually an excess of males. Lord Gardner, in the famous Peerage Case, had a son born 312 days after his absence from his wife, who was decided to be legitimate.

“There is a prevalent belief among farming men, and I believe, farmers, that, when the time of gestation of a cow is longer than usual, the produce is generally a male calf. I confess I did not believe this to be the case, but this table shows that there is some foundation for the opinion. In order fairly to try this, the cows which calved before the 260 days, and those which calved after the 300th, ought to be omitted as being anomalous cases, as well as the cases in which twins were produced, and it will then appear that, from the cows whose period of gestation did not exceed 286 days, the number of cow calves produced was 233, and the number of bull calves 234; while from those whose period exceeded 286 days, the number of cow calves was only 90, while the number of bull calves was 152.”

As Earl Spencer considers 284 days as the usual period of gestation in the cow, and as his figures show that an equal number of each sex born of cows whose period is 284 days, it is only fair that the relative proportion of male to female births should be shown from this period; then we should have from the 284th to the 300th day, 141 cow calves (37.7 per cent), to 33 bull calves (62.3 per cent). If this same method be pur-

sued in calculating the births from the 260th to the 284th day, we find 149 cow calves (55.3 per cent), against 120 bull calves (44.6 per cent).

We have taken the trouble to separate the sexes of the calves given in the tables¹ of Lord Spencer, and find that the average duration of gestation of all the cows that gave birth to heifer calves was 282.7 days, whereas the bull calves were carried 285.1 days, as will be seen from the following table, viz.:

Total number of cows, 764.

Bearing heifer calves, 354; average dur. of ges. 282.7	} 283.9
“ bull “ 401; “ “ “ 285.1	
“ twins, both heif., 7; “ “ “ 277.6	} 276.8
“ “ “ bulls, 5; “ “ “ 274.4	
“ “ cow & bull, 11; “ “ “ 278.3	

Proportion of males to females in single births, 115.3 to 100.

“ “ “ twin “ 84 to 100.

Average duration of gestation, single births, 283.9 days

“ “ “ multiple “ 276.8 “

From an examination of the preceding table it will be seen that the average duration of gestation, when more than one fetus is carried, is seven days shorter, and that twin bulls are carried $10\frac{1}{2}$ days less than a single bull. As the male fetus is heavier than the female, from what we have noted above it is clear that the total weight of the fetal mass has a decided influence on the duration of gestation—shortening it considerably when that mass is excessive in bulk.

M. Gaston,² in his researches on the duration of gestation, found that boys were carried from two to seven days longer than girls, as may be seen from the following table:

PRIMIPARÆ (AVERAGE).

Cases.	Age Mother.	Dur. Mens.	Dur. Gest.	Wght. of Child.
10	23	5.45	276.2	3145 gm. boys.
21	22	4.60	274.4	3151 “ girls.

MULTIPARÆ (AVERAGE).

12	29	4.5	285.2	3280 gm. boys.
18	26	5.1	277.6	3250 “ girls.

In nearly 800 cases recorded in the obstetrical wards of the

¹Journal of the Royal Agricultural Soc., 1840, vol. i., pt. 2, p. 165.

²Nouvelles Recherches sur la durée de la grossesse, ses rapports avec la conception, l'ovulation et la menstruation, 8vo, 1876.

Philadelphia Hospital, I found, from a careful tabulation of the cases, that the duration of gestation was on an average always longer with male children than with female—as may be seen by an examination of the following table:

WITH GIRLS.		MONTHS.	WITH BOYS.		MONTHS.
168 cases first pregn'y,		8.87	153 cases first pregn'y,		8.95
48 " first "		8.81	54 " first "		8.92
51 " second "		8.85	17 " second "		9.03
44 " second "		8.97	44 " second "		8.98
<hr/>			<hr/>		
311 " 1st & 2d "		8.87	268 " 1st & 2d "		8.97

or $\frac{1.0}{100}$ of a month (3 days) longer with boys than with girls.

Ravn found in 40 cases that the average number of days from conception to quickening was 134 with boys and 149 with girls; while the average duration of gestation in 150 cases was 277.4 days for boys and 276.7 for girls. For the year 1854, boys, 278.8; girls, 272.8. 1855, boys, 279.9; girls, 277.7.

The ancients having discovered that females were carried in the womb a longer time than males before quickening, reasoned from analogy that gestation lasted ten months with girls, whereas it was but nine months with boys.

Bonaventura, the author of a curious and rare book as large as a family Bible, of which I am the fortunate possessor of a copy, with much learning spread over nearly a thousand pages, to prove that eight months is the natural period of gestation in the human female, against the vulgar opinion, argues in the opposite way, from effect to cause, maintaining that boys are more slowly formed than girls, and that is why they are carried longer—as may be seen from the following:

"Hoc posito sequitur, verisimè à D. Thoma dictum esse, masculi materiam majori, ac multiplici terminatione indigere quàm fœminæ materiam; hoc n. non universaliter pronunciat D. Thomas, nâ sic à veritate, ac preceptore dissentiret, cùm mas brevior tempore formetur in utero quàm fœmina, ut superius vindimus, potissimumque; libro tertio, et sic minori indigeat terminatione. . . .

"Id est et inter mares natura fœminam referentes, et inter fœminas marem. Unde verè in partu hoc, de quo agit D. Tho-

¹ Om Svangerskabstidens Graendser, 8vo, Kjobenhavn, 1856, pp. 31, 36.

mas, cunctabitur mas tardius conformatus, proindeque tardius veniens in lucem quàm sua natura patiatur.”¹

By way of variety in opinion we may cite Valesius, Sacrae Philosophæ, who maintains that because we admit that the male is sooner formed than the female, it does not follow that gestation is shorter with the male, but that what the female loses in slowness of formation up to the period of quickening, she makes up by greater rapidity afterwards, and is born after gestation of equal length with the male, as may be gathered from the following:

“Necesse est ergo fœminæ quam mares tardius accipiant figuram et moveantur; non tamen proinde et tardius edantur in lucem, sed eisdem temporibus quibus mares; quia quanto tardius accipiunt figuram, et moveatur, tanto, postquam formata sunt, citius veniunt ad incrementum.”

11. *Influence of Sex on the Position and Presentation of Fetus.*

Gorræus, in his Annotations on Hippocrates concerning the Seed, says that hitherto all men have followed Hippocrates, who teaches that the position of the fetus in the womb is the same whether it be a boy or a girl, except that “the male hath this difference with the female, that he is turned with the fore part of his body towards the *abdomen* of his mother; but with his back part leaning upon his mother’s back.

“But the female has the contrary situation, which undoubtedly appears evident at the birth, which, when it comes, after the child is turned, the male seems for the most part to come forth and exhibit itself with the face turned towards the backbone and *podex* of the mother, but the female after a different manner.”

Mercurio² gives illustrations of the position and presentation of the fetus at the time of birth according to the sex. On page twenty-seven, he illustrates “Il sito del parto naturale, nel quale nascono così i maschi come le femine rare volte. [The natural presentation, in which males are born, as are also the females on rare occasions. This is the head-first, occipito-posterior position.]

¹ Bonaventura, F.: *De Natura Partus octomestris, adversus vulgatum opinionem. Libri decem*, 944 + 40 pp. folio. Venetiis, 1602. Lib. 9 Cap. xliii., p. 841.

² Scipio Mercurio, *La Commare*, 4to, Venice, 1642.

On page twenty-eight he gives an illustration of "Il sito del parto naturale, nel quale nascono così i maschi come le femine per lo più." [The natural presentation, in which males are (sometimes) born, as are also the females for the most part.] This is the head-first, occipito-anterior position.

Now, whether there is any truth in this assertion of Mercurio I am not prepared to state, as statistics of labor do not usually separate the sexes in head presentations. It is quite probable that the fact of the female fetus being smaller than the male would exert some influence on determining the presentation or causing a difference in the frequency of the same presentation in the different sexes.

Physicians and philosophers in all times have taught that the male fetus is usually to be found on the right side of the uterus, and the female on the left—or that the uterus inclines to the right side of the body with a male, and to the left side when carrying a female. This is probably not as invariable as they would have us believe, for Schuré, p. 389, found that the fetal heart was heard 238 times on left side, and 141 times on the right, and Jacquemier found the pulsation 34 times on the left and 22 on the right.

12. *Influence of the sex of the fetus on the duration of labor in general, and as causing painful, protracted, difficult, and fatal labors in particular.*

Statistics show in ordinary labors the duration of all the stages combined is greater with male children than with female, as may be seen from the following, elaborated by the author from the register of the Philadelphia Hospital.

PRIMIPARÆ.

153 males.	20.21 hrs.,	weight, 7 lbs. 7.3 oz.	Single women.
54 "	20.36 "	" 6 " 14.8 "	Married "
168 females.	17.41 "	" 6 " 13.2 "	Single "
48 "	16.26 "	" 6 " 7.3 "	Married "

MULTIPARÆ (2D LABOR.)

17 males.	11.03 hrs.,	weight, 7 lbs. 12. oz.	Single women.
44 "	13.30 "	" 7 " 12.5 "	Married "
51 females.	16.16 "	" 7 " 9. "	Single "
44 "	11.18 "	" 7 " 7. "	Married "

Sir James Y. Simpson, in his memoir,¹ shows that "of mothers that die under parturition and its immediate consequences, a much greater proportion have given birth to male than female children."

From Dr. Collins'² book in which 16,654 births are recorded, he finds that of 154 women dying in childbirth, 105 gave birth to male children and 49 to female children, or as 214 male births to 100 female.

The second proposition of Dr. S. is, that "among labors presenting morbid complications and difficulties, the child is much oftener male than female."

The following table will show the number and proportion of sexes of those cases included under the second category.

NATURE OF COMPLICATION.	TOTAL CASES.	NO. MALE CHILD'N	NO. FEM. CHILD'N	PROP. OF MALES TO FEMALES.
Tedious labors.....	109	65	54	148 to 100
Convulsions.....	28	17	11	153 to 100
Puerperal fever.....	88	54	34	161 to 100
Rupture of uterus.....	34	23	11	207 to 100
Post-partum hemorrhage.....	44	31	13	240 to 100
Forceps cases.....	24	16	8	200 to 100
Crotchet cases.....	74	50	24	208 to 100
Total.....	401	256	155	165 to 100
Placenta previa.....
Extrauterine gestation.....

Comparative size of male and female fetal head.

Dr. Clarke gives the following measurements of sixty male and 60 female children at birth.

AVERAGE CIRCUMFERENCE OF HEAD.		AVERAGE DIMENSIONS FROM EAR TO EAR.
In male.....	13.983 inches.	7.429 inches.
In female.....	13.617 "	7.221 "
	0.366 inches.	0.208 inches.

1. "The head of the male infant, when measured across from

¹ Memoir on the sex of the child as a cause of difficulty and danger in human parturition. 8vo, pp. 55, Edinb., 1844. From Edinb. Med. and Surg. Journ., Oct., 1844.

² A practical treatise on midwifery, containing the result of 16,654 births occurring in the Dublin Hospital during a period of seven years, commencing Nov., 1826. 8vo, London, 1836.

³ There is an element of error in this proportion arising from the fact that in the general births there were 106 males to 100 females, involving a reduction of six per cent.

ear to ear, over the fontanelle, is about $2\frac{5}{7}$ lines, or nearly two-eighths of an inch greater than the female.

2. "In circumference, the head of the male is $4\frac{2}{3}$ lines, or almost precisely three-eighths of an inch greater than that of the female. Hence,

3. "The *transverse* diameter of the male head is nearly one eighth of an inch greater than that of the female child."

Simpson deduces from the figures of Dr. Clarke that the proportion of the surface of the head of the male new-born child to that of the female is nearly as nineteen to eighteen, or the surface of the head of the female is one-nineteenth part less than that of the male.

Professor Reid found the weight of the

Brain of 53 adult males to be 3 lbs. 2 oz. $3\frac{1}{2}$ dr.

" 34 " females " 2 " 11 " $8\frac{1}{2}$ "

Average difference, . . . 5 oz. 11 dr.

Professor Tiedeman states that the female brain weighs on an average eight ounces less than that of the male, "and this difference is already perceptible in the *new-born child*."

Tiedeman says that the weight of the brain of a new-born child is relatively to the weight of the body as one to six, while in the full-grown adult it is as one to forty, or nearly seven times greater in the child than in the adult.

According to Braxton Hicks¹:—"At birth the skull of the male head is in a more advanced state of ossification than that of the females. This is so noticeable that, before the child is born, one is able to pronounce rightly in eight out of ten cases whether it be a male or female.

Dr. Clarke² attempts to explain the assumed greater mortality of male fetuses *before* birth than female, by saying that they are of larger size, and consequently more difficult of delivery, and their greater liability during their intrauterine life to disease and debility, from their requiring, in consequence of their size, more actual nourishment from the mother than smaller female children stand in need of.

Quetelet³ also tries to explain the same matter by saying

¹ British Medical Journal, April 17th, 1877.

² Observations on some causes of the excess of mortality of males above that of females. Philos. Trans., 1786, p. 353.

³ Treatise on Man, p. 25 and 30.

that "It appears beyond doubt that there is a particular cause of mortality which attacks male children by preference, before and immediately after birth. It will be interesting to investigate the causes of a circumstance which is so unfavorable to the male sex."

"If," he continues, "we were desirous of guessing at this point, we might say, with those who suppose that a male conception requires a certain excess of energy in the woman, that this excess of energy was absent or wanting during the growth of the fetus, and that energy failing, the child would suffer more from it if a boy than if a girl. Hence the proportion of dead births between the sexes," etc.

Dr. Graves¹ also contends "that a *greater* mortality before birth prevails among males than females."

Dr. Simpson contends that it is fallacious to assume that a greater number of males than females are dead before labor commences.

This he sets forth in the following table taken from the statistics of Dr. Collins:

STATE OF CHILDREN.	TOTAL CASES.	MALE.	FEMALE.	PROP. OF MALES TO FEMALES.
No. of still-born putrid children...	527	257	270	95-100
" " at full term & putrid	296	148	148	100-100
" " premature still-births	293	146	147	100-100

Dr. Simpson points out that this table "seems particularly valuable and instructive," as "it demonstrates satisfactorily that the intrauterine morbid agencies (whatever they may be) which act fatally on the fetus before birth, act equally on the female as on the male child; and that it is to other agencies than these that we are to look for the remarkable proportion of male over female deaths which is observable among still-births."

Further on he observes "that of *all* the children that had perished from intrauterine causes, and before the commencement of labor (as demonstrated by their putrid state), the females were even more numerous than the males.

"The whole series of facts prove (contrary to what is generally alleged) that the proportion of male children that die *before* birth is not greater than the proportion of females.

¹ Dublin Med. Jour., about 1836 or '7.

Indeed, if we deduct the usual six per cent for the normal over-proportion of males, the ratio of girls dying before parturition would be found to be greater than that of boys—a conclusion which the first column would seem to go far to corroborate and strengthen, for in that computation the number of the dead females distinctly and considerably exceeds that of the dead males.

“ This result, with regard to the equality of the sexes among putrid and premature still-born children, becomes only the more striking when we couple and contrast it with the fact which we have already brought out, that of the children who are still-born, and *not* putrid, as many as two out of every three are boys. In other words, among the infants that die *before* labor, the females are equal, if not greater, in number than the males. Among the infants that die *during* labor the males are raised to the high proportion of 150 boys for every 100 girls.”

The following table will show more clearly by comparison the influence of the sex of the child on the arrest of development and death of the fetus in utero.

STILL-BORN CHILDREN OCCURRING AMONG 16,654 BIRTHS.

Still-born children not putrid at term dying during part.	No. Males.	No. Females.	Prop. of Males to Females.
594	357	237	151 to 100

STATE OF CHILDREN.	TOTAL CASES.	MALE.	FEMALE	PROP'N OF MALE TO FEMALE
Still-born putrid children.....	527	257	270	95 to 100
Still-born at full term and putrid.....	296	148	148	100 to 100
Premature still births	293	146	147	100 to 100
	1,116	551	565	97½ to 100

Supposing equal No. male and female concep-

tions, deduct 6 per cent, 92 to 100

Proportion of sexes in all births, 106 to 100

Contrary, therefore, to the hitherto received opinion, we are forced to conclude that influences which cause the death of the fetus before labor act more frequently on the female than on the male, a fact which would tend to corroborate the view we have taken, viz., that the demands of the female fetus on the mother are greater than those of the male. This is in harmony with the observations of Morgagni and Desormeux, who enter-

tain the opinion that female fetuses are more common in abortions than males.

Dr. Simpson is of the opinion that the excessive mortality of males in childbirth is due solely to the fact of the larger size of the fetal head of males; while this is true as far as it goes, in searching for a more proximate cause, we are persuaded that should a given number of male fetuses be subjected to a certain pressure and detained for a certain time in the maternal passages, and the same number of females *of exactly the same size and conformation of head* be subjected to exactly the same conditions in labor, the mortality would still be somewhat greater among the males than among the females, on account, as we believe, of the greater development and more sensitive organization of the male brain.

Females are less susceptible to noxious and morbid influences than males, as is seen in cases of deaths from asphyxia from carbonic oxide gas, the male dying first; and where there have been two individuals of the same sex, one ill and the other in good health, exposed to the gas from charcoal, the one in good health succumbed first.

Dr. Farre pointed out in the Second Annual Report of the Reg. Gen., that "the diseases of the nervous system are *twenty-three per cent* more fatal to males than to females, *the chief difference arising from the diseases which affect children.*" It is worthy of note here, that of all infantile diseases, whooping cough is the only one in which the mortality is exceptionally excessive for females.

Influence of Sex on the Duration of Labor.

Dr. Simpson gives the duration of labor in 427 cases (249 male and 178 female), registered in the Edinburgh Hospital, the average duration of each labor

With male children was, . . . 10 hours and 38 minutes.

With female children, . . . 9 " " 34 "

Av. greater length of male birth, 1 hour and 4 minutes. Cases of speedy labor, say less than three or four hours, are not included in the above.

Dr. Simpson shows from Dr. Collin's tables that the average greater length of the male births with 501 still-births was 2

hours and 17 minutes; in morbidly tedious labors, 3 hours and 6 minutes; cases requiring the crotchet, 4 hours 2 minutes leading to death of mother, 5 hours 11 minutes.

From this he concludes that "the average duration of labor is longer with male than with female children, and the difference in this respect between male and female births becomes increased in length when the labors become more severe and dangerous in their character."

Bonaciolus and other ancient authors taught that the labor was slower, the pains more continuous but more obtuse, with boys, whereas they were quicker and easier, though more acute, and molested the woman through a greater space of time with girls, as may be gathered from the following:

Plerunq.: verò diluta pallidiusculaq.; sanieo si fœmina natum itura est: cruenta, si mas, prefluit. Ceterû maris ac fœmelle partus discrimine illo interstinguntur: Quoniam hujus partus tardior, dolores etiâ continui ac perpetes, sed obtusiores: illius celerior simul et faciliior, ac acriores et longè molestiores dolores.¹

(To be concluded.)

A REVIEW OF THE OPERATION OF GASTROTOMY FOR MYO-FIBROMATA OF THE UTERUS.
WITH COMPLETE STATISTICAL TABLES.

BY
HORATIO R. BIGELOW, M.D.,
Washington, D. C.

(Concluded from page 163.)

Remarks on French Table.—In the report of his case, February 28th, 1859, Dr. A. A. Boinet says that he has done two ovariectomies, finding fibrous tumors, which he removed. Lemarquay's case, of June 10th, 1868, does not specify definitely what was removed. The fatal result seems to be due to peritonitis rather than to hemorrhage. It is difficult to ascertain from the report of Koeberlé's case, of Dec. 5th, 1863, whether the uterus was removed or not. In the case of

¹ Bonacioli Enneas Muliebris, cap. viii.

March 7th, 1866, by the same operator, Caternault gives date as March 7th, age as twenty-nine, and the incision as being long. Koeberlé, in *Gazette Méd.*, Strasb., 1866, p. 95, gives date as March 6th, age as thirty-two, and says the tumor sprang from the fundus uteri. Larey says of Koeberlé's case, of Aug. 31st, 1868, that it is the only instance of this kind of tumor of the womb being recognized before operating. Of his case of Aug. 4th, 1870, Panas says: "*Tenait à la face postérieure de l'utérus par un large pédicule immobile;*" and lower down: "*Le corps fibreux était sessile . . . sans tendance à se pédiculiser.*" Péan says of his patient (Aug. 15th, 1871) that the menses have not reappeared, but she has, every month, weight in the loins, headache, and giddiness. The autopsy of Richet's case (1872) showed lymph adhesions of intestines, with purulent foci in lungs; there was an opening just above the symphysis, large enough to pass in the index finger, where the tube had been, and from it passed a walled canal to the stump. Hospital poisoning prevailed at the Hôtel Dieu at the time. In Villeneuve's first case, the nature of the tumor is doubtful; was it cystic? The rest of Péan's cases are to be found in Vol. IV. of the "*Clin. Chir.*," which, I regret, I have had no access to.

The following additional matter came under notice too late for incorporation in the regular sequence of cases:

J. C. Renton (*Glasgow Med. J.*, 1883, xx.) reports a case of successful myomotomy in which the pedicle was first treated with silk ligature. Hemorrhage supervened, the abdomen was re-opened and the pedicle secured with Koeberlé's *serre nœud*.

Dr. Frederic Henry Gerrish (*Boston M. and S. J.*, Sept. 28th, 1882), of Portland, Maine, gives the histories of two very interesting cases of gastro-hysterectomy with intra-peritoneal treatment of the pedicles. Recovery in both cases. These cases are of more than usual importance, because of the good results which followed the use of the simple ligature tied in halves and dropped.

Dr. Seth C. Gordon, of Maine, in 1871, reports a fatal case of gastro-hysterectomy.

Dr. Seth C. Gordon, of Maine, in 1883, reports another, with fatal result. Ligature to pedicle in both cases.

Dr. E. Miller, of Florence, S. C., reports a case with fatal

termination, Jan. 10th, 1876. It was a gastro-hysterectomy. Ligatures and stump returned to cavity.

Dr. P. Porter (*Med. Advance*, 1883, xiv.) reports a case of gastro-hysterectomy with recovery.

Dr. J. W. Smith, Charles City, Iowa. Gastro-hysterectomy, fatal result from septic infection.

Von Rzehaczek (*Wien Med. Wch.*, 1883, xxxiii.) details a case of myomotomy.

R. B. Duncan (*Austral. M. J.*, Melbourne, 1883) reports a case of myomotomy with recovery.

A Gillingham and W. Meredith ("Trans. Obs. Soc.," London, 1882) each report a case.

A. Alsberg (*Archiv f. Klin. Chirur.*, Berlin, 1882-3, xxviii.) reports operations for the removal of uterine myoma, with death from septic peritonitis.

V. Maggioli (*Spallanzani*, Modena, 1883, xii.) has a very readable article on the intra-peritoneal treatment of the pedicle.

In the *British Medical Journal* for Oct. 13th, 1883, there are two singularly complete articles, one by J. Knowsley Thornton, on the "Operative Treatment of Uterine Myofibromata," and the other by Carl Schroeder, of Berlin, on "Myomotomy."

Dr. Thornton writes: "I attribute my large mortality in hysterectomy, as compared with that of ovariectomy (under 4 per cent), and that of removal of the uterine appendages for fibro-myoma, hydrosalpinx, or pyosalpinx, ovarian pain, etc., as entirely due to the fact that, in the majority of the former operations, it is impossible to carry out strict antiseptic or aseptic surgery.

"In conclusion, I would strongly urge that hysterectomy should be reserved for those cases which have, before they are seen by the surgeon, passed beyond the reach of the safer operation of the removal of the uterine appendages; the size of the tumor being our guide. There will thus remain a legitimate, though limited, field for complete hysterectomy; but I am certain that its death-rate will exceed that of ovariectomy by as much as that of the operation for the removal of the appendages should fall below it. I am also prepared to maintain that, while strict Listerism will give us perfect results in the removal of the appendages, it is to the perfecting of our anti-

septic precautions that we must look for improved results in hysterectomy."

Dr. Schroeder sums up as follows: "As to the indications for the operation, it is not my object to discuss them here. I will only remark that they are of a different nature altogether from those leading to ovariectomy. According to my opinion, ovariectomy is always indicated, because ovarian tumors always threaten life through their tendency to grow incessantly, or on account of their very frequent malignant degeneration. Every ovarian tumor, with the exception of the retention-cysts of the Graafian follicles, ought therefore to be removed as soon as it has been diagnosed. The fibroids, on the other hand, often cease growing as age advances; besides, they never show malign degeneration. There must, therefore, always be in each case a certain urgent indication justifying their removal. Now, these indications are seldom absolutely determinate—such as incessant growth threatening the patient's life in a measurable space of time, or such as incarceration of the tumor in the pelvis. I have, for instance, once performed the operation successfully in the fourth month of pregnancy, on account of two large fibroids situated under the ovum, obstructing the cervix completely. Generally, the indication is individual, differing for each case. A patient, for instance, of the better classes, who does not need to work, and who can take care of herself, can well stand a menorrhagia now and then; whilst another patient, belonging to the working-class, may be brought by a similar menorrhagia into such a state that she can no longer earn her living. The other symptoms may also reach such a degree that it is impossible for the patient to work, and that she prefers an operation to being starved.

"This is the reason why it is so very difficult to find general decisive principles regulating the indications for this operation. It is, therefore, of the greatest importance to improve more and more the methods of operating. It may then be hoped that the prognosis will not be much inferior to that now arrived at in ovariectomy. A statistical table will, however, only be of value if the cases are all operated according to the same method.

"Of 66 patients on whom I have operated, I have lost 20 (30 per cent); of the last 40 cases, however, I have only lost 9

(22.5 per cent) ; and I ascribe this result to my having operated in these latter cases according to my method described above (India-rubber ligature, method of suturing, enucleation). Compared with ovariectomy of nowadays, these results are not very brilliant. It must, however, not be forgotten that the majority of ovarian tumors are simple cases, and the tumors easily removed ; whereas, there being no indication to remove the simple cases of fibroids, only the difficult ones are operated upon. Besides, patients with ovarian tumors generally come into our treatment in a state of general good health, whereas patients affected by fibroids are always anemic and worn away by the disorders which form the indication for the operation."

I have now under observation a very large sub-peritoneal myofibroma of the uterus, in a colored woman, forty-eight years old. It has been growing for over ten years, and has attained unusual proportions. Within the past year it has somewhat diminished in size, or, perhaps, it would be a more exact statement to say that it has not increased, under the use of an elastic bandage, Cutter's diet list, and the hydrochlorate of ammonia. Ascites has developed slightly, and as she is in excellent physical condition, an operation would probably be attended with a good result. The tumor is freely movable and, so far as one can judge from very careful examination, does not seem to be intimately adherent. A simple myomectomy will probably suffice. If such prove to be the actual condition of affairs, after opening the abdomen, my purpose is to try the plan advised by Marcy, under full antiseptic precautions (bichloride of mercury 1-1,000). The rubber sheet so protects the contents of the abdominal cavity that the spray can be used directly, if desired, without any danger from absorption, while the constriction of the rubber ring entirely controls hemorrhage. The operation under these conditions would be a simple one, and if the intra-peritoneal method be thoroughly *aseptic*, the danger of blood poisoning will be nil. If the uterus itself is to be amputated, Dr. Marcy believes that it is advisable to remove the *mucous membrane* of the stump, as there is always an apprehension of danger from septic changes in the cervical mucous membrane. The great reduction in temperature may be provided for by the use of Noeggerath's cushions or Marcy's hot-water rubber coil. Exclude *germs*, control *hemorrhage*,

Number.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision	Antiseptic anesthetics.	Character of tumor and complications.
1	Prof. Eug. Beckel.	Mar. 12th. 1874.	Com.	Ut. and ov	2 h.	23 cm.	Chloroform.	Fibro-m. in ant. wall of double horn ut.; fibromata of broad lig., w. 2700 gm.
2	A. A. Boi-net.	Feb. 28th. 1859.	do.	Tumor	Fib. tum. of ut., w. 15 kilo.
3	do.	Nov. 15th. 1863	do.	Sup. vag. uterus.	1 h.	10 to 12 c.	Chloroform.	Fib. tum. of fundus, w. 4 kilo., 250 gm.
4	do.	Mar., 1864	do.	Ut. and ov	Fibrous tum. of ut. .
5	L. Championnière	do.	Tumor	Tum. of ut., w. 2 kilo.
6	do.	do.	do.	do. w. 7 kilo.
7	Demarquay.	June 10th. 1868.	do.	do.	1 3/4 h	10 to 12 c.	Chloroform.	Fib.-my. with serous cysts from fundus, w. 1950 gm.
8	do.	1872.	do.	Su. va. ut. rud. ov. c.	15 m	12 to 15 c.	Fibrous tumor of ut. and ovarian cyst.
9	Duplay	do.	Tumor	Long.	Fibroid of ut., w. 4 kilo.; l. ov. in mass.
10	do.	do.	do. left ov & tube.	do.	Noan-tiseptic
11	Gayst	July 22d. 1863.	do.	Tumor	5 1/2 h	do.	Ether	Fib. tum. nodulated to right lat. part of ut., w. 1300 kilo.
12	E. Koeberlé.	Strassburg Mar. 14. 1863.	do.	do.	2 1/2 h	55 cm.	Chloroform.	Fib. ped. tum. of ut., w. 33 kilo.; adhesions everywhere.
13	do.	ibid., Apr. 20th, '63.	do.	Su. va. ut. ov., and Fall. t.	1 1/2 h	26 cm.	do.	Fib. tum. of womb, w. 7 kilo.; adhesions to omentum.
14	do.	Nov 21st. 1863.	do.	Vas. ped. tum. of ut.
15	do.	Dec. 5th. 1863.	do.	Tumor	16 cm.	Vas. fib. tum. of ut.; broad pedicle.
16	do.	Dec. 19th. 1863.	do.	Great p't of ut. & tumor.	25 cm.	Fibro-cyst of uterus, w. 4 1/2 kilo.
17	do.	Sept. 27th. 1864.	do.	Uterus	10 m	Long.	Chloroform.	Fibr. tum. of ut., w. 4 1/2 kilo.
18	do.	Mar. 7th (?) 1866.	do.	do.	1 h. 10 m	26 cm.	do.	Fibr. tum. of ut., w. 5 kilo.
19	do.	Apr. 23d. 1866.	do.	do.	3 h.	23 to 25 c.	do.	Fibr. tum. of fundus uteri, w. 3 kilo.; many adhesions.
20	do.	July 7th. 1866.	do.	do.	1 1/2 h	27 cm.	do.	Fibrous tum. of ut., w. 4 1/2 kilo.

N C E .

Operation.	Age. Married or single.	Condition of pat't.	Previous operati'n.	Result and cause of death.	Reporter and where reported.
Stump and lig. fixed in wound; serre nœud and wire ligature.	S.	Exhausti'n, death, 4th day.	Gaz. Med. Stras., June 1st, 1875; Pozzi, Hyst., Paris, 1875, p. 74.
... ..	47, M..	Mar a s- mus.	Death	Bull. Soc. Chir., ii., 1862, p. 687; Traité de l'ovariotomie, 1867, p. 174; Gaz. Heb'd. Med. and Chir., x., 1873, p. 462.
Silk lig.: lig. at lower angle.	43, M..	Bad	do. 5th day.	Boinet, Traité prat. des mal. des ov., 1867, p. 420.
....	Death ..	Pozzi, p. 45; cited by Koeberlé, Gaz. Med., Stras.
Ped. returned to ab.	do.	Champ., Soc. de Chir., Mai 9th, 1883; Gaz. Heb., xxx., 1883, p. 352.
Pedicle fixed outside.	Recovered..	ibid.
Cyst of ov. & part of omen, fast. in wound; ecraseur chain.	43, S....	Good...	May 20, '68; para-centesis	Peritonitis: death 36 h'rs after.	L'Union Méd., vi., 1868, pp. 429 and 458.
Serre n., ped. in lower an.	40	do.	Death 23 h'rs after.	Bull. Acad. Med., Paris, i., 1872, p. 1148.
Lig.	41	Anemia	...	Death 3d d.	Obs. J., London, viii., 1880, p. 56.
Ped. in lower angle.	26, S....	Recovery...	ibid.
Lig. in wound; 2 ecraseurs.	38, M..	Death, shock	Lyon Med., 1869, No. v.; Pozzi, Paris, 1875, p. 88.
Lig. and serre nœud.	24, S....	Tapped several times.	Exhaustion; death 3d day.	Koeberlé, Gaz. Med. Stras., 1864, p. 160, and "Gastrotomie," 1866, p. 2.
2 lig. to cervix: clamps to broad lig.; brought outside.	30, M..	Fair....	...	Recovery...	Koeberlé, Gaz. Med., Stras., 1863, p. 153, and "Gastrotomie," 1866, p. 47.
....	Death, peritonitis.	Boinet, p. 462; Caternault, p. 28; Pozzi, p. 45.
Serre nœud...	35, M..	Bad	Tapped 30 times	Death 18 h'rs	Caternault, p. 4; Koeberlé, Gaz. Med., Stras., 1865, p. 79.
do. and lig. both brought out.	36, S....	Good	Death.	Caternault, p. 12; Koeberlé, Gaz. Med., Stras., 1865, p. 165.
Wire & serre nœud.	39, S....	Ex s an- guinated.	do. 4 hours.	Caternault, p. 14; Boinet, p. 463.
Silk lig. to r. br. lig. and ser. nœud to neck of ut.	29, M..	Good...	Recovery ..	Caternault, p. 16; Gaz. Med., Stras., 1866, p. 95; Soc. Med., Stras., April 19th, 1866.
Act. caut. for hem'ge and wire lig.	35, M..	Feeble..	Syncope; d. 12 hours.	Caternault, p. 19.
Rem. ut. with act. cautery.	27, S....	Emacia- ted.	Recovery...	do. p. 21.

Number	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision.	Antiseptics.	Character of tumor and complications.
21	E. Koerberlé.	Aug. 31st, 1861.	Com.	Uterus...	2½ h	33 cm.	Fibro-plastic peri-ut. from fundus. w. 14½ kilo.
22	do.	do.	Sup. vag. ut. & ov	2½ h	27 cm.	Solid fibroid of ut., w. 8½ kilo.
23	Labbé....	April 3d, 1874.	Fibrous tumor of ut.
24	do.	Mar. 9th, 1875.	Com.	Sup. vag. ut.; both ovaries.	Long.	Fib. tu. of ut.; mult. cyst of l. ov., w. 35 to 40 lbs.
25	Lande ...	Lyons, Sep 5th, '76.	do.	Su. va. ut.	¼ h.	do	Chloroform.	Vas. fib. ant. wall, w. 6 kilo.
26	Ollier	Lyons, No. 15th, '74.	do.	do.	23 cm.	Fibro-cyst of uterus..
27	Panas ...	Paris, Aug 4th, '70.	do.	Tum. and r. ov.	3½ h	15 - 16 cm.	Fib. tum. ut.; cyst of ov., broad ped.
28	J. Péan...	Par., Sep. 22d, '69.	do.	Su. va. ut. and ov.	1½ h	Pubisto 8 cm. above umb.	Chloroform.	Cyst. tu. of ut.; fib. tu. r. ov.; cyst. tu. left tube.
29	do.	Par., Aug. 2d, '70.	do.	Great part of ut. & ov.	3 h.	Long.	Fib's & fib. cys. tu's of ut., one inters. and one ped.
30	do.	Par., Aug. 7th, '71.	do.	Tum (morcellem't).	3 h.	do.	Chloroform.	Fibro-cystic peri-ut. tum., w. 19 kilo; pedic.
31	do.	Par., Aug. 15th, '71.	do.	Su. va. ut. and ov.	1½ h	do.	do.	Fibro-myoma of ut., w. 4 kilo. 200 gm.
32	do.	Par., Oct. 26th, '71.	do.	do.	2 h.	19 cm.	do.	Fibrous tumor of ut., w. 4 kilo. 225 gm.
33	do.	Par., Nov. 6th, '71.	do.	do.	3½ h	Long.	do.	Fibro-cyst. tu. of ut., mass w. 10 kilo.
34	do.	Par., Dec. 6th, '71.	do.	do.	1½ h	do.	do.	Fibrous of base, w. 3 kilo. 860 gm.
35	do.	Par., Dec. 30th, '71.	do.	do.	3 h.	do.	do.	Fibrous (mult.) myo., 37 in number, w. 3 kilo. 780 gm.
36	do.	Par., Feb. 29th, '72.	do.	Fund. ut. & r. ov.	40m	do.	do.	Fibrous. tu. of fun., w. 3 kilo. 125 gm.
37	do.	Par., Dec. 6th, '72.	do.	Uterus...	6 h.	Large calcif. fib. tu., w. about 37 lbs.
38	do.	Par., Dec. 21st, '72.	do.	Most of ut. some omentum	2 h.	Long.	Myo-cystic multiloc. tumor of uterus.
39	do.	Par., Mar. 6th, '73.	Not c.	Part of sac	Fibro-cyst of uterus.
40	do.	Par., June 26th, '73.	do.	Ut., ovid., & ov.; lig. ov. left.	Fibrous tumor of ut.

Operation.	Age. Married or single.	Condition of pat't.	Previous operati'n.	Result and cause of Death.	Reporter and where reported.
Lig., caut. and serre nœud.	34, S....	Recovery...	Gaz. Hebdom. Med. Chir., vi., 1869, p. 135; Larrey, Bull. Acad. Med., Paris, xxxiv., 1869, p. 113; Demarquay, <i>ibid.</i> , i., 1872.
Lig., drainage tube lower angle.	38	Good...	do.	Koeberlé, Man. op. de l'ovariotomie, Paris, 1870, pp. 7, 10, 20, 23.
....	Death 32 h's after.	Pozzi, p. 45.
Stump d'wn outside, and cauterized with perc. of iron.	50, M...	Emaciated.	Death 15th day.	do. p. 64.
Ped. in lower an.; lig. thermo-cautery.	44, M...	Feeble.	Death 8 h'rs.	Gaz. Med. de Bordeaux, 1877, p. 299.
Cintrat's serre nœ., st. in w.	33, M...	Peritonitis, d. 3d day.	Pozzi, p. 80.
Lig. & clamp in wound.	M.	Recovery...	Panas, Soc. de Chir., June 14th, 1871; Gaz. des Hôp., 1871, p. 350; see "Remarks."
Clamp, caut., drainage.	41, S...	do.	Péan, L'Union Med., viii., 1869, p. 874; Bull. Acad. Med., xxxiv., '69, p. 1234; Péan & Urdy, Hysterotomie, 1873, p. 124.
Double lig.: ecraseur, caut.	45, S...	...	Tapped several times.	do.	Péan, Hysterotomie, pp. 17 and 135; Bull. des. Hôp., xlv., 1871, p. 550.
Rubber drain. tube introd. on 9th day.	37, M...	do.	Péan, Hysterotomie, p. 142; Bull. des Hôp., xlv., '71, pp. 550 and 578.
Ped. in lower an. of wound.	32, M...	Emaciated.	do.	Péan, Hysterotomie, p. 52.
do.	37, S....	do.	do.	do. do. p. 58.
do.	38, M...	do.	...	Death 37 h'rs after.	do. do. p. 152.
do.	46, M...	do.	Recovery...	do. do. p. 12.
do.	42, M...	do.	do. do. p. 66.
Double lig. in lower angle	39, S...	Emaciated.	Died 11th d.	do. do. p. 73. There was a ret.-ut. hematocele in this case.
....	Death 5th d.	Pozzi, p. 45; Clin. Hop. St. Louis, i., 1876, p. 675.
Tapped cyst: lig. & cut off; resect'd some omentum.	23½, M.	Recovery...	Péan, Hysterotomie, p. 96; Urdy, D'ovariot. et d'hysterotomies, Paris, thesis, 1874, p. 36.
...	36	do.	Pozzi, 45, Clin. Hop. St. Louis, i., 1876, p. 680.
Ped. in angle; morcellement.	40	do.	<i>ibid.</i> , p. 675.

Number.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision.	Antiseptics used.	Character of tumor and complications.
41	J. Péan...	Paris, Jul. 3d, '73.	Not c.	Ut., tubes, and ov.	Inters. fibroid ut., w. 2850 gm.
42	do.	Par., July 15th, '73.	do.	do	5 h.	Inters. fibroid ut., w. 5850 gm.; adhes.
43	do.	Par., July 31st, '73.	do.	Fund. ut., and l. ov.	Subperitoneal tumor.
44	do.	Par., Aug. 7th, '73.	do.	Tu., tubes, and ov.	Intersti. tumor, w. 13 kilo. 325 gr.
45	do.	Par., Sept. 23d, '73.	do.	Ut., tubes, and ov.	Interstitial tumor....
46	do.	Par., Dec. 11th, '73.	Com..	Sup. vag. ut. and tumor.	2½ h	Long.	Chlo-roform.	Fibro-cystic tumor, pedic.
47	do.	Par., Dec. 4th, '73.	do.	Tum., left tube.	Interstitial fibroid...
48	do.	Par., Jan. 27th, '74.	do.	Ut., tubes, & p't of br. lig.	Inters. fib., w. 4200 grammes.
49	do.	Par., Oct. 22d, '74.	do.	Tu., l. tube, & r. ov.	Interstitial fibroid...
50	do.	Par., Dec. 15th, '74.	do.	Fibro-cyst, w. 7300 grammes.
51	do.	Par., Jan. 12th, '75.	do.	Fibro-cyst.....
52	do.	Par., Apr. 27th, '75.	do.	Uterus....	Interstitial tumor....
53	do.	July 12th, 1875.	do.	Sup vag. uterus.	Inters. fib., three lobed, w. 3260 gr.: vascular shell.
54	do.	July 12th, 1875.	do.	Ut., l. ov., & tubes.	Suppu. fib. cyst. vas. adhes., w. 7 kilo.
55	do.	Aug. 17th, 1876.	Not c.	Part of sac	Long.	Cystic sessile tumor of uterus.
56	do.	Oct. 24th, 1876.	Com..	Fundus & ovaries.	Mult. fib. ut., w. 2000 gr.: inters.
57	do.	Nov. 9th, 1876.	do.	Tumor...	Long.	Fibro-cyst. tu. of ut.; umb. hernia, ascites, adhesions.
58	do.	Nov. 16th, 1876.	do.	⅔ of uterus	Fibroid, w. 4700 gr.
59	do.	Feb. 13th, 1877.	do.	Ut., ov., & tubes.	do. w. 1500 gr...
60	do.	Mar. 20th, 1877.	do.	Tu. & part of ut.	Long.	Inters. emb. plas. tu. of hypertrophied ut.; 2 large cysts.
61	do.	April 7th, 1877.	do.	Ut., ov., & tubes.	Fibroid, inters., w. 5500 gr.
62	do.	April 24th, 1877.	do.	Tumor....	Subperit. myoma, w. 3500 gr.; vas. ped.
63	do.	May 15th, 1877.	do.	Ov. & ut..	Fibroid, w. 3800 gr..
64	do.	July 17th, 1877.	do.	Tumor....	Subper. fib. left horn 4250 gr.; ped. ad.
65	do.	Nov. 5th, 1877.	do.	Tum. and ovaries.	Very l	Inters. fib. of fundus, cyst of ovary.
66	do.	Nov. 29th, 1877.	do.	Ut. and r. ovary.	Inters. fib., w. 6500 gr.; very vascular.

Operation.	Age. Married or single.	Condition of pat't.	Previous operati'n.	Result and cause of death.	Reporter and where reported.
Ped. in wound	43	Recovery...	Pozzi, 45, Clin. Hop. St. Louis, i., 1876, p. 675.
do. do., morcellem't	51	Death	ibid., p. 675.
....	41	Recovery...	ibid., p. 676.
....	48	do.	ibid., p. 676.
....	39	Death, perit.	ibid., p. 676.
Drain. tube in 50, M... wound and thro' post. c. de-sac to va.		Recovery...	Péan and Urdy, Hystérotomie, p. 55.
Stump in ang.	41	do.	Pozzi, 45, Clin. Hop. St. Louis, i., 1876, p. 676.
....	31	do.	ibid., p. 677.
....	35	do.	ibid., p. 677.
Stump in ang.	49	do.	ibid., p. 679.
do.	Death.....	ibid.
do.	33	do.	ibid., p. 671.
Morcellement, st. in wound	33	Death 3d d..	Clin. Chir., i., 1876, p. 677.
Morcellement, st. in angle	31	Tap. 5 w'ks before.	Death, perit.	ibid., p. 679.
Tapped.....	41	Recovery...	ibid., p. 812.
Stump in ang. 41, M... morcellem't.		Death 3d d., perit.	ibid., p. 808.
Tapped cyst.. 48, M...	Death, sept.	ibid., p. 811.
Stump in ang. 55, M... do.		Recovery...	ibid., p. 808.
do.	43	Anemic	do.	ibid., p. 109.
do.	53	do.	do.	Bull. Acad. Med., vi., 1877, p. 346; viii., 1879, p. 1195.
do.	48, M...	do.	Clin. Chir., i., 1876, p. 810.
do.	31	do.	ibid., p. 809.
do.	47, S...	Death, cyano. asphyxia, etc.	ibid., p. 800.
do.	54	Recovery...	ibid., p. 809.
Lig.....	42, S...	ibid., p. 810.
Stump in ang.	42	Death, perit.	ibid., p. 811.

Number.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision.	Antiseptic anesthetics.	Character of tumor and complications.
67	J. Péan ..	Dec. 12th, 1877.	Com..	Uterus....	Long.	Interstitial fibroid, w. 6000 gr.
68	do.	Feb. 12th, 1878.	do.	Ut. and ov....	do.	Multiple fibroid tumor of uterus, inters., w. 3200 gr.
69	do.	Feb. 19th, 1878.	do.	Tum. & ut....	69 cm.	Subperit. fibroma, w. 15½ kilo.
70	do.	Mar. 21st, 1878.	do.	Ut. & ov..	..	Long.	Fib. tumor uterus, w. 4525 gr.
71	do.	July 16th, 1878.	do.	do.	Fib. tumor uterus, w. 9 kilo., interstitial.
72	do.	Mar. 4th, 1879.	do.	do.	Fib. tumor uterus, w. 9500 gr.
73	do.	July 22d, 1879.	do.	Tumor ...	2 h. 15m	Fibro-cyst of uterus, thick pedicle.
74	do.	Apr. 20th, 1880.	do.	Ut. & ov....	Long.	Fibroid of uterus, w. 6 kilo.
75	do.	June 15th, 1880.	do.	Ut. & r. ov....	do.	Infil. fib. à géodes, w. 15 kilo.
76	do.	Nov. 25th, 1880.	do.	Uterus....	do.	Infil. fib., w. 6 kilo..
77	do.	Dec. 2d, 1880.	..	Fluid of cyst.	Short.	Interstitial cyst....
78	do.	Feb. 10th, 1881.	Tumor ...	3½ h	Long.	Fibro-cyst of uterus, w. 5 kilo.; vascular adhes.; pediculated
79	do.	Feb. 21st, 1881.	Uterus....	2½ h	do.	Interstitial sarcom. fib. of uterus, w. 6 kilo; ascites; adhesions.
80	do.	Feb. 24th, 1881.	do.	3 h.	do.	Interstitial sarcom. fib. of uterus, w. 6 kilo; ascites; adhesions.
81	Richet....	Hotel Dieu, 1872.	Com..	Su. va. ut.	1½ h	do.	Uterine fibroid
82	do.	May 31st, 1873.	do.	do.	1½ h	do.	Interstitial myo. of ant. wall of uterus.
83	do.	1874.	do.	Fibroid tumor of ut..
84	Terrillon .	La Salpêtrière, '82.	do.	Three-lobed tumor of uterus; very large.
85	Tillaux...	do.	Tumor	6 in ..	Lister	Subm. ut. fibroids, w. 2 kilo.
86	Villeneuve..	do.	Cystic tumor of ut...
87	do.	Com..	1 h., 35m	Carb. acid.	Fibro-myoma, w. 8 kilo. 600 gr.
88	1	do.	Fibroid of fun.; retroflexion uteri.

¹ Not completed because of failure to replace uterus.

Operation.	Age, Married or single.	Condition of pat't.	Previous operati'n.	Result and cause of death.	Reporter and where reported.
Stump in ang.	45	Recovery...	Clin. Chir., i., 1876, p. 811
do.	49, S...	Death 4th d.	ibid., p. 968.
Morcellement, stu. in ang.	48	Death 4th d. perit.	ibid., p. 969.
Stump in ang.	36	Death, perit.	ibid., p. 970.
Morcellement, stu. in ang.	46, S...	Death 3d d., perit.	ibid., p. 970.
Morcellement, stu. in ang.	35, S...	Recovery...	ibid., p. 971.
Stu. dropped.	39, S...	do.	ibid., p. 978.
Stump in an., divided in 3 parts.	do.	ibid., p. 973.
Stump & lig. in belly.	35	Death, exh..	ibid., p. 974.
Stump & lig. in belly.	36, M...	Recovery...	ibid., p. 975.
Tapped.....	27, M...	do.	ibid., p. 980.
Stump tied in 3 parts by 5 ligatures.	do.	ibid., p. 979.
Stump in ang.	do.	ibid., p. 975.
do.	32, M...	Death, perit.	ibid., p. 976.
Clamp. double drainage.	39, M...	Death 23d d.	Pozzi, p. 59; Progrès Méd., p. 2.
Ligature.....	26, M...	Death 50 h..	Pozzi, p. 61.
....	39	Death, septi.	do. p. 45.
Schröder's treat. of ped	44, M...	Recovered..	Terrillon, Soc. de Chir., Mai 9, 1883; Gaz. Heb. Med. et Chir., xxx., 1883, p. 352; Gaz. des Hôp., Mai 12, 1883, p. 429.
Stump in low angle.	35	do.	Obs. Jour., London, viii., 1880, p. 56.
....	do.	Gaz. Med., Paris, v., 1883, p. 128; reported by Ter- rier.
...	do.	ibid.
Interstitial oc- clusion.	Death.....	Pozzi, 12.

Number.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision.	Antiseptics and anesthetics.	Character of tumor and complications.
1	N. Eck...	Aug. 24th. 1874.	Com.	Ut., ov. & tubes.	2 h.	Long.	Myo-fibro-cyst, w. 3750 gr.; inters.
2	do.	do.	do.	Large myoma.....
3	B. G. Kleberg.	Odessa, July 8, '77	do.	Ut. & ov.	do.	Carb. spray.	Myoma, w. 10 lbs.; interstitial.
4	do.	Odessa, June 29.	do.	do.	do.	do.	Fibroid; interstitial.
5	do.	do.	Tu. & ov.	do.	do.	do.
6	Krasso w-sky.	St. Pet'bg May 27, '74	do.	Su. va. ut.	4 h.	28 cm.	Myo-fibro-cyst, w. 2400 gr.
7	do.	St. Pet'bg May 25, '76	do.	Su. va. ut. & r. ov.	4 h.	11 cm.	Fibro-myoma, weight 3838 gr.
8	do.	St. Pet'bg June 3, '76	do.	Ut., ov., & tubes.	2 h., 30m	do.	Fibrous and cystic tumors.
9	do.	St. Pet'bg Aug. 31st, 1866.	do.	Tu. cyst., both ov.	1½ h	20 cm.	Subperit. fib.; mult. cysts of ovaries.
10	do.	St. Pet'bg Oct. 16, '66	do.	2 fib. of ut. cyst & ov.	2½ h	12 cm.	Subperit. fib.; colloid cyst of ovary.
11	Reyher...	do.	Ut. & ov.	3 h.	Ant i-sepsis	Fibroids of uterus....

S C A N D I -

Number.	Name of operator.	Date and place of operation.	Completed or not.	What was removed.	Duration of oper.	Length of incision.	Antiseptics and anesthetics.	Character of tumor and complications.
1	O. Hjelt..	Com.	Uterus...	Myo-fibroma, w. 17 kilo.
2	L. Howitz	Denmark.	do.	Ut. & ov.
3	do.	Denmark, May 17th.	do.	Ut. & app.	20m	4 in.
4	F. Howitz	Copenh'n, Feb. 21, 1879.	do.	Su. va. ut.	Spray	Fibro-cystoma of ut.
5	Nicolays'n	Christiania, July 5th, '78.	do.	Ut., ov., & tubes.	do.	Myo-fibroma.....
6	Stadfeldt.	Denmark, Ap. 13, '74	do.	Uter. and append.	10 cm.	do.	Fib. ut., cyst of ov.
7	Vogt.....	do.	Tumor....	Fibro-cyst, w. 2050 gr.

S I A .

Operation.	Age. Married or single.	Condition of pat't.	Previous operati'n.	Result and cause of death.	Reporter and where reported.
Lig. cut short.	33	Feeble..	...	Recovery...	Wojenno Medizinsky Jour., St. Petersburg, cxxx., '77, Oct., p. 27; Cent. f. Gyn., ii., 1878, p. 286.
Drainage	19	do.	do.	Wojenno Medizinsky Jour., St. Petersburg, cxxx., '77, Nov., p. 85; Cent. f. Gyn., ii., 1878, p. 287.
Drain. tu. outside, stump dropped.	40	Good...	do.	St. Petersburg Med. Woch., ii., 1877, p. 333.
Stu. dropped..	28, S....	Anemic	do.	ibid., p. 333.
Lig. outside ..	35, M...	D'th, septic.	ibid., p. 373.
Stu. in wound	42, M...	Anemic	Tapped.	do.	Archiv f. Gynäk., ix., 1876, p. 414.
Drainage .. .	39, M...	Recovery...	Gaz. des Hôpitaux, xlix., 1876, pp. 900 and 915.
Stump in ang.: drainage.	38, M...	do.	ibid.
Stu. returned.	45, S....	Death.....	Krassowsky, "De l'Ovari- otomie," St. Petersburg, 1868, p. 41.
....	35, M...	do.	ibid., p. 49.
Freund's op. with Coht's mod.	40, M...	do.	St. Petersburg Med. Woch., iv., 1879, p. 200.

N A V I A .

Operation.	Age. Married or single.	Condition of pat't.	Previous operati'n.	Result and cause of death.	Reporter and where reported.
....	Finska läkarasällsk. handl., xxi., p. 304 (Helsingford).
Clamp.	53	Death	Pozzi, p. 82.
Wells' clamp..	62	Recovery...	ibid., p. 83.
Stu. in wound	41, M...	Death.....	Gynäk. og Obstet., Medde- lesen, ii., 1879, p. 280; Cent. f. Gynäk., iv., 1880, p. 69.
Péan	44	Recovery...	Norsk. Mag. f. Läge., ix., '79, p. 225; Cent. f. Gyn., iv., '80, p. 462; Lon. Med. Rec., viii., 1880, p. 458.
Stu. in wound	56, S....	Death.....	Pozzi, p. 83.
....	do.	Pozzi, p. 87; Norsk. Mag. f. Lägevid; bd. ii., 1872, p. 665.

provide against shock by preparatory treatment, and by regulating the temperature of the body during the operation, and the surgeon may have a reasonable confidence in the success of his operation.

[Dr. Thos. Keith (*British Med. Journ.*, Dec. 8th, 1883) has done 25 gastro-hysterectomies in the last ten years with only 2 deaths. They were all hospital cases. Seven of these were very difficult.]

PROCEEDINGS OF THE NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, December 6th, 1883.

FORDYCE BARKER, M.D., LL.D., *President, in the Chair.*

THE PREVENTION AND TREATMENT OF PUERPERAL FEVER.

DR. T. GAILLARD THOMAS read a paper on the above subject, and said that his motive in presenting it was involved in the fact that the plan of treatment of puerperal fever which he should advocate had not been anywhere fully elaborated, or in any single essay carefully systematized. Indeed, the general principles of the plan of treatment had not been universally accepted as orthodox; the question of intrauterine antiseptic injections for the cure of puerperal septicemia was the one which would come up for discussion. While the paper contained nothing which had not been elsewhere as fully and as clearly stated, and nothing which had not already received careful investigation at the hand of progressive obstetricians, the author hoped that, on account of his large experience, the presentation of his estimate of the method might prove of some value. At the annual meeting of the American Gynecological Society, held four years ago, the subject of intrauterine antiseptic injections for the cure of puerperal septicemia was discussed, and, with a single exception, he stood alone in its advocacy. Furthermore, he was among the very first, if not the first in this country who adopted this method, together with early cutaneous refrigeration, in the treatment of this disease. Among all the advancements which have been made in obstetrical medicine, he regarded none as more important or more signal than that relating to the prevention and cure of the febrile conditions incident to the puerperal state. Brief reference was then made to the views which have been entertained concerning its exact nature, such as a fever due to suppression of the lochia, inflammation of the uterus and peritoneum, specific puerperal fever, uterine wounds, arrest of the process of formation of milk, the doctrine of the multiplicity of puerperal affections grouped under one name, and the doctrine of puerperal blood-poisoning, etc.

Dr. Thomas then directed attention to the condition of the blood

in the puerperal woman, being favorable, first, to the development of thrombi, and, second, the existence of a tendency most prolific for sepsis and zymosis. Besides this, the nervous system is in a plus-state of excitement and sensitiveness. With this general condition, there exist several local conditions which result from parturition. Before enumerating these, he briefly alluded to the normal condition of the lymphatics, the blood-vessels, and the uterine tissue, and then described the appearances seen forty-eight hours after delivery. Outside, all looks very well. The uterus is simply much larger than the non-pregnant womb, but the inner surface has an unhealthy, diphtheritic look, although free from exudation: there is a large, raw, irregular placental site, and small clots can be seen which cover the mouths of the uterine sinuses. The odor from this surface is disagreeable. On examining the cervix uteri, there may be found two or three small lacerations, and, in consequence of these, with absorption, through them, of irritating lochial discharge, the cervix is swollen and edematous. Examining the vagina, it is found that, in two or three places, superficial rupture of the mucous membrane of this canal has been produced. The vulva also presents several solutions of continuity, the fourchette has been torn through, and this rent has extended perhaps through a small portion of the perineum, and one or two small fissures have occurred in the mucous membrane covering the *ostium vaginae*.

Material taken from these surfaces, at this stage of the puerperal state, and introduced by inoculation, will give rise to more or less irritation and lymphangitis, but it is not sufficiently poisonous to give rise to erysipelas or angioleucitis. And yet, what are the results of parturition? Recovery is the universal rule, unless some unusual occurrence manifests itself to prevent this consummation. Notwithstanding the existence of all these circumstances which are best calculated to insure a bad result, only one or two of hundred parturient women die when properly cared for, even in public hospitals.

But now and then all this is changed, and some poison gains access to the genital canal, and acts as rapidly and decidedly as a little yeast added to dough, and striking and alarming phenomena develop, spread, and progress steadily toward a fatal issue. At the morning visit, the physician leaves the patient with a normal pulse and temperature, and in an apparently favorable condition. A few hours only may elapse when the patient has scarcely a perceptible chill, with some pelvic pain, the lochia cease, the milk has disappeared, she suffers from severe headache, a look of indescribable anxiety is upon her face, she has a pulse of 120, and a temperature of perhaps 104° F. The poisonous element has reached the genital tract, and the result is already manifesting itself. Suppose now that the physician remains inactive, or relies upon medicines given by the mouth which are supposed to exercise some control over this poison, the local conditions, in the mean time,

which favor its admission remaining unchanged, the lymphatics are active. lymphangitis follows, cellulitis, peritonitis develops itself probably, and what was originally a septicemia will emerge into one of those affections which have been grouped under the general name puerperal fever, and pass through all its attendant phenomena.

PATHOLOGY OF PUERPERAL FEVER.

At this point, the author of the paper proceeded to the consideration of the pathology of puerperal fever, and defined it as a puerperal septicemia. It matters not whether it assumes a form of phlebitis, cellulitis, lymphangitis, or peritonitis, the essence of the disorder is absorption of poison into the blood of the parturient woman, through some solution of continuity in the tissues of the genital tract. As had been well described by Lusk, puerperal fever is "an infectious disease, due, as a rule, to the septic inoculation of the wounds which result from the separation of the decidua and the passage of the child through the genital canal." In other words, Dr. Thomas accepted the definition given by Hervier up as long ago as 1870, who defined the disease to be "a multiplicity of affections produced by puerperal poison."

NOMENCLATURE.

Concerning nomenclature, of late an effort had been made by the Dublin School of Midwifery to supplant the old term puerperal fever by the use of the word "*metria*." Dr. Thomas hoped the suggestion would not be adopted, as the word *metria* does not convey any more accurate idea of the pathology to the mind of the student than does the old term. Of the two terms, *metria* is more objectionable than puerperal fever. On the other hand, puerperal septicemia conveys a definite and clear idea, which appears to be in accord with the truth as taught us by modern pathology; it should be adopted, despite the fact that it is far from being absolutely perfect.

Dr. Barnes had proposed to use the word in its comprehensive sense, employing it to mean that "the blood of the puerpera is empoisoned," and Dr. Thomas was willing to admit this, although he believed in the existence of a specific poison which acted as surely as does a specific poison in the production of typhus or of small-pox. As to the exact nature of the poison, we are not yet able to say; nor are we able to say what the exact nature of the poison is, in most, if not all, the infectious diseases. German pathologists declare that the round micrococci, especially, are important factors in the etiology of this group of affections, but this point in bacterial pathology is too unsettled to permit of its introduction into such a paper as the present one. But even if we do not know what the nature of the poison is, we surely know that some such toxic agent exists, and it behooves us to inquire how it can be best destroyed, or how its life and activity can be best counteracted if it gains admission despite our care and watchfulness. There are

only two methods by which it can reach the parturient tract: first, it may be carried into the vulva or vagina through the atmosphere; and, second, it may be carried to any part of the genital tract by the fingers of the doctor or nurse, towels and cloths laid against the vulva, sponges, instruments, and by bed and body clothing coming in immediate contact with the genital organs. Dr. Thomas then addressed his remarks to cases chiefly occurring in private practice, because even among the wealthy there is a laxity of system and a carelessness with regard to preventive measures which borders closely upon criminality. It is the duty of every practitioner to guard his patient against puerperal septicemia by every means in his power.

PROPHYLACTIC MEASURES.

(1) In all midwifery cases, whether in hospital or private practice, the floor and the ceiling of the room in which the woman is to be confined, should be thoroughly washed with a ten-per-cent solution of carbolic acid, or a bichloride solution, one to one thousand. The bedstead and the mattress should be sponged with the same solution. All curtains and upholstering should be dispensed with.

(2) The nurses and physician should take care that all their clothing is free from exposure to the effluvia of septic infection, such as typhus, erysipelas, septicemia, scarlet fever, etc., and if there has been any exposure in this direction, all the clothing should be changed, and the body should be thoroughly sponged with a saturated solution of boracic acid.

(3) As labor sets in, the nurse should thoroughly wash her hands with soap and water, remove the dirt from under the nails, and administer an antiseptic vaginal injection, repeat it every four hours during labor, and keep a napkin wrung out of the same antiseptic solution over the genitals until the birth of the child.

(4) Both doctor and nurse should wash their hands thoroughly with soap and water, and scrape the nails, and afterwards soak their hands for several minutes in a solution of bichloride of mercury one to one thousand.

(5) The third stage of labor should be efficiently produced, all portions of placenta should be removed, and ergot administered in moderate doses three times a day, to be kept up for at least one week to secure complete expulsion of the clots and closure of the uterine vessels.

(6) The doctor should take nothing for granted, but at the conclusion of labor should carefully examine the vulva of the patient. If there is any rupture of the perineum, it should be closed at once by suture, and, if slight lacerations are found, they should be dried thoroughly with a cloth, and equal parts of a saturated solution of carbolic acid and per-sulphate of iron applied, and again the surface dried with a cloth, and painted over with gutta-percha collodion.

(7) Within six or eight hours after the termination of labor,

syringe out the vagina with an antiseptic solution, and introduce a suppository of cocoa butter containing from three to five grains of iodoform.

(8) The vaginal injections should be repeated every eight hours; but in all cases of difficult labor, and in those in which instruments have been employed, they should be administered twice as often, and kept up at least for ten days. The nurse must wash and disinfect her hands before *every* approach to the genital tract of the woman.

(9) Employ a new gum elastic catheter, which has been thoroughly immersed in an antiseptic fluid each time the bladder is evacuated, rather than trust the nurse to cleanse an old silver catheter.

(10) The physician must inform himself *by personal observation* as to the competency of the nurse with regard to the use of the catheter, the administration of the vaginal injections, and the introduction of the suppositories.

It might be objected that so many details with reference to the lying-in woman are unnecessary, because of the enormous preponderance of cases in which complete recovery ensues without such treatment, and that to introduce them savors of the performance of some grave surgical operation. So much the more did Dr. Thomas urge them on this very ground, because he believed that the woman who is to bring forth should be treated as though she is to go through a capital operation.

At this point, Dr. Thomas made a strong protest against the use of intrauterine injections as a prophylactic measure, except after very severe operations in the uterine cavity, which rendered the occurrence of septicemia almost certain.

But suppose that, despite all these precautions, the poison has entered, what are the most reliable means for checking the advance of the septic disorder? He did not believe there is any specific disease germs which gives rise to puerperal septicemia. It is probably the same cause which gives rise to septicemia in the stump after an amputation, or after a wound with a compound fracture, or in the lacerated tract after gunshot wounds.

A portion of retained placenta or membranes does not give rise to true puerperal septicemia, but rather to a toxemia. If the mere presence of decaying material would produce septicemia without the agency of a specific disease germ, the disease would develop in healthy country localities.

As soon as the patient is stricken by the poison, certain morbid phenomena develop themselves, such as chill, high temperature, pelvic pain, mental disturbance, headache, pain in the back, and sometimes, though not commonly, nausea and vomiting.

TREATMENT FOR THE CURE.

First. As soon as the diagnosis is made, all pain and nervous perturbation should be allayed by a hypodermic injection of mor-

phine, unless there is some special idiosyncrasy in regard to opium, and throughout the attack, whether suffering in mind or body, the hypodermic use of morphine should be repeated sufficiently often to allay it. In this particular case, the drug should be used hypodermically, and special care should be taken to use a clean syringe, dipping the needle before its use into a solution of bichloride of mercury, one to one thousand, which will prevent the formation of abscess.

Second. Being relieved of pain, spread an India-rubber cloth over the edge of the bed, making it fall into a tub of water rendered antiseptic by the use of carbolic acid, two and one-half per cent, or bichloride, one to two thousand. Then move the patient very gently across the bed, place a pillow under the head, allow each foot to rest upon the side of the tub, and cover with blankets. Then introduce either a Chamberlain's glass tube or Lyman's metallic tube, very carefully guided by the index finger, passing it up to the very fundus of the uterus, attach a Davidson syringe, and throw a stream of water with gentle force against the lining membrane of the organ. If there is any suspicion that there remain attached portions of placenta or membranes, they should be carefully removed, using the finger nail as a curette, as advised by Dr. Wilson, of Baltimore.

DANGERS OF INTRAUTERINE INJECTIONS.

There are dangers attending the administration of these injections. First, the entrance of air into the uterine sinuses; second, the production of hemorrhage; third, the danger of forcing fluid directly into the general circulation through the injection tube introduced into the mouth of one of the sinuses; fourth, convulsions and violent pain which produce a sudden and baneful influence upon the nervous system; and, fifth, the passage of fluid into the peritoneal cavity through a Fallopian tube. All of these may, to a very great extent, be avoided by careful attention to details, as by the use of a large tube, with water not less than 100° F., and using only a moderate degree of force, proceeding gently, cautiously, and slowly. The tube should not be allowed to fill the os internum or externum completely. If, after the use of the injections, it is found that the cervical canal hugs the tube too closely, it should be dilated before further injections are practised, and this can be done by the use of the hard-rubber or Barnes' dilators. If hemorrhage occurs, persulphate of iron should be added to the antiseptic solution, and ergot administered.

The frequency of the administration of the intrauterine injection should be varied greatly with individual cases. In moderate cases, where the temperature falls readily, only once in five hours may be all that will be required, while in other cases they may be required every three hours, and in bad cases they may be administered as often as every hour. These injections should be administered by the physician always, and should be carried up to the fundus uteri, and every precaution exercised concerning detail.

Dr. Thomas favored the intermittent stream. For a number of years, he entertained the idea that the continuous flow was most desirable, but on that point he had changed his opinion entirely. Continuous irrigation he regarded as a delusion and a snare. For vaginal irrigation it is an excellent method. Nevertheless, in severe cases he preferred to employ continuous irrigation and use the intermittent stream every three hours rather than exhaust the patient by the use of injections as often as seemed desirable. At all events, that plan is best which best cleanses the parturient canal.

Third. Control the temperature by the use of Townsend's rubber-tube coil placed over the entire abdomen from the ensiform cartilage to the symphysis pubis, with ice-water flowing through it. In his service at the Woman's Hospital, this means of controlling the temperature is as commonly and freely used as are gargles for diseases of the throat, and thus far no ill-effects had been produced, either in the way of chilliness or by the development of complicating diseases, such as pneumonia, pleurisy, etc. Formerly he relied upon Kibbee's cot for the same purpose, but had found the coil much more convenient.

Fourth. Keep the nervous system under the influence of quinine, fifteen grains night and morning, or Warburg's tincture administered in capsule according to the recommendation of Dr. John T. Metcalf, or by the use of salicylate of soda.

Fifth. The diet should consist of fluid food, and the staple article should be milk, but animal broths may be alternated with it.

Sixth. Efficient and abundant assistants. Two nurses are necessary, one for night and the other for day, and at least one extra physician as an assistant, in order to carry out this method of treatment effectually.

In regard to the antiseptic substances used, they have been thymol, boracic acid, salicylic acid, carbolic acid, and bichloride of mercury. The last two are the best, and the bichloride seems to be about to supersede the carbolic acid, and for intrauterine injections it should be used in the strength of one to two thousand.

DISCUSSION.

DR. W. M. POLK believed he echoed the voice of every Fellow when he said that we are deeply indebted to Dr. Thomas for the able and even eloquent presentation of this most important subject, and most of his views were too well recognized and too generally accepted to require more than commendation on his part. Dr. Thomas had entered upon the subject so thoroughly that there was but little left to say, especially with reference to treatment. With regard, however, to the question of pathology, Dr. Polk thought that perhaps it would have been better, in view of the stress which the author of the paper laid upon antiseptic treatment, if he had planted himself squarely upon that pathological view which regards puerperal fever as identical with septicemia and pyemia. So long as there is any question on this part of the subject it seemed to him that some doubt would still exist in the minds of many concerning the propriety of most of the remedial measures

advocated by Dr. Thomas. So far as he was concerned, Dr. Polk was free to say that he considered the disease as identical with surgical septicemia and pyemia, and that the variations which exist in the clinical history or in the pathological lesions are simply those which are peculiar to the organs invaded. With regard to the view that there must be a special puerperal fever poison or a multiplicity of poisons upon which the disease depends, Dr. Polk thought, if it were taken into consideration that a woman recently delivered had within her pelvis a large amount of rapidly retrograding material; that a large number of uterine and vaginal arteries, veins, and lymphatics were dilated and in places collapsed, giving areas of sluggish circulation; that the blood of such people, besides being deficient in red corpuscles (the oxidizing element), and containing an excess of fibrin, was so filled with waste material that its vital and chemical forces were often taxed to the utmost; then one could readily understand how an amount of septic poison, which in any other condition would be inoperative, would here be all-sufficient.

As to the question of treatment, standing, as he did, upon this basis of septicemia and pyemia, it seemed to him the principles of Listerism as laid down by Mr. Lister and his followers, and advocated by Dr. Thomas, entirely covered the ground. With regard to precautionary measures, there is absolutely nothing to be added. He would simply suggest, especially with reference to this city, that the question of purity of air is one which should be especially inquired into. Concerning measures of actual treatment, he had but a single suggestion to make, and it was this, that, according to his experience, intrauterine injections had oftentimes been used when in reality the sole lesion which was killing the patient existed in the cervix. The stream of water, as it ordinarily flows from the interior of the uterus is, in many instances, of insufficient weight to separate the closely applied surfaces which are frequently in a sloughing condition. His rule, then, had been to thoroughly inspect by means of Sims' speculum the entire vaginal canal as far as the external os, and then draw down the cervix with the tenaculum and separate its edges, and if he found that there is anything demanding attention he applied directly to the spot a strong solution of carbolic acid, or some other similar strong disinfectant. In fact, in many cases it had been the only treatment which he had found necessary.

With regard to the interior of the uterus, if he did not find the vagina or cervix in conditions which seemed to be responsible for the disturbance, it had been a practice advocated to introduce borated cotton upon an ordinary applicator, wet in a strong solution of carbolic acid, two per cent, into the interior of the uterus, and thoroughly wipe it out. He had introduced the broad curette (Skene's), when he suspected remaining portions of placenta or membranes, and removed whatever material might be found there. Of course, his remarks were not at all at variance with the general principles advocated, but were simply items of personal experience concerning the details in carrying them out, but he believed that if we are to adopt the measures which have been so properly advocated by Dr. Thomas, we must go a little farther than the author of the paper had done, and plant ourselves upon the ground that we have to do with a condition of things which differs in no essential respect from surgical septicemia and pyemia.

DR. JAMES B. HUNTER said that while Dr. Thomas had left but

little to say, there were two or three points which he thought worthy of attention concerning the details of carrying out the method of treatment, and on which he should differ somewhat from the author of the paper. He had firm convictions with regard to the propriety of washing out the uterus, and was well aware of the great difficulty of accomplishing it thoroughly. He did not believe that any nurse should be trusted to administer the intrauterine injections.

In the next place, it was important to disturb the patient as little as possible, and he thought this could be accomplished without moving the patient at all, if a bed-pan of large size is used, and the patient permitted to lie upon her back. He did not approve of the use of the Chamberlain tube of the size which had been exhibited, but preferred one about half the calibre and half the length, and he had found it necessary to have the openings confined chiefly to the end of the tube. He had found that, after a few injections had been administered, the uterus contracts, so that it is difficult to pass a tube of large size into it. He then resorted to a smaller tube, and liked the silver tube of Dr. Lyman, or, what he had frequently used with more satisfaction, was Knott's double catheter. He had in some cases had occasion to dilate the canal before using the tube.

A point of special importance is regularity in carrying out this plan of treatment. Thoroughness and gentleness, without disturbance to the patient.

With regard to the effect produced by the injections, he had found very commonly fear of shock and other bad results, but he did not think these had at all been realized. Where the water had been used sufficiently warm, that is, 100° F. to 110° F., the danger of chill was reduced to a minimum.

Again, when the water returns clear, one is very apt to think he has made a mistake, but according to Dr. Hunter's experience, even though the water does come away clear, the temperature falls, showing the beneficial effects of the treatment. He had resorted to this method as late as the seventh week after parturition.

With regard to the use of the coil, he did not regard it as proper in simple puerperal septicemia. In puerperal peritonitis he had the strongest possible confidence in it. He preferred the metal or Leiter's coil to the rubber coil, because he regarded it as more manageable and as more easily kept clean.

DR. E. L. PARTRIDGE spoke with reference to a few practical points in the method of treatment. Concerning the pathology of the disease, he thought there could not be any essential difference in the minds of those who had had experience in the management of obstetrical cases.

With regard to prophylactic treatment, he would impress the importance of personal cleanliness on the part of the medical attendant, as strongly as Dr. Polk had spoken of the importance of the purity of the atmosphere. It is absolutely essential that the medical attendant observe the utmost strictness with regard to cleanliness, keeping the nails clean, etc., and as a prophylactic measure.

With regard to the management of the case after the disease is established, he was in favor of the use of intrauterine injections, and would resort to them with the same frequency recommended. Concerning the agent employed, he had only to add that about one year ago the late Dr. Beverley Livingston introduced into the practice at the Nursery and Child's Hospital what Dr. Partridge be-

lieved to be a most excellent supplement to the intrauterine treatment, namely, after cleansing the intrauterine cavity, he used a mixture of iodoform and glycerin, in the proportion of one to ten, and injected one, two, or three ounces into the uterine cavity after finishing with the uterine douche. Dr. Partridge regarded it as an excellent adjuvant, both in hospital and in private practice, and thought it possible that a certain amount of intrauterine injection might be dispensed with by adopting this measure. With reference to the bichloride solution, he could declare himself in favor of its use. With regard to the coil, he believed in it for the reduction of temperature, and thought it the most efficient agent we have in our possession. He also thought we should not look for immediate and marked results from the plan of treatment, as Dr. Thomas had already stated. Again, because there is no peritonitis, is not a good reason why the agent should not be used. There is no area of the body where applications give the same results as when made over the abdomen. He favored, contrary to the opinion of Dr. Hunter, the rubber rather than the metal coil, and for precisely the same reason given by Dr. Hunter, namely, that it is more easily kept clean. In hospital practice, he had not found any objections to its use arising from the patient *herself*, as to the liability to produce chilliness, etc., and he doubted if it produced any more discomfort than did the ordinary rectal injection administered for the first time.

THE PRESIDENT said the subject was a most important one, and, owing to the lateness of the hour, several gentlemen of large experience and competent to discuss the question were unable to have an opportunity to speak, and he, therefore, would suggest a proposition. Several years ago, he himself advocated views with which not a single one present was in accord, and he had the pleasure and honor of presenting them to the London Obstetrical Society. But he thought that, at the present time, a very large number or prominent obstetricians in different parts of the world were in full accord with what he at that time expressed. The author of the paper of the evening did not consider it worth while to make allusion to these views, and inasmuch as he thought that opportunity should be given to have all views upon this important subject brought forward and receive full discussion, he would propose that the subject of the paper be made the topic of discussion at some future meeting of the Academy. With regard to the pathology of the affection, he should hold views which were at variance with those expressed by Dr. Thomas. He should accept in every particular the views expressed by the author of the paper so far as the description of septicemia is concerned as it exists in the puerperal state, but he should differ with those stated concerning the sum total of the affection: that is, the various local inflammations which occur in the puerperal condition. So, also, with regard to several points in the treatment of these different conditions. He therefore suggested that the evening of the first meeting of the Academy in February be devoted to the discussion of this subject, and, if it would not be regarded as improper, he would ask the Academy that he be permitted to open the discussion.

On motion, the suggestion of the President was adopted.

Meeting, February 7th, 1884.

DR. FORDYCE BARKER opened the discussion by reading a paper, of which the following is an abstract:

During the five years which he had had the honor to occupy this Chair, he had never before seen a meeting of this Academy so enthralled by the charm of elocution, the fascination of rhetoric, the glow of conviction, and the air of one who speaks by authority—an air which can never carry weight unless it has been before fairly and justly earned by good work—as on the evening of December 6th, when the paper was read on “The Prevention and Treatment of Puerperal Fever.”

Its authoritative tone, its earnestness and sincerity, its coloring of being based on experience and observation, instead of being unconsciously deduced from preconceived theory, gave the paper such a plausible air of scientific truth as must secure its acceptance without question by many minds whose belief rests on authority without examination of the data or analysis of argument. The more eminent the author of errors which may dangerously influence medical practice in matters of such vital importance as the saving of life of those who have just become mothers, the more striking the literary excellence, and the more admirable the artistic merits of a paper promulgating such errors, the more necessary it was that such errors should be boldly met and promptly refuted. Any paper read before this Academy by one to whom all conceded a place among the most eminent in the department of the profession to which his life had been devoted, if allowed to pass without examination and discussion, would be accepted by great numbers in all parts of the country as a statement of the science and medical practice as enunciated by the most prominent men of the period.

All would agree that the paper was remarkable for its originality, in that some of its pathological doctrines and the practice inculcated for the prevention and treatment of puerperal fever had never been taught in any work on obstetrics, or by any writer of acknowledged repute. If they were accepted by the common intelligence of the profession, they would assuredly be found in the obstetrical works of the future.

As there were many others who would take part in this discussion, he would, in the most concise language consistent with clearness of statement, give his reasons for thinking that the whole tone and coloring of the paper are misleading and dangerous, because it is supersaturated with septic infection. He would not “speak disrespectfully” of puerperal septicemia. He believed it to be one of the most dangerous incidents which may occur to women after child-birth, and he trusted that it would not be regarded as indelicate if he alluded to the fact that in a work on puerperal diseases, published more than ten years ago, a lecture devoted to the consideration of this subject in all its relations fills thirty-seven pages. He had yet found no reason to make any essential change of the views expressed in that lecture.

In the paper under discussion, the author distinctly avows his belief, without any qualification, that “puerperal fever is puerperal septicemia,” and that “it matters not whether it assume the form of metritis, phlebitis, cellulitis, peritonitis, or lymphangitis, the essence of the disorder is a poison which is absorbed into the blood of the parturient woman through some solution of continuity.” Not only the sentence quoted, but the whole tenor of the paper must convey to the unbiased mind that it is the well-defined opinion of the author that metritis, phlebitis, peritonitis, and cellulitis are never seen in the puerperal woman except as the result of an initial lesion, which permits the absorption of a specific

poison through the parturient canal, either from the atmosphere or from direct infection by doctors or nurses, from neglect or carelessness, or other agents brought in contact with the sexual organs.

The tendency to this pathological view has been rapidly growing within the past few years, as a result of the enthusiastic interest excited chiefly by the important investigations of our German co-workers, who have so zealously studied the character and effects of the micro-organisms in puerperal women *in hospitals*. No one, as yet, has maintained that the process of parturition and the puerperal state exempt a woman from those causes which induce local inflammation in the non-puerperal, or will deny that the process of parturition, and other attendant conditions besides the absorption of septic poison, may be the efficient cause of local inflammation; and he here stated his conviction that in private practice, when there is no epidemic influence, twenty cases of local inflammation due to such causes will be met with, where one will be found due to septic absorption.

Dr. Barker was in entire accord with the author in his preliminary remarks as to the peculiarities in the system of puerperal women, and had expressed these views in the work alluded to. He supposed that all educated men now know that the blood of a pregnant woman is in a state of hyperinosis, and that, as a rule, "her nervous system is in a plus state of sensitiveness and excitability, and influences which are very controllable in the non-puerperal state produce very evil results here." But it is very evident that in certain points our opinions are wide apart. Dr. Thomas regards certain conditions, which always are found following normal labor and always occur in normal puerperal convalescence, as pathological, but which Dr. Barker believed to be purely physiological. The ancients believed in the poisonous nature of the menstrual fluid.

Dr. Barker had supposed this superstition to be extinct, until informed by a letter from his friend, Dr. Weir Mitchell, that he knew "old men who would not permit a woman to enter their wine-room, for fear that, if menstruating, it would injure their wines." He also informed him that "in Roquefort women are not allowed in their cheese cellars." Dr. Barker supposed the theory must be that menstrual bacteria will destroy the bouquet of the Roquefort cheese.

But on the evening of December 6th, 1883, in this Academy of Medicine, he first heard the full evolution of this doctrine clearly enunciated. The lochia are described as an offensive fluid, made up of dead and decaying animal tissues, which poisons freshly-made, unprotected wounds, and here Dr. Barker quoted:

"In every case of child-bearing, the endometrium is thus encumbered, and freed by a process of exfoliation and sloughing: in every case, the cervix, vaginal mucous membrane, perineum, and vulva are, in varying degrees, lacerated; and in every case, the offensive fluid, called lochia, poisons these freshly-made, unprotected wounds."

Again the writer says: "Here we have a number of recent wounds constantly and unavoidably bathed with a fluid made up of dead and decaying tissue, animal tissue in a woman whose blood and nerve states are, with reference to septic disease, like flax prepared for the spark, and who is exhausted by pain, anxiety, loss of blood, and deprivation of sleep."

Other quotations might be given of a similar tenor, and the pro-

phylactic measures, which he asserts "should be adopted in all midwifery cases, whether they occur in hospitals or in private practice," are based mainly on this theory.

Can it be true that the process necessary for the birth of the human race is always attended with the development of a deadly poison whose malignant effects must inevitably prevent the spontaneous and kindly healing of such little traumatisms as always result from the process, and that, therefore, it is the duty of the accoucheur to take preventive measures of the character proposed? Does every parturient woman, in performing the function of maternity, like the scorpion, that carries in its tail an agent for suicide, if death is threatened by fire, physiologically generate an equally fatal poison in a corresponding locality, which the obstetrician must guard against by means that are most inconvenient, alarming, and not altogether free from danger?

He did not intend now to examine the question, which he had before discussed very thoroughly, and his views had long been published, whether there be not a distinct disease, most appropriately denominated puerperal fever, when, if there be any septicemia, it must be a consequence of a primary disease, and not a cause. Nearly a hundred years ago, the eminent obstetrician of London, who succeeded Denman, Dr. John Clarke, wrote as follows in regard to puerperal fever: "Unfortunately, the uniformity of the disease was assumed, and each author erected his own experience into a standard by which to judge of the descriptions and the practice of others."

All we know of any disease is derived from the study of its etiology, its clinical phenomena, and its anatomical lesions. The epidemic disease to which he had just referred differs in all characteristic points from what is known as septicemia. It differs in its origin, its modes of attack, its symptoms, and its anatomical lesions. The symptoms are frequently manifested a day or two before or even during labor, even when the child is subsequently born alive. In septicemia, the symptoms are never observed before or during labor, except when the fetus is putrid. The former disease, puerperal fever, originates from epidemic causes, and from contagion and infection; the latter, from nosocomial malaria, from autogenetic infection, and from direct inoculation. Can a woman after childbirth be exposed to the danger of receiving the poison which produces septicemia in larger doses than when she has retained in her uterus a portion of putrid, decomposed placenta? Yet he did not believe there was a single person who had had considerable obstetric practice for twenty years who had not had more than once to remove portions of putrid placenta which had been retained for days, and the patient had had no disturbance of such severity that he would call it puerperal fever.

Before leaving this part of the subject, he briefly alluded to one other point, which strikingly illustrated the difference between puerperal fever and septicemia. He thought there could be no doubt that the majority of the profession believed that all those cases of nosocomial malaria, such as aggregation, bad ventilation, contact with septic material, etc., which have a tendency to induce septicemia in surgical cases, have an equal tendency to develop the disease known as puerperal fever in women recently confined. But this does not prove that the diseases are identical, for there is abundant evidence that, while these causes are always requisite for the development of surgical septicemia, puerperal fever may be a very epidemic when these causes are wholly wanting.

In the early months of 1873, puerperal fever prevailed in the best parts of the city, and in that class of society possessed of abundant means and living under as good sanitary conditions as are possible in any large city, to a degree and extent here unknown for the previous twenty-five years. The deaths from this disease in the hospitals and in the wards of the city where the poor are aggregated were much less than in many former years. In five of the best wards of the city, in which are the residences of a great proportion of those of wealth, and few of the class of dwellings known as tenement-houses, with a population of 307,046, there were 80 deaths from puerperal fever, while in the remaining wards of the city, with a population of 605,245, there were but 63 deaths. In other words, he might say that during this period, in those wards of the city where the causes of septicemia must have existed in the greatest abundance, the mortality was nearly one-third less from puerperal fever, in proportion to the population, than in the best parts of the city, where these causes of septicemia could have existed only in a very limited degree.

From all these considerations, he thought that, if all the knowledge of this disease be derived from authors who have studied it in hospitals exclusively, it will be limited and one-sided, and the deductions, both as to its pathology and treatment, must, in many instances, be erroneous and unsafe. Without further details, he would content himself by expressing an opinion which will surprise many who have been carried along by the popular wave of the septic theory, as the initial cause of most of the puerpeal diseases. His conviction was strong, based partly on individual experience, but chiefly on a careful study of the clinical midwifery reports of private practice and all the literature of the subject in his possession—and this was very full as regards the English and French languages—that outside of hospitals less than two per cent of the puerperal diseases, and not half of one per cent of the deaths after childbirth were due to septicemia. There are no statistics of private practice which demonstrate the error of this opinion. The belief of the septicemists that terrible dangers threaten every woman in childbirth is based wholly on theory. In support of the opinion just expressed, he did not wish to summon any higher authority, even if it were possible to do so, than the writer of the paper now under discussion, and from which he made the following quotations:

"And yet, what are the usual results? Recovery, uniformly, I might say universally, unless some unusual occurrence manifests itself to prevent this happy consummation. Theorizing about the matter, one would suppose that the mortality resulting from such a state of things must be excessive."

"And yet the facts are these: Only about one or two in every one hundred parturient women ordinarily die, when properly cared for during labor, even in public hospitals."

With reference to "the prophylactic measures which should be adopted in all midwifery cases, whether they occur in hospital or in private practice," as the author of the paper distinctly avows: "If she who is about to bring forth" must "be treated as one about to go through the perils of a capital operation;" if all those preparations so definitely enumerated which gynecological surgeons insist upon, previous to an ovariectomy or a laparotomy, are necessary in ordinary labors; if the danger from child-bearing be so great that a wise and prudent obstetrician is justified in subjecting his patient

to the hazardous depression of intense anxiety and fearful doubt as to results, and in surrounding her with the vivid apprehension of her family, instead of stimulating and cheering her with the great happiness of maternity and the hope of increased interest and love from her husband; if all, or even a considerable part of the details mentioned are necessary "to save thousands of lives which are now lost," and to spare "thousands of desolate households the sorrow of losing their female heads"—then it seemed to him evident that the State should make child-bearing a penal offense for all those families who do not have a sufficient annual income to make it possible to carry out all these requirements. Such a law could only be made effective by adopting the facetious suggestion which appeared in *The Medical Record* of January 19th, 1884, over the signature of Seth Hill, Stepney, Conn.

The description given of puerperal fever, true as it may be, in its outlines, of the septicemia which gynecological surgeons are so often forced to encounter, he thought would strike obstetricians familiar with the disease in the lying-in chamber as the ideal picture of a poet, differing as much from the scientific description of trained clinical observation as the pictures of natural scenery by a Byron or a George Sand would differ from a scientific description of a mountain or a lake by Humboldt.

As to diagnosis, he could not regard the symptoms mentioned, even in their totality, as pathognomonic of septicemia, as all of them are to be found in other puerperal affections, when there is no evidence of septic absorption, unless, with the author, it be assumed that all puerperal disturbances are due to this cause alone.

Dr. Barker then expressed his convictions, by giving a few general propositions.

In puerperal fever, as met in private practice, we have to treat the *consequences* of some form of blood-poisoning. This may or may not be septic poisoning. In private practice, he thought it generally due to some occult, possibly atmospheric, epidemic influence; in hospital patients, nosocomial malaria, often associated with septic poisoning.

No treatment which interrupts the normal physiological processes—such as the retrograde metamorphosis of involution, the fatty transformation of the component fibres of the uterus, or the cicatrization of its internal surface by the exudation of organizable lymph, and the development of a new layer of mucous membrane, or the healing of traumatic lesions—can be justified, unless positive symptoms, now well understood in science, demonstrate their necessity.

Antiseptic injections, both vaginal and intrauterine, are of great service when the indications for their use are clearly shown by local signs of general symptoms; but they cannot be recommended with safety as a routine practice on theoretical grounds, as, for obvious reasons, they may be most detrimental in retarding the cicatrization of lesions and the other processes of normal convalescence, and are otherwise sometimes dangerous.

With reference to refrigeration as a means of reducing fever in puerperal diseases, he had no question that it may be useful in some cases, but his own experience in this method of treatment has not been favorable. Many years ago, he tried it in several cases in Bellevue Hospital, but he soon gave it up, as the results were less satisfactory than where other plans of treatment were pursued. Cold will effectively and usefully reduce the temperature in active inflammations and acute fevers, but in adynamic

diseases and in hectic fever this must be attended with a rapid waste of tissue more dangerous than the pyrexia.

Dr. Barker then asked permission to refer to a matter outside of the question of the prevention and treatment of puerperal fever, but in behalf of the "truth of history." He asked any who might have felt sufficient interest to turn to the 320th page of his work on "Puerperal Diseases," where they would find on that and the following pages the subject of intrauterine injections fully discussed. Instruments for their administration, which had been devised more than fifteen years ago, were shown to the class, and explicit directions were given as to the methods and indications for these injections, differing in no essential from those heard in this hall on the 6th of December. The lecture was delivered in the amphitheatre of Bellevue Hospital, in February, 1869, and the work in which it is printed was published in January, 1874. Then it may interest some to look at page 85 of volume iv. of the "Transactions of the American Gynecological Society," and read the papers by Dr. Edward W. Jenks, of Chicago, and Dr. James R. Chadwick, of Boston, on intrauterine injections, and the discussion which followed.

In conclusion, he added that his creed to-day is fully avowed on page 476 of the book to which he had before referred, and, unless in the future he learned new facts and new arguments to change his faith, he should "die impenitent."

Dr. Barker, when the reading of the paper was concluded, said: The subject is now open for general discussion. So many of the Fellows of the Academy have signified their willingness to speak, and there are so many whom all will be anxious to hear, that I felt it to be a duty to limit my remarks on the paper just read to a very restricted time and to content myself with the enunciation of such general principles as I believe to be important truths without entering much into details. But there is one point which I hope will receive attention from the speakers who are to follow. Very early in my practice I began to direct the use of vaginal injections for the first week after labor, the antiseptic being Labarraque's solution. When I went on duty at Bellevue Hospital, now nearly thirty years since, this was made the invariable rule in the lying-in wards. Subsequently carbolic acid was substituted, and I give a formula for its proportions in the work on "Puerperal Diseases." I have habitually directed its use in all my obstetric cases until the last two or three years past.

At the time of the meeting of the International Medical Congress in London, in 1881, I happened to sit at a dinner next to Dr. Thomas Keith, of Edinburgh, and had a very interesting conversation with him as to the use of antiseptics in ovariectomy. What he said was very suggestive and led to a good deal of subsequent reflection on my part as to the use of antiseptics in obstetrics. I recall to mind the fact that often in my practice I had seen disturbances and interruptions of puerperal convalescence the first week after labor, and it occurred to me whether this might be due to the carbolic acid; and the following autumn I decreased the proportion of the carbolic acid one-half, and thought that my patients did better.

On further thinking of the subject, I said to myself: The carbolic acid, even in the larger proportions which I have formerly used, was not strong enough to destroy the micro-organisms; and is it not possible that nature has wisely so arranged as to furnish the best fluid for constantly bathing the bruised and lacerated tissues in the much maligned lochia? Are not absolute rest and

freedom from disturbance of these tissues much more favorable to their restoration than any washes that can be used? Since that time I have considerably surprised nurses by directing that no injections should be used unless specially ordered.

Dr. A. A. Smith, who frequently visits my patients during the first puerperal week, when I have other engagements, informs me that this direction was given more than two years ago. Since September, 1882, it is only in a small proportion of my obstetrical cases that I have seen any reason for ordering vaginal injections. It is hardly necessary to say that absolute cleanliness is insisted upon, and that not a spot of blood should ever be found on either the person, or her clothing, or her bedding.

We both can declare that since this time we have not had a single case, including instrumental cases, which during the puerperal period has given us any anxiety, or required more than one ordinary attendance of one daily visit for nine days. This may be only a happy coincidence, but it seems to me significant. I think most present will be glad to hear the views of others on this new departure.

Dr. W. M. CHAMBERLAIN said: I do not propose to enter into the general discussion of this subject, or of either paper which we have heard, but would like to notice some remarks made at the first meeting, about the glass tube mentioned by Dr. Thomas and devised by me some years since for washing out the puerperal and septic uterus.

Dr. Thomas stated that he had found occasion to make some modifications in the instrument, namely, to bring the openings nearer together at the uterine end and to make an opening at the extreme end. The first of these I had long since done, and about the propriety of the second I still have some doubts.

Another gentleman criticised the tube as too large, and this suggests a point of much diagnostic value, as it seems to me, in determining in what cases intrauterine injections should be employed and when they should be omitted, or if employed, suspended. In every autopsy of a woman dying from puerperal septicemia which I have seen, the uterus has been found large, soft, and flaccid. And in all the cases during life which I have seen since I became critical on this point, the same condition has existed. There are different grades of septicemia, such as sometimes follows after slight breaks in the mucous membranes of the genital passages, a little tear of the cervix, vagina, or perineum, and is marked by a gradual rise of temperature day by day, until, a week or more after delivery, the patient may come into a critical condition. This is the analogue of surgical fever, and is to be distinguished from the explosive form, which is generally seen within forty-eight or seventy-two hours after delivery, ushered in by a sharp chill, lochial fetor, rapid rise of temperature, restlessness, anxiety, and prostration. When this exists, so far as I know, it always suspends the process of involution, the uterus becomes relaxed, the discharges accumulate in it, and are partially retained, and the uterus becomes a hot-bed where the spawn of microbes will fructify in profusion or the virus of putridity may ferment unrestrained. These are the cases, I believe, which require antiseptic uterine injections, and in these the os is so patulous and so soft that a large tube will pass readily, by its firmness will give a leverage which will lift the soft anterior lip, and leave beneath it a space for free exit of the injected fluid. In such softened tissue a large bearing surface is safer, even as

a large sound is safer than a small one in a soft urethra. While if the uterus be itself thus open there is reason to fear that its tubes and sinuses may also be patulous, and therefore liable to be entered or injected by a small tube, particularly if it be open at the end. The remarkable studies of Dr. Garrigues have shown us that the ordinary lochial discharge in a normal delivery is not noxious if access of a ferment or a contagium from without be carefully excluded. I hope we may hear from him again on this point, and will leave him to give his account, which he will do much better than I. To the objection that in my tube, as originally made, the antiseptic fluid is evacuated in the vagina before it reaches the womb, I can only say that I had the best results with the original tube, and think it not probable that so large a calibre would be evacuated so rapidly by an orifice without the womb when that orifice was so small.

I repeat it, that I find no occasion for irrigating an uterus in which the process of involution is going on at or about the normal rate, for such a uterus does not contain the septic material to be washed away. Some, at least, of the cases in which the injections have seemed to do harm are to be ascribed to the failure to make this distinction.

DR. W. T. LUSK thought that this Society was to be congratulated upon two such papers as that of December 6th and that of the evening, as, in many respects, in spite of the seeming antagonism between them, they really supplemented each other.

While it was his desire to confine his remarks to the local treatment of puerperal fever, yet he would venture to state his faith concerning the etiology of the disease, as in practice every man is governed by his theoretical views. He would say that in his opinion surgical fever and puerperal fever are not only analogous, but are essentially one and the same process.

To maintain this definition it was necessary to bear certain facts in mind. Much confusion had been occasioned by the failure to classify as distinct from puerperal fever the action of certain extraneous poisons, such as those of scarlatina, of typhoid and malaria which, ingrafted upon the puerperal woman, lose many of their characteristic features, and in their modified form present many features which are usually attributed to puerperal fever.

Again, the differences in symptoms between surgical and puerperal fever are in a large measure due to differences in the anatomical conditions. The entrance of poisons into the system through a clean cut stump necessarily would give rise to symptoms of a different nature from those produced by the same poison when introduced through a puerperal wound, where the serous infiltration of the tissues, the wide intercellular spaces, the dilated lymphatics and veins, the ordinary machinery for the removal of the retrograde products of the pelvic organs, all are active in conveying deleterious as well as the normal waste materials into the organism. Again, it should not be forgotten that in puerperal women a special danger exists in the proximity of the peritoneum to the seat of infection. But with differences of symptoms not explicable upon anatomical grounds, surgical and puerperal fever are linked together by the presence in both of the round bacteria. Both are of septic origin. Many arguments against the septic nature of puerperal fever are based upon a confusion of terms and the failure to recognize the modern distinction between putrid intoxication and septicemia proper.

A decomposing fluid containing rod-like bacteria only is charac-

terized by the foulness of the odors it emits. When injected into the veins it produces symptoms of profound disturbances in the nervous and chylipoietic systems, the animal experimented upon becomes feverish, depressed, a stinking diarrhea develops, but, unless the first injection be large or the injections frequently repeated, the animal finally recovers. This form is not infectious, because the rod-like bacteria under no circumstances thrive and multiply in the human system, and this explains the fact that physicians have been known to go from the dissecting room to the lying-in room with their hands still stinking, and yet have not communicated puerperal fever. But the round bacteria, which thrive in putrefying fluids, but to which putrefaction is not absolutely essential, unlike the rod-like variety, under favorable conditions, which we term the predisposition of the patient, may penetrate the tissues, enter the lymphatics, and be distributed to the parenchymatous organs and to the serous cavities, and far away from the original site of the disease may form sturdy colonies which excite inflammation destructive in character or interfering with the performance of function. Or again, the round bacteria may be disseminated through the system by portions of thrombi dislodged from inflamed veins. In many cases, it is true, no such general dissemination of these fungi need ensue. If the septic micrococci are of feeble activity or meet with resistance from the invaded tissues, it was possible for them to progress only a short distance from the point of entry, and there to give up the combat, giving rise to circumscribed inflammations, such as cellulitis and local peritonitis.

With regard to prophylaxis, he thought too great a burden could be thrown upon the practitioner by insisting upon non-essential details. He should hardly expect great results from washing furniture, walls, and floors with antiseptic solutions. At least, in lying-in hospitals, where he had seen many epidemics of puerperal fever, there was a time when everything was washed and scrubbed with great vigor. No pictures adorned the walls, no carpets covered the floors, and carbolic acid was used without stint, but he had never observed that these precautions ever exerted the slightest influence upon the prevention or restraint of puerperal fever. The fumes of sulphurous acid had since been substituted with the best results, and he would strenuously recommend the modes of disinfection employed by the Health Board in all cases where there had previously been scarlatina, diphtheria, typhoid, or erysipelas in the house. In this connection he would state that a woman should never, if possible, be confined in the chamber adjoining the bathroom, as he believed that puerperal women were extremely sensitive to sewer-gas poisons.

In normal confinements the uterus contracted during the expulsion of the head and body of the child in such a way that no air found entrance into its cavity. There is no evidence that decomposition ever occurs normally in the interior cavity. In the vagina the conditions, on the contrary, are all favorable to decomposition. There the lochia stagnate, and there are found heat, air, and moisture, which favor putrefaction. Putrefaction of the uterine contents is not a primary, but a secondary condition, the changes beginning in the vagina, creeping upward after a distinct interval into the uterine cavity. This he considered an important point in practice, as in cases where labor had been normal, and there had been no needless interference with its progress, if the

vagina was thoroughly cleansed it was rarely necessary to carry the injection into the uterine cavity.

In cases of difficult labor, on the contrary, where the hands or instruments had been introduced into the uterus, primary intra-uterine decomposition was rendered possible, and was then especially favored by lax uterine walls, and the presence of bits of retained placenta or deciduae. In such cases the intrauterine douche is often the direct means of saving patients' lives. It would, however, have been better if the uterus had been thoroughly disinfected, not after the symptoms of septic poisoning have developed, but immediately after labor. The douche was then harmless, it stimulated the uterus to contract, and was a powerful means of preventing subsequent dangers. For his own part he preferred the fountain to the Davidson's syringe, but the choice was apt to be determined by circumstances. In not every case of uterine infection were favorable results to be obtained from the douche. In cases where the round bacteria had been inoculated into a wound, the disease rapidly progressed into the tissues beyond the original lesion, so that they were often advancing, a victorious army, far beyond the reach of the stream which was thrown into the uterus. Washing the arm the day after vaccination does not prevent the development of vaccinia. Washing the uterus after the pelvic tissues are invaded does not prevent the development of puerperal septicemia. His advice, then, would be not to continue the uterine douche in cases where the results of the first injection furnished the evidence of its impotence.

The result of antiseptics, as understood by the speaker, had been most surprising in the lying-in asylums of Europe. In Vienna, where in his student days the mortality had been not far from five per cent, the reports now show one death from septicemia in two hundred. In Prague, during the last year, upon Professor Streng's division of eleven hundred cases, there was not one death from puerperal fever. But in these hospitals, and everywhere, the indiscriminate use of uterine injections has invariably added to the mortality. With all the blessings from their use, with the indubitable fact that they have been the means of saving many lives, unless the indications for their employment are carefully restricted, it is to be borne in mind that they are likewise capable of adding to the risks of the puerperal state, and of swelling the death-roll from puerperal causes.

DR. H. T. HANKS: Mr. President and gentlemen, having listened to the very valuable paper of Dr. Thomas on the evening on which it was read by the author, and having been invited by him and by our honored President to express my views upon this important subject, I have felt that it was due you all that what I have to say, where so many will be glad to speak, should be in the fewest possible words that will fully convey my ideas. I have said that this paper is a valuable one, and this subject an important one. I do not say it unadvisedly. I believe this paper will have a most vital and lasting influence, and that in America, among the more intelligent physicians, many of these suggestions made and the course treatment marked out by him are to become the accepted rules of the future on the treatment of this class of cases.

The profession of to-day, especially in this city, are ready to accept any plan which promises better results than those attained in the past. It is not enough for Dr. A. or Dr. B. to say that he has attended twenty-five hundred cases of labor, and lost but two patients from this fever among all this large number. If Dr. A. or

Dr. B. have had, or remember to have had, but *one* case in every thousand. Dr. C., just across the street, who is equally educated and just as skilful, confesses to have lost one in every 120 of all his puerperal women. Statistics from *memory alone are uncertain*. The disease is not found in the city so much more frequently than in the country, where you compare the numbers. I remember distinctly that in my five years' experience in country practice in Massachusetts one of my patients, two days after an easy, normal labor, drank a pint of *cold* milk. An hour afterward she was seized with violent colic pains and soon vomited. An hour later she had a severe chill, followed by every symptom of septicemia and rapid general peritonitis, and she died on the fifth day. Dr. Godding, from a neighboring town, a venerable and able physician, who was called in consultation, concurred with me in pronouncing this a typical case of puerperal fever. He explained it then in this manner: The cold milk drank by the patient was not digested and caused vomiting; straining at vomiting and the relaxation following allowed some extraneous poisonous matter to enter the open vessels of the womb; the chill and the fever followed in regular order, and death resulted. One other case, two years later, of severe puerperal fever came under my care during the five years of my country practice. This second one followed a difficult instrumental labor, where there was considerable laceration of the perineum and quite likely of the cervix, although at that time I had not been led to look after this lesion or expect a slow convalescence because of it. This was a case of puerperal septicæmia, resulting in pelvic cellulitis and general peritonitis. She recovered, however. This made two cases and one death out of seventy-five confinements. I believe, from the fact of my being called frequently in the country at the present time to see cases of puerperal fever, in consultation, that when numbers are actually considered and exact statistics are obtained, there will be found a much larger per cent of deaths in country towns from this disease than the profession have been led to suppose.

But in this city I have been interested to get at the facts, and have obtained from the President of the Board of Health the statistics of *deaths*—not cases treated—from puerperal causes during the last four years, and I find that there were 240 deaths in 1880, 259 in 1881, 246 in 1882, 262 in 1883. In 1882 the number was slightly less than in 1881, otherwise the increase from year to year could be explained by the increase of population. The startling fact is here manifest, that for the last four years in New York City, out of 120,418 puerperal women, there have been 1,095 deaths from puerperal fever, or one death in every 120 women who have borne children. One thousand and five deaths from among a class of patients who can be but poorly spared, who are the most important class, with one exception perhaps, of all that we are called upon to treat, is an alarming fact. I do not, therefore, speak at random when I state the profession at the present time are ready for any judicious change which promises better results for the future. And I thank Professor Thomas for able contributions upon a subject which has interested us all of late, and which is of vastly more importance to the profession and to the world to-day than the subject of "How to Operate for Cystocele," or "How to Fasten the Pedicle after Ovariectomy," or "How I Stand and Believe on the Code Question."

Many of us who are frequently called to see these cases of puerperal fever, have been often led to ask ourselves, "How can we

prevent and what can be done to cure these fearful cases?" I believe that I am sustained by a large number of the profession who are present this evening, if I say that "out of this one thousand and five deaths from puerperal cause in this city during the last four years many of these mothers would not have contracted this disease, and when contracted, might have been cured, had these rules laid down by Professor Thomas been judiciously carried out. My object in speaking will have been accomplished if I can help to emphasize the importance of nearly all of these rules. I will speak first of the name. I believe there is no better one than the one adopted or approved of in his paper, "puerperal septicæmia," and it may surprise you to know that to-day it is becoming more and more adopted by the profession. In 1880 there were reported in this city but thirty-four deaths from "puerperal septicæmia," while in 1883 there were *sixty-four*. In 1880 there were reported, however, forty-nine cases of death from "puerperal fever," and in 1883 there were eight less, although a larger number of *puerperal women died*. To show the result of the teachings upon this subject in the text-books of Playfair, Leishman, and our own Lusk, I will state that in 1883, in this city, there were but forty-one deaths reported from "puerperal fever," while there were 143 from "puerperal metritis and metro-peritonitis," sixty-four from "puerperal septicæmia," five from "puerperal pyæmia," one from "puerperal pelvic cellulitis," two from "puerperal cellulitis," and one from "puerperal phlebitis."

Thus we see that these several names are used to-day to denote that puerperal disease was manifested in these various ways. In the light of present pathological teachings and intelligent clinical knowledge, we may safely say that nearly all these several diseases have been caused, either directly or indirectly, by one and the same poison, taken into the circulation and resulting in these several organic changes.

For myself, I will say that during the last fifteen years in this city, where I have averaged sixty-five confinements in a year, besides a large number in consultation, I have had two and sometimes three cases of puerperal fever, not always three deaths, from so-called puerperal fever. I have had no case that I could not satisfactorily explain as caused by absorption of the poison at a certain point of the genital tract.

But to come down to the practical points of the paper—the prophylactic measures to be adopted.

Rule 1.—Preparing the room by proper antiseptics and disinfectants. Always try and carry it out, always be certain you do enough, and that without alarming your patient.

Rule 2.—The nurse and physician thoroughly to disinfect themselves and their clothing. This rule cannot be carried out too minutely.

Rule 3.—Bathing of the vulva, etc., of the patient with antiseptic fluids before the delivery of the head. Useful, but not so important as the first and second rules.

Rule 4.—Preparing nurses' and physicians' hands, etc., before labor sets in. Absolutely necessary.

Rule 5.—Removing all clots of blood and tufts of placenta, and the giving of ergot in *moderate* doses for a week. Certainly important. The use of ergot in small doses can do no harm, but in full doses (one-half to one-drachm doses) will cause an unnecessary amount of distress and uterine colic. Always give it where the uterus does not remain well contracted.

Rule 6.—The examination of the vulva should be made at once in all cases to see if the perineum is lacerated. If only a slight fissure exists, thorough cleansing with antiseptic fluid, or local application of persulphate of iron to be covered with vaseline is required. If the laceration is extensive, but does not involve the sphincter ani, one deep suture, or two at most, should be introduced at once, the ends lightly twisted, not tightly, as we must expect swelling of the parts—then bent down over the anus or fastened with perforated shot. In no case should the long loops be allowed to remain directly protruding, to be constantly jarred by the antiseptic napkin, and a source of irritation and sometimes of great suffering. But, however, where the patient is greatly exhausted, the nurse, husband, and friends somewhat demoralized, the *primary* operation should be postponed. For we all know that even the *secondary* operation is often the cause of great fever, distress, and occasionally, danger. Much more so *may* be the *primary* one. If laceration extends through the sphincter ani and into the rectum, by all means delay the operation until convalescence from the puerperal state has taken place, and you have your faithful assistants at command.

Rule 7.—Frequent antiseptic vaginal injections are necessary. Perhaps not in all cases; but if sickness follows when you have not used them, you will not cease to regret it. The use of the iodoform suppository, placed near os externum, may be useful in some cases. I should not insist upon this rule, since the distress and the labor attending their use must be considerable. The nurse cannot easily insert them, and the speculum ought never to be introduced for such purposes, excepting in extreme cases.

Rule 8.—Frequency of ordinary vaginal injection. A good rule.

Rule 9.—Drawing the urine frequently with a clean catheter. An important rule.

Rule 10.—See personally that the nurse can do, and does do, her work well. Excellent and absolutely essential.

In the *treatment* of puerperal septicemia I can only concur with every suggestion Dr. Thomas has made, and would add only one remedy to the list of therapeutic agents which he has recommended, and this is aromatic spirits of ammonia, given in one-half teaspoonful dose, diluted with brandy and water and generally given every two hours. Dr. Thomas' suggestion about the hypodermic needle is exceedingly timely.

In using the antiseptic vaginal injections I should suggest the use of a large bed-pan, like the one here shown, and thus avoid lifting the patient to the edge of the bed, for obvious reasons a dangerous procedure. The intrauterine injection should be used only when it is strongly probable that there is loose debris in the uterine cavity. In that case the cervical canal will be partially open, and the small-sized Chamberlain glass tube should be passed in with the greatest gentleness, and the antiseptic fluid cautiously injected. I would again repeat that only when it is almost *morally certain* that the cause of the disturbance is within the uterine cavity should the intrauterine injection be used. I should not advise the introduction of the Sims or Cusco speculum into the vagina, nor dilatation of the cervical canal, *unless* I were certain of finding large tufts of placenta in the cavity. The use of the rubber coil with cold water has served patients well after ovariectomy. I certainly should try it in puerperal cases whenever the occasion seems to demand. But this is a dangerous assistant if not watched. The diet of milk and animal broths is excellent.

The selection of a competent nurse and assistants necessary, if we are to carry out this plan, is an important matter. Some of the suggestions may seem unnecessary, a few, I believe, can be safely dispensed with. If the young physician who wishes to succeed in life should find it impossible to safely introduce the iodoform suppository every six hours, and he has no rubber coil, or that his patient has lost two drops of red blood from the vagina after the expulsion of the placenta, as one has lately claimed in this hall to be an abnormal and pathological condition, instead "of going out and hanging himself," or even following Horace Greely's advice and going West, he should stop and reflect upon the fact that women *have* recovered from puerperal fever without the aid of either iodoform suppository or cold coil; and also that though his first patient may stain one or two napkins daily for a week with red blood, still she too may recover, and they both may in time laugh at the author of such a statement.

In conclusion I wish to give you the names of the four assistants who have always, when called upon, effectually aided me in the lying-in chamber. I have, of late, been accustomed to call upon my new patient and the nurse some weeks previous to the expected confinement, and telling them both of my desire to have present during delivery, and all through the puerperal state, these four assistants. If they do not consent to my request, I respectfully withdraw from the case. If they consent, I go on and am satisfied with my labors, and expect perfect results. These four assistants are all important, and the first, second, and third, are of quite as much consequence as the fourth.

And as I look back at my record of births I find that the want of the first assistant has been the cause of a few deaths; the want of the second assistant the cause of no less than four deaths; the want of the third assistant the almost certain cause of three deaths, and weeks of suffering for others, while the want of the fourth assistant has been equally disastrous. I give their names: "pure air," "absolute quiet," "judicious diet," "proper antiseptics."

DR. PAUL F. MUNDÉ had listened with much pleasure and interest to Dr. Thomas' paper and to the discussion, but he felt at a loss as to what to say himself. Since he began the practice of obstetrics about eighteen years ago, he had seen a number of cases of puerperal fever in hospital and private practice, and it had become his custom to treat the disease locally—just as a wound which gave rise to septic infection would be treated in any other part of the body—and he had to confess his conviction that puerperal fever was, properly speaking, puerperal septicemia; yet he would qualify this statement somewhat. Certainly, when he was called to a case in which he found an offensive discharge from the uterine cavity, with rise of temperature, preceded or not by a chill, he felt it his duty to wash out the cavity of the uterus, and, if he failed to do this, he felt that he had failed in his duty to himself and to his patient. But he had seen cases in which, in the entire absence of evidence of infection of the uterus or of the parametrium, he could not help feeling that the condition was something different from what we found in cases of septic infection and that there were instances where we could not exactly trace the source of the trouble to septic infection. In short, he was obliged to subscribe to the views of the honored president in so far as to believe that there were some forms of puerperal fever which we could not call puerperal septicemia. What the nature of the poison was in these exceptional forms of puerperal fever he could not say. He saw

no reason why there might not be two forms of fever in the puerperal woman: one being what is known as puerperal septicemia, and the other a zymotic, infectious disease—puerperal fever proper. At any rate, whenever he had a case of fever during the puerperal state, with offensive lochia, he took the risk of washing out the uterus, and even if it were not necessary, he felt that, if properly done, it would probably do no harm.

With regard to prophylactic vaginal injections, much had been said for and against them. He himself had employed them regularly, but he must admit that the objections that had been brought forward against them might have some foundation. Probably one of the chief reasons why he used them was that the nurse and the patient commonly thought that all had not been done that should have been done if they were neglected, and would surely impute anything that might happen to their omission. He objected to the use of the Davidson syringe for either vaginal or uterine injections: cases had been reported in which liquid or air had been forced by it into the uterine sinuses, resulting in death, and he had seen one case of dangerous shock from an involuntary injection of the uterus by the nurse on the ninth day, while giving a vaginal injection with this syringe. He always used a fountain syringe, and he thought its advantages should be still more emphasized. The mere absence of discharge from the uterus did not contraindicate the injections; indeed, it might be a special reason for employing them. This was illustrated in a case in which the discharge had ceased simply because the uterus had become anteфлекed, and the uterine cavity closed in consequence. A chill took place, and the temperature rose to 105° F., but these symptoms were relieved at once by straightening the uterus by bimanual manipulation and washing out its cavity. He doubted the advantage of a terminal opening in the Chamberlain tube. He agreed with Dr. Lusk that, when the temperature remained high in spite of the injections, the "victorious army" had probably gone on into the general system, and our washings simply brought away the rear guard, and were liable to do more harm than good if continued.

DR. T. GAILLARD THOMAS was then requested to close the discussion. He rose and said: Mr. President and gentlemen, when quite a young lad I was present at a murder trial, which made a deep impression on my mind. When the case had been presented, the attorney for the commonwealth, who was a florid and rather bombastic orator, got up and made a speech of two hours. At his close all were very anxious to hear the counsel for the prisoner, who had a reputation for great eloquence. Judge of the surprise of the audience when he arose and quietly said: "May it please your honor, the case is closed. I rest it here. The gentleman on the other side has made a speech which tells so decidedly in favor of my client that I rely upon it for his acquittal." The prisoner was acquitted. I feel very much in this way with reference to the paper of Dr. Barker, to which I have just listened with feelings which I know you, who are his well-wishers, fully share with me—feelings of surprise, regret, and sorrow. Discussions such as these cannot fail, however, in the end to do good, for I am a great believer in the old Latin adage, *Ex collisione scintilla*; and although in view of the very grievous errors which, according to my distinguished colleague, I have brought before the profession and suggested for its adoption, I may be preferring silver to gold in deciding in favor of speech instead of silence, I feel called upon to say a few words in simple self-defence, that first law of nature.

Our honored President has been very guarded in opening his attack upon my paper, and has seemed to feel concern lest the author should take offence at his sallies. Let me assure him and you that it would take a great deal more than such a discussion as this to weaken the ties of a friendship which a quarter of a century has cemented between us, or to cause me to take exception to the criticism of one whom I have often in times past encountered in the lists of debate, and have ever found just, magnanimous, and courteous.

But alas, gentlemen, my adversary has to-night incautiously, and I think unwisely, ventured to use against me that dangerous weapon, a two-edged sword, ridicule. Right mercilessly has he given me one edge; let him beware of the other! As I saw him draw and flourish this weapon a few minutes ago I experienced mingled feelings of pleasure and of pain: pleasure, because I felt that I should probably escape absolute annihilation at the hands of one whose cause was so poor as to make him resort to it; pain that I should see my old friend, our President, in such a strait. In a scientific discussion, more especially in a debate which directly and immediately concerns the saving of human life, which at this very moment is being deplorably sacrificed among us, ridicule, elsewhere a powerful weapon, is the poorest and most pitiful of arguments. It is the resort of the weak, not of the strong; and, as my adversary used it just now, I said to myself, he feels himself to be very weak, he totters upon his pedestal; 'tis pity that he should feel so, for otherwise that master pen, which so often in times past has enchanted us, would not to-night emit what carries pain to my heart and to the heart of every true friend of his in this assembly, evidences of irascibility and of irritability which are so little characteristic of his real nature; otherwise I should not be able to recognize, as all others must do, the utter want of logic, the complete absence of argument, the total neglect of appeal to facts, and the very conspicuous presence of signs of wounded *amour propre*, which, like an unwholesome stream, meanders through his essay.

I shall not detain you long. I have little to say, for Dr. Barker's attack calls for no rebuttal and demands no argument on my part. I said all that I had to say on December 6th, when the original fire-brand was thrown down and unfortunately picked up by the wrong end by my excellent friend. There are, however, one or two points upon which I must touch, to avoid misrepresentation.

Dr. Barker declares the pathology which I have advocated to be unsustained by even the most recent researches of those who are our recognized guides; he appears to object to the fact that I have not stuck closely to the dicta of our text-books, and hugged to my soul the tenets of a past age, as he has done. This is hardly fair. I strove to follow the advice of Dr. Billings when he says: "Have something to say, say it, stop when you have said it." Had I had no opinions of my own to offer you, had a practice in a large metropolis and in great hospitals taught me nothing during a period of thirty years, I should not have appeared before you. Let my adversary inform himself upon the recent views of pathologists upon this subject, and he will find that it is his views which are effete, not mine which are jejune.

As far as I can gather anything certain from his discursive paper, the pivotal idea of Dr. Barker's attack seems based upon the belief that I regard the lochial discharge as a poisonous fluid which, by absorption by abrasions in the genital tract, gives

rise to puerperal septicemia. I need not tell you that no such absurd idea ever obtained foothold in my brain or enunciation from my tongue. If his idea be this, he has been guilty of very superficial reading of my paper, and should not so easily have concluded that I was affected by idiocy. Look at my essay, which is now in print, and you will see what you already appreciate must be the fact, that I stated merely that the lochial discharge was a material ready to take on those alterations which are effected by micro-organisms of bad character, which, changing its nature, render it poisonous to the abraded tissues. I believe that you will find that the pathology which I have there offered to you is abreast with the views of the advanced pathologists of Germany, France, and Great Britain. As to the pathology of my adversary, Dr. John Thorburn, of Manchester, England, very justly expresses concerning it, I think, the accepted view of the profession when, in a foot-note to an article upon "Metria," which appears in the *British Medical Journal* for August 11th, 1883, he says: "It would be inexcusable not to make some reference to the very valuable papers of Dr. Napier, in the *Obstetrical Journal* for 1880, on 'Puerperal Fever.' He, along with Fordyce Barker, defends the old position of a specific puerperal fever *sui generis*. The time limit imposed by our regulations allows no opportunity of consulting step by step such arguments as he adduces. I can only say that his invaluable collection of facts produces in me an opinion diametrically opposed to his own."

My critic upbraids me with want of thoroughness and sketchiness of detail with reference to my description of symptoms. I will merely say in answer to this that I intentionally assumed this style, as I was not preparing a lecture for a class of medical students, and my paper was distinctly announced to be upon "The Prevention and Treatment of Puerperal Septicemia," and upon nothing else. I cannot but thank him for his kindness in comparing my style in this sketchy description to that of Byron and Humboldt (I believe that these are the authors with whom he compared it); but alas, as I recall the passage to which he alluded, I am pained to confess that the similarity of style does not strike me as forcibly as it does my partial friend.

Here let me draw the veil of pitying silence over the unfortunate allusion to the squib of Stepney and the relics of the Hotel de Cluny. We stand to-night upon ground consecrated to science by the dignified fathers of the New York Academy of Medicine who have now passed away; we stand face to face with the terrible mortality which marks puerperal fever at this very moment.

And now, gentlemen, a few words as to the prevention and treatment of puerperal fever, which is the only legitimate subject before us for discussion this evening; the only theme which should not, at the very commencement of these exercises, have been rigidly ruled out as irrelevant by our President.—

Dr. Thomas then spoke of the methods which he had suggested for avoiding the occurrence of puerperal disease, and to which some seemed to have taken exception as being impracticable. To carry out the first requirement, regarding the preparation of the lying-in room, it was only the work of two or three hours for a laboring woman. But in making rules, it was necessary always to give a high, and not a low, standard. He would admit that a woman might be confined in a dirty room and yet have a perfectly natural confinement, but he thought it did good to get the mind of

the obstetrician into the proper channel, which was that of cleanliness, three times repeated.

Coming to the second rule, he thought this also quite practicable, and it was only this morning that he had bathed his body and washed his hair and beard with a solution of boracic acid, according to the rule laid down, having attended a case of puerperal septicemia yesterday. After going over the other rules, to which it would seem there could not possibly be any valid objection offered, Dr. Thomas spoke of the administration of ergot, which it had been his custom to do for some years, and with apparent benefit; and then of vaginal injections, regarding the propriety of which he must confess, in view of the evidence which had been brought out in the discussion, he felt a little weak. As Dr. Mundé had said, it was his usual habit to make vaginal injections, but he was willing to give it up at any time he became convinced that it was faulty. Only recently he omitted it in a case of deformed pelvis, in which labor was brought on at the eighth month, and the woman did perfectly well. In laying down the rules in the paper which he had read he simply gave what was his practice, and what seemed to him to be desirable, but he was willing to discard any part of them as soon as they were shown to be rash or injurious. With regard to intrauterine injections, he was, as stated in his paper, and in as plain and good English as he could command, utterly opposed to them except where there was good and sufficient cause, and he had stated as strongly as he could do the dangers which might attend the use of such injections. He was not aware that Dr. Barker had made use of them and described how they should be used in the book referred to, else he certainly would have given him due credit.

Dr. Thomas then quoted the statistics given by the speakers of the evening, and by others, going to show that puerperal fever was not of so infrequent occurrence as one might be led to suppose, and this was especially the case where antiseptic precautions were not observed. Since December 6th he himself had seen as many as five cases of undoubted puerperal fever, and this was not above the usual number for an equal period of time. He then gave an instance going to show that statements made from memory by physicians, to the effect that out of a large practice for a great many years they had not had a single case of puerperal fever, were very unreliable. They meant to tell the truth, but their memory failed them.

Dr. Thomas closed his remarks by saying that he was not dogmatically inclined to the rules which he had laid down; not at all. He was willing to alter them as soon as it was shown that they were objectionable.

ACADEMY OF MEDICINE—OBSTETRIC SECTION.

Stated Meeting, December 27th, 1877.

ALEXANDER S. HUNTER, M.D., *Chairman.*

DR. E. L. PARTRIDGE read a paper entitled

“PUERPERAL FEVER;” AN OUTLINE OF ITS NATURE, MANIFESTATIONS, AND MANAGEMENT.

For convenience, Dr. Partridge adopted Spiegelberg's classification chosen by Lusk in his chapters on the subject of puerperal fever. A practical study of the clinical history and pathology of

the affection seen in the puerperal patient leads to the conviction that there is, in the vast majority of cases, an underlying *septic* element in their causation. In a small proportion of cases, the fever depends at the outset upon one or more local conditions, septic poisoning occurring at a later period in the puerperal state. In a few rare cases, we may believe that we have simply the manifestations of local affection from first to last. Some would question the truth of the last statement; but the author of the paper could not believe that the recently-delivered woman might not suffer from cellulitis or peritonitis, as well as that those diseases might arise independent of childbirth or septic infection. In almost all instances, however, full facilities for clinical and pathological examination will reveal unmistakable indications of the absorption of septic poison from some portion of the genital tract.

Dr. Partridge then spoke of the facilities for the introduction and development of septic poison in the lying-in woman, such as from the placental site, laceration, etc. Septic material may be brought in contact with the absorbing surfaces in two ways: (1) Autogenetically; that is, by decomposition of blood, fragments of secundines, inflammatory products, etc. (2) Heterogenetically; that is, from exposure to atmosphere containing emanations from patients suffering from septicemia, erysipelas, scarlet fever, diphtheria, or from direct inoculation, etc.

With regard to the nature and mode of action of the septic poison, Dr. Partridge thought the view that the contagious principle consisted of microscopic organisms (micrococci) afforded a ready explanation of the numerous lesions of puerperal fever, the variation being due to the different degrees and extent to which the blood-vessels and lymphatics were permeated. Besides, he regarded it as reasonable that the quality and kind of this microscopic poison might vary, and therefore produce a variety of lesions.

The author of the paper then reviewed briefly the clinical history of endocolpitis and endometritis, in the simple form, and also that known as ulcerative, diphtheritic, or septic, which is far more serious.

Parametritis or pelvic cellulitis is the puerperal inflammation which occurs most frequently, and it may be either septic or non-septic in character.

Perimetritis or pelvic peritonitis is often associated with parametritis.

General peritonitis is a common puerperal affection, and there are two varieties: (1) That which is due to septicemia. (2) That due to an extension of the pelvic variety. In the septic form, pain and tenderness are wholly secondary to the prostration due to the blood-poisoning. The most prominent symptom for diagnostic purposes is persistent abdominal distention, with indications of septicemia.

Septicemia has a protean character, but the symptoms are

clearly demonstrable after the second week, and are chills, profuse sweats, rise and fall of temperature (never reaching normal), irritable stomach, yellow skin, coated tongue (disposed to be dry), low delirium and hallucinations, respiration shallow and hurried: and mental faculties dull. There will also be present indications of one or more of the local septic inflammations.

With reference to prevention and treatment, Dr. Partridge maintained that an uncontaminated atmosphere is the first essential. Inasmuch as the poison can be conveyed from place to place, the greatest care must be exercised by physicians, nurses, etc., etc., lest they carry the disease from one patient to another. In prolonged labors, it is necessary, and it is proper in *all* cases to use disinfectant vaginal injections *during* labor, and their judicious use, not too frequent, is called for *after* labor. The proper management of the third stage of labor is important, looking to the complete expulsion of clots and secundines and permanent uterine contraction. The hands of the accoucheur should be scrupulously clean, especially the finger nails, and rendered antiseptic.

In cases of septicemia when the point of inoculation is within the uterine cavity, or when it is known that this cavity contains decomposing fluid, warm carbolized intrauterine injections should be repeated every few hours, and they usually cause the temperature to fall promptly. Care is necessary that the injection be made slowly, and that there is free escape of the fluid. If sloughs occur, they should be removed as early as possible, and contiguous exposed surfaces should be covered with iodoform or touched with carbolic acid or tincture of iodine.

For local or general parametritis, leeches, early, may be beneficial. When exudation is present, vaginal injections of hot water, and, externally, poultices may be serviceable. Pelvic abscesses should be treated by aspiration or incision. Opiates, to relieve pain; quinine, for its antipyretic and supporting effect; and stimulants as indicated. Food should be concentrated, and easy of digestion. General blood-letting is improper at any stage of general septicemia. If arterial sedatives are used, their effects must be watched with the greatest care. Salicylic acid may reduce temperature, and the wet pack, carefully employed, may be equally useful. Stimulating enemata, asafetida, or turpentine, or even aspiration of the intestines with fine needles will afford some relief from excessive tympanites.

DR. JOHN C. PETERS said that one readily gets the idea that there are two varieties of puerperal fever: first, that which is of septic origin, and, second, the affection which does not depend upon sepsis. The remedies, also, can be divided into two classes: opium, calomel, carbolic acid, quinine, and camphor should not be used in the septic form of the disease, for micrococci will live in these drugs.

On the other hand, the mineral acids, such as sulphuric, nitric, muriatic, will destroy these organisms when used in very weak solutions. Twenty drops of a solution consisting of ten drops of

the strong sulphuric acid to one ounce of water will kill the bacteria in two ounces of fluid. Even a five-grain solution of tartaric acid will kill bacteria more quickly than will carbolic acid.

Dr. Peters spoke especially of the value of bichloride of mercury and turpentine as germicides, the former of which will kill the germs when used even of the strength of 1 to 20,000; of course, a much stronger solution could be used with safety.

DR. PAUL F. MUNDÉ said that there was little or nothing in the paper to which he wished to take exception. He desired to say, however, that Dr. Partridge had omitted to mention one of the possible causes of puerperal fever. He had always believed, as advocated by Dr. Thomas in a recent paper, that in the majority of cases puerperal fever is puerperal septicemia due to absorption of poisonous matter from the genital tract. But Dr. Mundé could not accept this as the cause in *all* cases; that is, he could not help leaning somewhat towards the view that puerperal fever *may* be a disease by itself. He had seen cases in which there was no evidence during life or at the autopsy of septic absorption. He would say, therefore, that, while in most cases puerperal fever is puerperal septicemia due to absorption of septic material from the genital tract, there are cases in which puerperal fever is due to a cause the exact nature of which is as yet unknown.

With regard to intrauterine injections, Dr. Mundé thought that neither the author of the paper nor Dr. Thomas had spoken sufficiently of the *exact* indications for their use. It will not answer to simply say that *every* time the temperature rises or there is a fetid discharge from the vagina we must wash the uterus out. Not at all. There must be more exact indications. He then referred to a case under his care at the time Dr. Thomas read his paper, and in which there was a rise of temperature to 102° F. before he went to the Academy, and to which, after the meeting, he returned with fear and trepidation and found the temperature up to 103° F. There were no fetid lochia. It was about the time for the beginning of the secretion of milk. He did not inject the uterus, the temperature fell under the influence of aconite and twenty grains of quinine, and the subsequent progress of the case was favorable. Dr. Mundé believed that a mere rise of temperature did not indicate the use of intrauterine injections; nor did the mere existence of fetid lochia. There must be an association of the two, or a combination with other symptoms, as with a chill, or manifest constitutional disturbance, to call for their use in the puerperal state. The *very absence* of lochial discharge may be a good reason for washing out the uterus. An illustration of this fact is furnished by a case in which the lochia were retained on account of antelexion of the uterus; absorption took place and a chill occurred. He corrected the malposition, a large quantity of fetid lochia were discharged, the uterus was washed out, and normal convalescence took place. The mere absence of fetid lochia, therefore, would have been an unsafe guide in deciding whether or not the uterus should be injected. He did not wash out the uterus unless the condition (fetid or not) of the lochia was associated with other symptoms indicating septic absorption.

In his experience, the very worst cases of puerperal fever are those in which, with persistent high pulse and temperature, there is entire absence of local symptoms, and the patient expresses herself as feeling exceedingly well.

As to the manner of giving intrauterine injections, Dr. Mundé did not agree with Dr. Thomas. He had met with a case in which

an intended vaginal injection with Davidson's syringe in the hands of a competent nurse produced the most severe collapse on the ninth day after delivery. He objected to propelling a stream of water with force into the puerperal uterus. He also believed that there is a time in septic endometritis when intrauterine injections are no longer useful. When the temperature is not reduced permanently within forty-eight hours, the injections having been given three or four times a day, they are more likely to do harm than good. It is true there are exceptions to this rule as to all others. He had several times seen a chill with rise of temperature follow an intrauterine injection. When this measure had been continued after the discharge had lost its offensive character, he thought that the injection then acted as a direct irritant to the endometrium.

With reference to parametritis and perimetritis during the puerperal state, Dr. Mundé had seen cases where the pulse and the temperature were those of puerperal fever, but an examination revealed an exudation in the neighborhood of the uterus, and made the diagnosis clear. When there is such a local inflammation, to wash out the uterus would be positively injurious. As to the antiseptic to be used, he was inclined to favor the bichloride of mercury, 1 to 2,000, for intrauterine injections. His rule is, in every case of confinement where there is an outlook for a labor of unusually long duration, or it is likely to be difficult, or there is a probability that operative interference will be necessary, to wash out the vagina, perhaps after every vaginal examination, then immediately after the child is born, and also, if he had introduced his hand into the uterus, to wash that cavity out with *carbolyzed ice-water* of the strength of two or three per cent. He did not use *hot* water habitually because it could not be employed *very* hot without the liability of having it *too* hot, and when only warm it is not a hemostatic. Ice-water, however, is effectual as a hemostatic, because one can never get it *too* cold. Then give a hypodermic syringe of the fluid extract of ergot, deep into the abdominal walls. Also give ergot, quinine, and strychnine until the uterus is thoroughly involuted, and two or three vaginal injections daily.

DR. S. BARUCH asked Dr. Mundé if he used vaginal injections as a prophylactic measure after normal labors.

DR. MUNDÉ said yes, and he directed that they be given once, twice, or three times a day, while he continued his visits.

DR. PARTRIDGE also said that he had always used them for that purpose, but not oftener than twice a day.

DR. BARUCH thought it was inconsistent to regard the genital tract after normal labor as in a condition of traumatism, or like a wound after an operation, and then treat it altogether differently. He believed it to be a practice fraught with some danger. At least it had been found so in German hospitals. He wished to record himself as opposed to these vaginal injections.

DR. DANIEL BROWN also wished to put himself on record against the continuous washing-out of the vaginal tract after normal labor. The genital tract of every woman after normal delivery is not necessarily in a septic condition, and besides he believed that considerable injury might be done by disturbing the patient, as must needs be for giving them. The genital tract is not an open canal. If the passage is offensive, of course, cleanse it with whatever remedies may be regarded as the best. With regard to quinine, he had never seen benefit follow its use, in either large or small doses, for reducing septic fever.

DR. MUNDÉ said that, while it might not be absolutely *necessary* to give vaginal injections, as a prophylactic after normal labor, yet he had not seen any damage done by ordinary vaginal injections with a *fountain* syringe, and thought it wise to resort to them.

With regard to the claim made by Dr. Brown that the ordinary lochia are not *necessarily* septic, and that the genital tract is not *necessarily* in a condition of laceration, or in a condition favorable to the development of puerperal septicemia, Dr. Mundé said that Ahlfeld had demonstrated that the most poisonous lochia were entirely inoffensive; and he further believed that no primipara is delivered without some lesion of the genital tract; even of multiparæ, there is probably not a case in which there is not some slight laceration of the cervix. Besides, Ahlfeld had shown that the most dangerous period in the puerperal state for septic infection is about the third day, and he therefore believed that vaginal injections should be given for four, five, or six days at least, after which date they might perhaps be safely omitted.

DR. BROWN said when he spoke of the normal condition of the vagina, he did not mean absolutely, but that there is not sufficient injury to necessitate the disturbance of the patient for giving the injections. Of course, if any reason showed itself, as proved by the symptoms or the local conditions, he would use them without hesitation.

DR. CHARLES JEWETT, of Brooklyn, said he had been accustomed to state that no physician can treat a score of fresh wounds with thorough antiseptic precaution; without becoming a convert to antiseptic surgery. Theoretically, his convictions were equally strong as to the value of antiseptics in the treatment of obstetric wounds, which he believed always exist after labor. Practically, however, his experience is not fully in accord with his theoretical convictions as to the necessity for all the antiseptic precautions that have been recently advocated. A little more than six months ago, he began a series of experiments in the maternity wards of the Long Island College Hospital to determine the value of certain antiseptic measures in childbed. These observations, thus far, cover about fifty cases, too small a number, it is true, to be of much value as evidence, but the results are at least suggestive. The first thirteen of these cases were conducted under the direction of a skilled obstetrician with every safeguard except the use of special antiseptic measures. Thirty-six cases immediately following were conducted under the following precautions: The wards were alternately vacated and disinfected with chlorine on the 1st and 15th of each month. The beds were renewed throughout, even to the straw, on the discharge of the patient, so that each new patient went upon an entirely fresh bed. The bedsteads were cleansed antiseptically. The usual antiseptic measures were enjoined in case of the medical attendants and nurses. The vulva was regularly cleansed and constantly protected, during the puerperal period, with a guard of oakum dipped in carbolic or bichloride solution.

Of the thirteen cases, two (nearly sixteen per cent) had temperatures which at no time in the first week of the puerperium exceeded $99\frac{1}{2}^{\circ}$. Of the thirty-six cases, twenty-five (nearly seventy per cent) at no time in the first week had temperatures above $99\frac{1}{2}^{\circ}$ F.

To test the value of the vaginal douche as a prophylactic measure, with a few exceptions every other case was douched twice daily or oftener during the lying-in period, beginning at the close

of labor. Half the cases, therefore, were douched, and half not douched. The fluid used was, in the portion of the cases, three per cent carbolic solution, in a majority of them a one to one thousand bichloride solution, both used warm. The temperature record thus far shows practically no evidence in favor of the douche. Involution, however, appeared to be more rapid in the douched cases. True, this evidence is not conclusive against the value of the douche, for, should septic matter in a given case gain access to the passages in spite of other precautions, then the douche might save the patient from child-bed fever.

For curative purposes, the value of the douche is beyond question. He had resorted to intrauterine injections only when vaginal irrigation has failed to reduce the temperature within a few hours. For the intrauterine douche, he had not been wholly satisfied with the Chamberlain tube. He had had a smaller tube made of three-eighth-inch diameter, with two holes only, on opposite sides, and close to the end.

There are certain forms of child-bed fever, however, in which the antiseptic douche can have no curative value.

DR. PARTRIDGE, in closing the discussion, said he certainly believed the vaginal douche is capable of doing a great deal of harm unless properly used. He would not trust to any nurse to give them unless he knew positively that she was skilful. Then, if used twice in twenty-four hours, even in cases of normal labor, it can do no harm.

With reference to thorough drainage which the anatomical conformation of the parts is said to afford, he thought it a uniform experience that where a digital examination is made, the finger is covered with blood and mucus, offensive, and in a condition to readily become poisonous in character. He did not believe there is any single indication for the *intrauterine* douche: it is only when there is a combination of symptoms which indicate the presence of decomposing substances in the uterus that they should be resorted to, and then not too frequently. He favored the practice of supplementing them by the use of iodoform and glycerin—a ten-per-cent solution—already mentioned in his remarks on Dr. Thomas' paper before the Academy.

With reference to puerperal fever depending upon some poison not septic in character, he must confess that he believed the question still to be an open one, although he was unable to see why those cases might not be of septic origin. They are cases of intense toxemia, although we do not know what the nature of the poison is, and he could not see why it might not be septic.

Dr. Partridge believed that labor is never completed without some laceration about the cervix.

TRANSACTIONS OF THE GERMAN GYNECOLOGICAL SOCIETY.

(Continued from page 203.)

Third Day—Morning Session.

MUELLER (Berne) in the Chair.

WIEDOW (Freiburg) presented a paper on

SPAYING FOR UTERINE FIBROIDS.

He pointed out that at present the value of spaying for uterine fibromas is estimated very differently. This is due, in his opinion, to an over-enthusiastic reception at the beginning, and the consequent performance of the operation in inappropriate cases, which explains the failures as opposed to the very favorable results.

W. gave a brief synopsis of the 63 cases thus far reported which have come to his knowledge, 12 of which ended fatally. Hegar has done the operation 21 times with 3 deaths, equal to a mortality of 14 per cent. In one of the cases, after six months of good health with diminution of the tumor and cessation of the hemorrhages, there appeared renewed growth and fresh metrorrhagias. At the same time, fluctuation could be demonstrated in the tumor. The patient died a few months later. The autopsy showed a fibrocystic tumor, the lymphatic spaces of which were filled with purulent serum. This case, like one operated on by Schroeder, in which the tumor also enlarged after operation, was one of a colossal tumor. For such the prognosis must be called unfavorable.

In the remaining 17 cases of Hegar, the result was very satisfactory. Sooner or later the menopause and diminution of the tumor ensued. Comparative drawings of the size of the fibromata before and after the operation were exhibited; finally four cases operated on were presented for examination. To a question by Schatz whether there were among the cases reported any in whom the function of the ovaries had ceased, he answered in the negative.

FREUND (Strassburg) reported respecting six cases operated on by him. The results were favorable: the tumors diminished, the hemorrhages ceased, but in one case, indeed, they returned at intervals of four or five weeks. In one case only the result was nil. In that case likewise the tumor was a colossal one.

HOFMEIER (Berlin) inquired where the line should be drawn in the indication for spaying as opposed to myomotomy.

HEGAR (Freiburg) said that in this respect he would not confine himself to the size of the tumor. In a concrete case we should weigh particularly the dangers of the one operation against those of the other. On the average, oöphorectomy is less dangerous. Still, cases are met with where the conditions are reversed. For this reason, where oöphorectomy is to be performed, he always keeps in readiness the instruments for the eventual removal of the tumor. In tumors with an appropriate pedicle he also prefers ablation. In general he holds oöphorectomy to be a very effectual operation. He thinks it desirable to operate before the tumor is too large.

KALTENBACH (Giessen) pointed out that the position of the ovaries at the posterior side of the tumor may greatly complicate their removal. He reported two operations which were followed by good results. He mentioned the fact that he had seen several spayed women in whom the expected cessation of the menses did not occur. Schatz, too, had experienced this after double ovariectomy.

MUELLER (Berne) had operated six times, once with a fatal result, once without completing the operation. In the remaining four cases the hemorrhages ceased, the tumors diminished. He looks upon spaying as a make-shift merely; the ideal remains the extirpation of the fibromata.

HEGAR (Freiburg) stated that even with the normal climacteric there occurs cystic degeneration of very large fibromas; or else, first a shrinkage, then enlargement with subsequent fluctuation. The same takes place (comp. the cases cited above) with the anticipated climax. For this reason he, too, thinks that the prognosis in very large fibromas is doubtful.

PROCHOWNICK (Hamburg) read a paper on

PUNCTIFORM CAUTERY OF THE VAGINAL PORTION.

The author acknowledges the advantages of the bloody operations on the vaginal portion—the wedge-shaped excision, amputation of the cervix—but he points out that their result is often negative; that untoward sequels (*e. g.*, retroversion) may appear after amputation of the cervix. Besides, they are complicated, require a more careful watching of the patient, and hence are not appropriate for dispensary patients and for women of the laboring classes. Therefore P. recommends, as a substitute in such cases, punctiform cautery of the vaginal portion with a red-hot wire or fine-pointed Paquelin. The punctures should extend to a depth of one or two centimetres. They are to be repeated about every three weeks. Altogether, from three to five sittings are generally sufficient. The funnel-shaped granulations heal in two to three weeks. The result of the cauterization is not an immediate one. At first even, owing to the irritation of the tissue, swelling appears; subsequently, a shrinkage due to the obliteration of vessels and glands.

FRÄNKEL (Breslau) confirmed P.'s experience. He called to mind the fact that Spiegelberg had formerly effected the decrease of the swollen uterus by the punctiform employment of the Paquelin cautery. He had observed the same effect in the vaginal portion after a superficial application.

JUNGBLUTH (Aix-la-Chapelle) recommends his aseptic sponge tents for the affections in which P. would employ the cautery. Of course, after their introduction the patient must remain quietly in bed. After the tent has remained eight to ten hours, it is removed and the vaginal portion cleansed. By this treatment, ulcers of the cervix are not made worse; they heal up under the involution to which the whole uterus is stimulated.

MUELLER (Berne) inquired for the results of the cautery.

PROCHOWNICK stated that the cervical catarrh diminishes, the slight bleeding of the cervical mucosa ceases, and the swelling of the uterus becomes less.

BANDL (Vienna) thinks that inasmuch as the uterus does not in-

volute in the climacteric with old chronic forms of metritis, its diminution cannot be effected in these cases by cautery. In spite of all therapeutic measures the uterus remains hard and large. We must be content with the removal of the leucorrhea or the hemorrhages, respectively.

SCHATZ (Rostock) spoke on

THE OS INTERNUM.

He put the question: How can we, during pregnancy, labor, and the puerperium, recognize the internal os uteri which is generally easily and certainly determined in the non-gravid, non-puerperal uterus? Neither careful macroscopic nor microscopic examination of the musculature can here solve the problem. The thickness of the uterine wall and the course of the muscular fibres is varying also in the body of the uterus. The narrower network of the muscular bundles of the cervix is no proof that a portion with narrower muscular meshes does not appertain to the body likewise. Nor does Sch. admit that the quality of the mucosa can be valid, for its alterations beyond the internal os are but gradual and vary individually just as much as in the external os. The mucosa of the upper part of the cervix is very often, if not always, similar to or exactly like that of the body, or vice versa. In the same way, the height of the more intimate attachment of the peritoneum furnishes merely approximate landmarks, and can be utilized, at best, in the specimen, and in the living woman only during laparotomy.

According to Sch., absolute certainty is furnished by the physiological activity. Body and neck of the uterus he holds to be altogether different in their physiological function, perhaps like the ventricle and auricle of the heart. Only the former two are not divided by a tendinous ring which makes the difference so clear even anatomically. They are directly joined muscle to muscle. But they are completely separated by their altogether different innervation or contraction. Unfortunately, at least in clinical observation, the contraction of a muscle can be recognized merely by its shortening or tension. Now, if two muscles are so joined together as to form a single cord with fixed ends, the whole system becomes tense by the contraction of one: the one muscle actively by contraction, the other passively by extension. The thickening of the former, the thinning of the latter, will furnish a guiding point for the differentiation of the two muscles only when the difference between them is sufficiently marked by very firm contraction of the one or very great extension of the other. On the uterus the variation in thickness cannot be utilized as a means of differentiation; first, because a thinner zone occurs in the neighborhood of the internal os also in the actively tense body of the uterus; second, because the thickness of the uterine wall cannot be determined with sufficient exactitude during labor and pregnancy. On the other hand, the tubular form of the uterus with its varying width, and still more, the great and regular interchange of the

phenomena from pregnancy to the end of the puerperium, furnish sufficient information.

The difference between body and cervix shows itself most clearly immediately after labor. During the pain, the former is contracted throughout and on itself so that its several muscular bundles keep themselves mutually in tension. Per contra, by the complete evacuation of the genital tube, the passive tension of the cervix is deprived of its support and, so to speak, of the fixation of the terminal points. Hence, the passive tension ceases altogether. The cervix, dilated during labor to a length of ten to twelve centimetres and a width of thirty centimetres, is so lax that to a cursory examination it imparts the same sensation as the equally relaxed vagina.

Aside from the great laxity and width of the cervix, an error is all the more likely to be committed because, on the one hand, the lower part of the greatly thickened, firm, and the descended uterus, as opposed to the relaxed, vagina-like cervix, forms something like a vaginal portion with an os admitting two fingers; on the other hand, because there is above the apparent external os (really the internal os) a narrower part which may be mistaken for the internal os. But this, as has been stated, is merely the peculiarly contracted body. The latter, like every completely contracted hollow muscle having a larger opening in its wall, allows the gaping of the margin of this opening—the margin of the internal os—by the external muscular layers of the body drawing it so far outward until the internal ones antagonize it. Above this opening a narrowing is found at the point where the eversion ceases. On the whole this relation presents great individual variations; the constriction above the internal os is more pronounced in a long slender uterine body than in one shorter or even transversely developed.

Despite the above-mentioned deceptive appearances, the cervix can be clearly distinguished from the body, immediately after labor. The lower margin of the corpus uteri muscle surrounds the internal os in the form of a more or less sharp-edged, firm ring. The true vaginal portion lies much lower than this apparent one, is equally relaxed as the vagina and cervix, but is readily recognized by placing the index finger into the true vaginal vault, the middle finger into the cervical canal, and thus grasping the relaxed vaginal portion between the two fingers. We can even depress or withdraw its anterior margin to the vulva, where it often enough appears spontaneously. Cervix and uterus can be best differentiated during a pain.

During the first days of the puerperium the relations remain nearly equally distinct; in one respect even they become still clearer. The cervix contracts rather quickly and thus the vaginal portion becomes more easily recognizable; while during the pains which are still of frequent occurrence the internal os and the lower part of the body can also be clearly felt. From the

sixth to the tenth day on, however, the os can no longer be certainly distinguished. At this time the upper end of the then firmer and narrower cervix can be mistaken for it. Errors may also be caused by lacerations and old cicatrices which may make the cervix appear at some point narrower and firmer in the form of a ring. They will be avoided if we bear in mind that the cervix in the puerperium contracts equally rapidly and perfectly in length as in circumference to its ordinary dimensions, so that its length on the sixth or seventh day is no more than four to five centimetres.

Special attention should be called to the fact that, after thinning of the uterine wall, the involution causes the above-mentioned narrower part of the lumen to disappear so that the internal os again becomes the most contracted point of the body of the uterus. In the case of mal-involution, however, the internal os may be relaxed and dilated even some weeks post partum.

As regards the manner of contraction of the cervix in the puerperium, Sch. has not been able to satisfy himself exactly. He has not been in the position to demonstrate any rhythm. It is certain that it is not directly dependent upon the rhythm of the pains. Breisky and others have previously called attention to the fact that the pains of the pregnant, as well as of the non-gravid uterus soften and dilate the cervix.

Sch. then discussed the difficulties of recognizing the internal os during labor. They consist, in the first place, in the fact that the os moves continually upward as the cervix dilates, but more especially in that, with equal contents, the cervix is equally tense though passively, as the body. Still, with some experience, it is usually possible to find the os in primiparæ by the fact that the cervix, at least in the beginning, is somewhat wider and more dilatable below than the os itself. In multiparæ this is still more evident, and most so in those with a flat pelvis. Here the os internum, as compared with the cervix which in former labors has been greatly distended and remained dilatable and which has been more fully expanded by the bag of waters, forms a very prominent firm ring even before the membranes have ruptured. After the rupture the cervix, now no longer distended, depends so loosely that it was formerly believed to be paralyzed, while the internal os still juts out distinctly below the head. After the cervix has retracted over the head, its passive tension recommences. On passing the hand high up, the internal os is soon found, eight to ten centimetres above the innominate line, and, where it is difficult for the head to engage, even at the level of the umbilicus. Not only in contracted pelvis, but in every labor impeded in the course of the lesser pelvis, the internal os can also be felt and even seen through the abdominal walls. Not that there is a stricture of the uterus, as was formerly believed. Sch. objects to terming this ring a ring of contraction, because that would imply the conception that the ring is more firmly contracted than the parts

above and below it. Below, there is no contraction at all; above, it is no greater.

The author also looks upon the expression "lower uterine segment" as ill-chosen. Some do not seem to attach any definite idea to it, others understand by it a lower zone of the body of the uterus, and others again all that part of the uterus below the internal os. But for the latter the term cervix is amply sufficient.

Finally the author considered the oft-repeated question, Where is the os internum during pregnancy, particularly the latter part of it, and is a portion of the cervix even during gestation employed in inclosing the ovum? Where this is the case, Sch. does not look upon it as the effect of parturient activity. Perhaps the simple, elastic tension may have this effect also independent of the pains. But even when the latter are present, we are not justified in interpreting it always as parturient activity. Often parturition is not effected despite long continuance of the pains. Not rarely, in spite of considerable dilatation of the cervix, it does not set in. Sch. has observed several such cases. He would not say that the dilatation of the cervix belongs to pregnancy as a rule.

BANDL (Vienna).—As is well known, it seems indeed after labor as if the cervix and vagina had been simply dilated by the child like a rubber tube. In order to understand this phenomenon perfectly, we must bear in mind, on the one hand, the arrangement of the musculature; on the other, the absolutely normal course of labor. The anatomical substratum for the internal os—so conspicuous in pathological cases and after labor—is not a simple sphincter; but is the middle muscular layer to a greater extent, at the point where the larger vessels enter the uterus. In cases proceeding quite normally, it extends pointed far into the cervix and is distributed pretty evenly in the middle of the uterus. The internal longitudinal layer radiates outward into the external os where it is inserted; the external layer, investing the uterus like a hood, extends into the adnexa, especially the round ligaments, is continued to the vagina, and inferiorly it connects intimately with the muscular introitus vaginæ and the muscular pelvic floor. In the normal primipara, the head is low in the pelvis when the pains commence, and the lower uterine segment, cervix, surrounds the fetal skull like a hood. The parts are so thin that the sutures and fontanels can be felt through the anterior vaginal vault. Attached to it is the remaining portion of the cervix in the shape of a small tube, two to three centimetres in length, usually drawn backward, and with Müller's ring, having a free border, generally still contracted. The remaining portion of the cervix and the external os are opened only by the musculature of the uterus which is inserted there. For the shortening and gradual thickening of the cervix in the first period of labor can be distinctly followed. Moreover, if its tissue was thickened, diseased, or its structure was naturally too small for the head (both of frequent occurrence), the caput succedaneum forms even during the opening of the organ, and there ensues true mechanical dilatation or laceration. Similar conditions obtain with the vagina and its introitus. In a normal labor, the soft, thin muscular hood—cervix—in which the low-lying head is resting, becomes tense in the early period with the first pains; then shorter and thicker without the descent of the head or

ovum; nay more, the external os gradually disappears over the head into the wall of the utero-vaginal tube. The same phenomenon takes place with the vaginal tube. By degrees only does the entire utero-vaginal tube enter into effective combination with the adnexa, especially the round ligaments, the muscular introitus vaginae, and the musculature of the pelvic floor. Still more complicated is the process of opening the introitus vaginae. A part of the musculature relaxes, another contracts. Cervix and vagina are found so relaxed after labor because they have before participated in the general contraction while in a distended condition. The opinion that cervix and vagina normally are simply mechanically distended is altogether erroneous. For the most delicate ovule is often delivered unruptured, and the infantile body, covered with a thin layer of vernix caseosa, shows that at no point has it been in prolonged close contact with the parturient canal. Nor is the vagina a narrow tube outside of the period of pregnancy, but it presents itself on examination through a Sims speculum as an organ sufficiently roomy often to contain a small infantile head. When distended with air, and still more with water, it becomes harder, contracts especially in its outer layers. Braun's section of a parturient woman also shows how the vagina (anterior wall only four centimetres long) shortens during its action.

In normal primiparous cases, if examination is possible beyond the pelvic inlet, no projecting internal os is present, and even when the head is low, after the external os has disappeared in the wall of the utero-vaginal canal, the internal surface is smooth (this seems to be contradicted by Braune's section, but Br. himself states that the labor was not normal). Only when abnormal resistance is presented by the external os, the pelvis, or the soft pelvic floor, after prolonged ineffectual activity the middle muscular layer and the entire corpus uteri contract farther, the latter becomes more thick-walled, smaller, while cervix or vagina now really begin to be mechanically dilated. Thus is effected a difference in the wall between body and cervix, and the internal os is then felt more or less projecting. Therefore, after a perfectly normal labor, the lax cervical portion is not felt by far as thin-walled as after a pathological labor. B. has also sought to become familiar with the normal and pathological relations of the genital canal and the behavior of the internal os by injecting, shortly after death, a composition of tallow into the tube which had been closed at the introitus, freezing the subject, and then making sagittal and frontal sections. In perfectly normal cases, the form of the uterine cavity was that of the figure 8, the thickness of the wall at the cervix, vagina, and fundus nearly equal. Slightly below the middle was the constriction of the figure-of-eight, and here the organ, for a distance of three fingers' breadth, was at least twice as thick as at the fundus and cervix, into which the thickened part gradually merged. This greatly thickened portion of the uterus is the internal os; its physiological function is chiefly the arrest of hemorrhage after labor.

A similar section of a pathological case, in which during life a distinct limit was visible between the body of the uterus and the abdominal walls, was shown by B. at the meeting in Graz. At the anterior wall, the cervix; at the posterior, the vagina had remained much thinner. During life, the external os was caught in front between the head and the brim of the pelvis, while behind the vagina was drawn over the head which was high up.

SCHATZ (Rostock) considers that examinations of the cadaver are not valid. Only by examining the living we can gain an insight into the relations actually existing.

The lower uterine segment had always been termed the upper portion of the cervix by himself, and he had attempted to prove that the decidua found on this part of the organ is nothing but the cervical mucosa as altered by the hypertrophy of gestation. K. refers to his former investigations of this subject and exhibits two specimens in support of his view. The first of these was obtained from a primipara who had died one hour after labor, in whom the lower uterine segment was very well developed. Inferior to the lower uterine segment is still found some of the cervix with folds having a length of $3\frac{1}{2}$ cm. On this cervix the longitudinal ridge of mucous membrane, termed by K. the trunk of the arbor vitæ, extends upward to a distance of about three centimetres. On the non-gravid uterus, however, it extends exactly to the middle of the cervix. Now, it is improbable that in the puerperal organ the upper half of the cervix should be only one-half centimetre in length, while it measures two centimetres in the non-puerperal one. Consequently, the portion bearing the arbor vitæ is not the whole of the cervix, but only a part of it. The rest is to be looked for above, that is to say, the lower uterine segment must be considered to be such.

The other specimen is a menstrual decidua having a length of five and a half centimetres (after maceration in spirits of wine) which represents, in hour-glass shape, a cast of the entire body and cervix, with distinct constriction at the internal os. Thus it is proven that the cervical mucosa may both anatomically and functionally change into decidua.

FREUND (Strassburg) contradicts Bandl's assertion that during normal labors the ring of contraction cannot be felt by the high touch. Müller's ring likewise can always be felt. He illustrated by some drawings his views in regard to the contractile relations of the uterus.

HOFMEIER (Berlin) thought Schatz's proposition to determine the internal os according to the different functions of the body and cervix to be unpractical. He denied Schatz's statement that the Berlin school does not connect a definite conception with the term of lower uterine segment. By the latter he understands that part of the body which lies below the firm attachment of the peritoneum to the anterior and posterior uterine wall, that is, inferior to the ring of contraction in the parturient. This latter is not identical with the internal os. H. does not admit that the expression "ring of contraction" is to give rise to the idea that the ring is more firmly contracted than the parts above and below it; it is to indicate merely that at this point the contraction of the musculature of the body is most clearly manifest.

(To be concluded.)

ITEMS.

1. THE desire to bring the whole of the recent discussion on PUERPERAL FEVER, which was initiated by Dr. Thomas and continued by Drs. Barker, Lusk, and others, in this number, has crowded out several original articles already in type.

2. THE great success of the NEW YORK POLYCLINIC, since its inception, one and one-half years ago, has enabled the Faculty to purchase the large

four-story, double-front building of which it has hitherto occupied the ground floor, at a cost of sixty thousand dollars. Possession will be taken May 1st, and the entire building will be utilized for the Polyclinic, the upper floors being intended for private clinical and lecture rooms and a hospital, while the ground floor will be continued as a dispensary, apothecary's quarters, reading-room, etc. During the past five months, one hundred and twenty-eight practitioners have availed themselves of the advantages of the institution, as against one hundred and sixty-one during the first eleven months of its existence. Of these 128, 82 took the gynecological ticket. The prosperity of the Polyclinic proves that there is ample room in New York for more than one institution for practical post-graduate instruction.

3. A YEAR and a half ago, the "COLLEGE OF MIDWIFERY" was started in New York City, under the general law permitting the foundation of charitable institutions. The object of the institution seemed praiseworthy, namely, to prepare well-trained midwives for those classes of society who will not employ an educated physician, in place of the too often ignorant and unscrupulous women imported from abroad, or manufactured here by bogus schools. The "college" should really have been called a "School for Midwives." In spite of this minor objection, which was overruled by the original projectors of the enterprise, several professional gentlemen of reputation and standing lent their names and active assistance to make it a success, hoping, by their presence and supervision, to prevent any possible infringements of the unwritten code of professional propriety and honor. Unfortunately, although assured by the active promulgators that no important step should be taken without their cognizance and approval, several highly irregular and objectionable moves were made by the original projectors of the enterprise, which, after repeated protest, compelled those of the Board of Supervision who valued their professional honor and self-respect to retire unconditionally from the institution.

Being still of the opinion that an enterprise of that kind would be of value to the class of women who need intelligent and trained assistance in labor, and who are unable to pay a physician a fair fee, and with the view of raising the standard of midwives in this country, and of counteracting the evil influence of an institution managed on non-professional principles, as is now the above-mentioned "College of Midwifery," a number of gentlemen, chiefly medical, and a few laymen, have combined to secure the passage of an act through the Legislature of the State of New York, incorporating a "Maternity and School for Midwives," and another act requiring all midwives who may practise in this State, after October 1st, 1884, to pass an examination before a board of examiners, consisting of the Faculty of said school or any other similar incorporated school. The gentlemen comprising the incorporators of this school are Drs. A. L. Loomis, A. Jacobi, T. G. Thomas, Paul F. Mundé, A. S. Church, P. C. Cole, John Alsdorf, Rev. Drs. Watkins, Armitage, and Messrs. Chas. Coudert and B. A. Willis, several of whom were connected with and withdrew from the "College of Midwifery" when its management became irregular. The character of the gentlemen above-named, and the fact that they have control of the institution, will be sufficient guarantee that no objectionable or doubtful methods as to the securing of patronage, etc., can be practised.

These bills were presented to the Medical Society of the County of New York at its last meeting, and referred to a special committee, which will report at the next meeting. If favorable, the bills will at once go before the Legislature.

We hope the profession will recognize the difference between these two institutions, and give their support to the one here indorsed, the "NEW YORK MATERNITY AND SCHOOL FOR MIDWIVES."

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY

GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

POISONING BY POTASSIUM CHLORATE.

BY

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POTASSIUM chlorate, discovered in 1786, became immediately popular as an antifermentative remedy, containing as it does an excess of oxygen (KClO_3), and readily giving it up under favorable conditions. It was recommended by the chemists as such an agent, and great was its renown and use in every affection which could possibly be classed as a "putrefactive" disease. The ease with which the salt gave up its oxygen when subjected to heat, and the reports of its reddening effects upon the blood and tissues when administered to men and animals, served still more to establish it firmly as a valuable oxygenator.

Successive failures, however, began gradually to contract the sphere of its alleged utility, and so it became less and less fashionable, until, finally, at about the end of the eighteenth century, the drug enjoyed a very limited popularity.

During the furor, however, incidental to its discovery and introduction into therapeutics, and to its almost universal employment, Dr. Ferriar, of England, had recorded its success in the sore gums of scurvy. This observation lay quiescent until about 1815, when others corroborated it. Gradually the drug came again into favor, but now as adapted to a special and entirely different class of diseases, namely, inflammatory affections of the mouth and upper air passages; and while to-day we occasionally see accounts of its use in other

affections, I think it safe to say that the medical use of chlorate of potash is practically confined to the treatment of the conditions just mentioned.

The accounts given of the effects of both small and large doses of this drug upon man and animals are conflicting. The early observers, as well as those of the present day, agree, however, that solutions taken internally, or injected into the veins, produce a decided reddening of the venous blood and flushing of the gums, palate, and skin, as well as diuresis.

Large doses were said to produce an abundant flow of urine, salivation, a sense of weight about the loins, and in some children diarrhea with green stools. O'Shaunnessy first said that the salt passed off unchanged in the urine, and this important statement was subsequently confirmed by eminent chemists (Wöhler, Gustin). About 1860 several English physicians (Osborn, Cooke) suggested that chlorate of potash might not be altogether harmless, yet, according to Stillé, gave no experimental proof of its dangerous effects.

In the 1868 edition of Stillé's *Therapeutics and Materia Medica*, the author says: "There are only two examples of injury produced by chlorate of potash. The one occurred in a case of phthisis for which a physician prescribed 300 grains of the salt to be taken daily, in solution. The unfortunate victim of professional ignorance took this dose on four consecutive days, when 'the pains in his bowels became very severe, incessant vomiting came on, and finally death ensued.' 'The stomach was inflamed on its external surface, while its mucous coat was entirely disorganized and softened.' " The other case narrated by Dr. Stillé is that of Dr. Fountain, of Davenport, Iowa. This case is also given by Dr. Jacobi in his work on Diphtheria, from which I quote: "Fountain took half an ounce in a goblet of warm water at 8 A.M., on March 22d, 1861; free diuresis followed during the course of the day; it ceased entirely at 4 P.M. He looked fatigued and pale, but ate heartily at 7 P.M.; was attacked with purging, vomiting, and cramps after 8 P.M.; was in dangerous collapse at 9.30, with vomiting and purging, with intense pain and cramps, skin cold, with the hue of a person nearly asphyxiated. He rallied, but retained an exceedingly dusky appearance of the skin. Between 6 and 8 A.M. the following day he voided about 2 $\frac{3}{4}$ of black urine.

After this there was no secretion from the kidneys. When he called my attention to the urine,' writes Dr. Adler, of Davenport, 'he remarked that he feared the chlorate had seriously injured his kidneys.' Immediately after the choleraic symptoms returned with profound collapse, but he rallied again; the purging ceased and there was no further evacuation from the bowels during the six subsequent days of intense suffering. Vomiting and intense pain were incessant. He died just one week from the time of taking the chlorate.

"The autopsy revealed a general intense inflammation of the entire alimentary tract, from the stomach to the rectum; portions of the mucous membrane were destroyed, hanging in ragged shreds and patches, as if the mucous membrane had been macerated a long time in strong alkaline solution. The mucous membrane of the bladder gave a similar appearance. The bladder was empty. There were crystals of chlorate in the pelvis of the kidneys, and a large bulk of extravasated urine (apparently) under the capsule of one kidney."

Dr. Jacobi, in 1860, warned the profession against the indiscriminate use of potassium chlorate, he having found that single doses of half an ounce or six drachms produce "a sensation of heaviness and dragging in the lumbar region." And in his treatise on diphtheria—New York, 1880—he has collected a dozen cases, with a view to show the symptoms of poisoning and their accompanying pathological lesions.

Most of the literature on this subject being in the German language, I thought it worth while to make public the results of some recent experiments touching the matter. My objects are to repeat Dr. Jacobi's warning, and to show the cause of death.

I was comparatively ignorant of the subject until my interest in it was excited by the receipt from Dr. Wm. Maddren, of Brooklyn, late in December, 1882, of a specimen of urine voided by a patient who was poisoned by chlorate of potash.

A girl fifteen years old had been treated by Dr. Maddren for a slight angina, and given a gargle containing chlorate of potash in excess. The doctor instructed the mother to get five cents worth of the drug, and put it into four ounces of water, and to gargle with a teaspoonful every two hours. The instructions were perverted to read—Take a teaspoonful of the

solid salt every two hours. This was done, so that the patient took on Wednesday, December 27th, 600 grains. Wednesday night she became unconscious and cyanotic. Pulse 106, temperature 99° F., respiration 30 and shallow. No urine passed since morning.

Thursday, cyanotic. Pulse 100, temperature 99½°, respiration 25. Passed urine in small quantity, of which I received a sample. The patient died on Friday.

The specimen of urine interested me deeply. Its specific gravity was 1055; it was of a very dark bloody color, and opaque.

The microscope showed blood-corpuscles excessively crenated, and dark brown. They were gathered together in little masses throughout the field, and appeared agglutinated.

It was evident that we had here urine mixed with a decomposition product of hemoglobin, and placed before the spectroscope, the characteristic spectrum of methemoglobin presented itself.

Methemoglobin is a substance first described by Hoppe-Seyler, and is found in old extravasations, hydrocele and ovarian fluids, and can be prepared by adding to blood-coloring matter weak acids, and also, according to MacMunn, potassium permanganate.¹ This observer states that he has also found methemoglobin in the urine of acute desquamative nephritis, and in the nephritis of scarlet fever. Spectroscopically, it is distinguished by an absorption band in the orange at C, the two characteristic bands of oxy-hemoglobin still persisting.

Here then was a suggestion. The blood, the oxygen carrier, had been destroyed. Was this destruction effected by the potassium chlorate, or by mixture with the acid urine? According to Preyer,² and I have corroborated his statement, a solution of oxy-hemoglobin in normal, markedly acid human urine undergoes no change, even at the body temperature.

The effect of the addition of potassium chlorate, in either solution or powder, to blood, at a temperature below 60° F., is to decidedly intensify the red color. No other effect is produced. Before the spectroscope, the two bands of normal arterial blood are seen, and decomposition has certainly not taken place.

¹ The Spectroscope in Medicine. London, 1880, p. 99.

² Die Blutkrystalle. Jena, 1871, p. 93.

Gently warm the solution, however, and changes are immediately manifest. The color grows dark, and, by further investigation, we find the corpuscles shrivelled and colorless, and a new absorption band in the spectrum at C. That is to say, we have reproduced in the test tube the exact conditions found in the black urine voided by our poisoned patient.

Proceeding to experiment further, I injected, by means of a catheter, two ounces of a warm saturated solution of potassium chlorate into the stomach of a healthy rabbit, and stood by to see what would happen. The animal (it was not etherized) remained quiet, and for half an hour appeared comfortable. From this time there were evidences of discomfort, and at the expiration of two hours it suddenly expired in a convulsion.

On opening the abdomen I was greatly surprised to find the cavity flooded with the contents of the stomach, and, upon further examination, I discovered a perforation in the greater curvature of the stomach, about the size of a twenty-five-cent piece. The entire mucous membrane of the organ was more or less softened and congested, as was also that of the duodenum.

The blood-corpuscles were not affected, nor did the spectroscope give evidence of any decomposition of the coloring-matter. The animal had died from the local effects of the drug.

Another healthy rabbit, not etherized, received into the stomach two ounces of a cold saturated watery solution of potassium chlorate. At the expiration of an hour the animal manifested marked symptoms of dyspnea, and very soon the distress terminated in a fatal convulsion. Post-mortem examination revealed congestion of stomach and swelling of the mucous membrane. The red blood corpuscles were universally crenated and distorted, but still retained their coloring-matter. The blood color was not decomposed. This rabbit succumbed to asphyxia.

From what has been said, I think we are justified in drawing the following conclusions:

Chlorate of potash is capable of toxic effects, and should be prescribed and handled with care.

It acts, in large and concentrated doses, as a corrosive, and produces inflammation, softening, and perforation of mucous membranes. If the local action is sudden and severe, absorp-

tion does not take place rapidly enough to cause decomposition of the blood, and death under such circumstances may result from the primary effect on the stomach and intestines.

If administered in doses insufficient to cause immediate death from the above causes, gradual softening of the entire alimentary tract ensues, the coloring-matter of the blood is decomposed, and the red corpuscles are destroyed. A circulating medium, consisting of such detritus, soon ends the scene. Every vital function is checked, the organs, as shown by other observers, become filled and choked with blood debris, and they all alike but meagrely perform their part. The morbid appearances of the kidneys and other organs I regard as but secondary and inevitable conditions consequent upon the destruction of the blood. The victim dies of asphyxia.

INFANT FEEDING AND SUMMER COMPLAINT.

BY

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THE object of the little I have to say is not to propound any new thing pertaining to the nature, cause, prevention, or cure of the enteric diseases included under the popular name, summer complaint.

It is solely to emphasize a single factor in their production, under the belief that by giving it the prominence it deserves a rational way will readily suggest itself of meeting and repulsing this slaughterer of the innocents.

It is unnecessary for my present purpose to describe the various digestive disorders that are comprehended under the name of summer complaint.

Attention is simply called to the conditions that obtain in their causation. Text-books and the common experience of physicians give for answer:

Urban life, artificial food, summer heat.

Of course, each one of these three includes and entails a multitude of debilitating agencies, but in the main they cover the whole ground.

They make frightful havoc, and when we consider how, acting together, these allied forces of evil are multiplied and intensified in their maleficence, we can only wonder that so many escape their onslaught.

Moralists tell us the first step downward is the one to guard against, and this is true physically as well as morally.

This brings us to the pith, how to guard against indigestion. Answer, *avoid over-feeding*. To avoid wrong or improper feeding goes without the saying; medical books and medical heads are full of it, and as a result, infant foods of numberless designs and kinds are concocted, advertised and given, with various results.

Each physician in each case must, taking human milk as his model, get as near to nature as possible, and "fight it out on this line if it takes all summer" (and it generally does).

Over-feeding, it seems to me, in military parlance, is the key to the position. This, in my belief, is the bane of bottled children.

Look at it. The doctor is called to a case of summer diarrhea. He prescribes, and leaves instructions as to what food to give and how often, and adds "keep the child well aired, clean, cool, and quiet," and goes on his way, thinking he has been specific enough.

Now, what does the child's attendant do? That last injunction about keeping the child quiet makes a major impression, because this same quiet consorts with her own comfort. The child cries and must be quieted, and the ready bottle is its comforter. Through the day that other injunction about feeding only so often acts in a measure as a deterrent; but the long night comes, and the tired nurse or mother needs quiet, too, and now the bottle becomes a duplex comforter. Filled and refilled it is kept to the child's lips.

A stomach that has no rest gives up, or gives out.

A fundamental principle in the treatment of disorder of any organ is to give it a rest. In the case of digestive misdemeanors nothing is so effective as to starve the offending viscera into good behavior. And this plan, at first thought, so abhorrent to

the fond mother, or indulgent or indolent nurse, may be made feasible by a simple explanation. Tell her the child cries more from thirst than from hunger, that his wail, like the wail of the ancient mariner, is of "water, water everywhere, nor any drop to drink." Lay down inflexible rules about amount of food and times for feeding, but give her carte blanche to water the infant as often as it cries. Tell her hot weather induces perspirations, and that as surely and in the same way as waste makes wants, sweating makes thirst.

This sounds plausible, but the doctor of to-day is a sceptic and wants evidence. My experience is limited and my figures are few, but it is believed that they are significant if not convincing.

For twenty-one years I have been physician to the Rochester Orphan Asylum. Each of these years had witnessed deaths from enteric diseases until 1882.

In the early summer of that year I said to the matron, feed your babes but once in three hours during the day, but give them water to drink as often as they will take it.

The summer came and went, and when frosts appeared I congratulated her on the good results of our plan; not a child had died, and no serious case of diarrhoea had occurred.

"Yes," said she, "but it seemed cruel to feed these babes but three times a day."

She had actually carried out my instructions to the letter, as she had misunderstood them. Instead of once in three hours, she thought I had said three times a day!

On the following (that is, last) summer the plan was carried out, giving the infants food once in three or four hours, according to age, during the day, and an additional meal in the night if the child awoke and would not be quieted with a simple drink of water.

The same immunity from summer complaints ensued as in the previous year, with two exceptions, and these exceptions emphatically proved the rule.

They were children of tubercular parents, and because of scrofulous manifestations were removed to the hospital, then empty, in the upper story of the building. A nurse was detailed from the hired help. Soon these infants became dys-

peptic, intractable gastro-enteritis followed, and in a few days they died.

Inquiry elicited the fact that the newly made nurse had kept a bottle every night and all night in their mouths, for, as she declared, she could have no peace without. In legal parlance, it is submitted that a case is made. That is, that rest for the stomach may be obtained by recognizing thirst more than hunger as a summer want, and thus, by prolonging the intervals of feeding, preventing indigestion and its deadly train of attendants.

ABSTRACTS.

Prepared by J. FEWSMITH, JR., Newark, N. J.

1. **A. Napier: Purpura** (*Glasgow Med. Jour.* February, 1893).—A girl fourteen years old, on the day after a very tiresome walk, complained of such great weariness and pains in the legs that she had to be sent home from school. In the evening there was but slight constitutional disturbance, appetite and sleep good, tongue clean, pulse 85, temperature 37.9°. Both legs, however, were distinctly swollen and tense from the knee to the ankle, and the thighs for a short distance above the knee. All these parts were firm and hard, showing no depression after pressure by the finger, and extremely tender upon the slightest touch or movement. Scattered over the swollen parts appeared numerous purple-colored maculæ varying in size from the head of a pin to a cent, and not disappearing on pressure. These spots had come out during the afternoon without any feeling of heat or itching. The joints were not painful, the gums and mucous membranes were normal. On the following day the swelling in the legs was still present, but the tenderness much less. There were no new spots, but the old ones were darker. Urine, temperature, and pulse normal. In the course of the next two days all tenderness disappeared, and the spots became yellowish-green, as after a bruise. There appeared, however, symmetrically on both temples, a somewhat painful round swelling about an inch in diameter, also symmetrical swellings on both elbows posterior to the external condyles, yellowish-green in color, and tender. The back of the right hand was also swollen and tender, but not discolored. On the next day the left hand was also swollen and tender, while the spots on the legs were rapidly disappearing. After a few days all the symptoms of disease had disappeared, but it was some time before the patient recovered her full strength.

Napier remarks in connection with the case that it could not be considered a purpura simplex or purpura urticans because there was neither heat, itching nor vesiculation; nor could it be classed with purpura

rheumatica, because there was no swelling or pain in the joints, and the girl had no disposition to rheumatic trouble; while the healthy condition of the membranes and the absence of other hemorrhage spoke against the existence of scorbutus. The author was inclined to attribute the disease to a nervous origin, in consequence of exhaustion and over-exertion, especially with regard to the completely symmetrical character of the symptoms.

2. Stocks: Cardiac Disease with Purpura (*British Med. Jour.*, No. 1157).—In the Manchester Medical Association Mr. Stocks showed the photograph of a boy fourteen years old who had suffered for six years from marked aortic and mitral insufficiency, and in whom extensive subcutaneous hemorrhages occurred two weeks before his death. The upper half of his face from the lower border of the orbits to the boundary of the hair upon the brow was one great ecchymosis. Upon the rest of the face, the nose, ears, gums and other portions of the body were similar spots, some raised, colored either in streaks or homogeneously. The conjunctivæ below the greatly swollen lids were chemotic and sugillated. In the beginning of the purpuric eruption there was a slight hemorrhage from the bowels. Four days before death there was a further eruption of purpura spots, and three days later hæmoptysis and dyspnea. The urine remained free from albumen. The temperature varied from 38.5° to 39.5° C.

3. Tonge Smith: The Period of Incubation of Scarlet Fever (*Brit. Med. Jour.*, 1152).—From his experience of over 2000 cases of scarlet fever, observed within the last three and a half years in the London Fever Hospital, Dr. Tonge Smith has become convinced that the incubation of scarlet fever does not last more than three days. Accordingly, he does not extend the quarantine for persons who have been exposed to the infection beyond three days, and has never so far gone astray in following this rule. As ground for his opinion, so decidedly outspoken and so different from the usual views, he gives the details of twelve cases. Of these, seven were admitted to the scarlet fever division with the diagnosis of scarlet fever while actually two had rubeola, one had measles and four had no special disease. Although these patients were again immediately isolated, the characteristic symptoms of scarlet fever appeared in four of them in from sixty to seventy-two hours, and in two of them within three days. There were two further cases of nurses who had newly come in charge of the scarlet fever patients. One was attacked seventy-two hours, the other five days after entering the division. The other patients, in whose cases the source of infection was exactly known and who had been exposed to it only a short time, sickened on the third, fourth, fifth, and thirteenth days. From these latter cases the author does not conclude that there is a period of incubation of so and so many days, but that there is a possibility of remaining a certain time among scarlet fever patients without being infected. Thus he has observed that in well-ventilated rooms for scarlet fever cases, other patients who were in bed remained free from scarlet fever as long as three weeks, but were attacked by the disease three days after getting up and mingling with the scarlet fever patients. It was only in surgical and puerperal scarlatina that he had seen the shortest period of incubation, *i. e.*, twenty-four hours.

4. Longhurst: The Period of Infection of Scarlet Fever (*Lancet*, II., 1888).—Dr. Longhurst brought before the Clinical Association in London some new examples in proof of his opinions concerning the period of infection of scarlet fever. These cases show that the scarlatinal infection does not act upon all people with the same rapidity, and on the other hand, that it may be transported from patients in the very earliest stages and even during the period of incubation. The practical and important conclusion from this latter observation is that upon the outbreak of scarlet fever in one of a family, the other members should not be too quickly sent away to make new foci of infection, but the first patient should be isolated either in hospital or at home. Again he shows that the danger of transmission of the contagion during the period of desquamation is much less than during the early periods, and there is no danger in greatly shortening the usual long isolation of from two to three months.

In the discussion which followed, Dr. Broadbent remarked that the question of the transmission of scarlet fever in the pre-eruptions stadium was difficult to answer on account of the rarity of clearly single cases and the varying susceptibility of different individuals to the infection. He had had a case in his own experience where two children slept in the same bed until the appearance of the exanthem in one, and yet the other remained free from the disease. There was no doubt in his opinion of the transmission of measles in the pre-eruptions stadium. A number of speakers uttered decided warnings against any shortening of the period of isolation and brought up examples where transmission of the disease took place after six or seven weeks, and claimed that at least as long as the desquamation continued the disease was easily transmissible and that there should be a complete isolation for at least one month.

5. Broadbent: Hydrophobia (*Brit. Med. Jour.*, 1157).—Dr. Broadbent reported the three following cases in the London Clinical Association.

1. A boy twelve years old was admitted to hospital for convulsions which had already continued two days. These began with a loud, deep inspiration, followed immediately by a stiffening of the body and rapid rotatory movements of the head and loud sounds in the larynx. This lasted about three minutes, then the boy would sigh and complain of headache. Such attacks were brought on by the attempt to drink water, by the sight or sound of falling or running water, by the touch of cold objects or pressure over the heart. In the intervals the boy was perfectly conscious and his mind clear. His countenance was pale and anxious, temperature normal, pulse 108 and weak, respiration sighing. He said that he had once been bitten by a strange dog, and the scar found on his hand had a peculiar chancre-like induration. Amyl nitrite was first given, but without result, and the treatment then consisted of enemata of chloral hydrate 1.2; brandy, 30.0; beef tea, 60.0. These were given every three hours. He was admitted on February 25th, and by the 28th was so much better that the chloral was stopped—but the attacks then began again and the chloral had to be continued for some time. He left the hospital April 2d, and even up to that time the tones of the chapel organ had a peculiar effect on him, throwing him into great ex-

citement. Since there was nothing in the case or in the history of the boy pointing to a simulation of hystero-epileptic attacks, the author considers the case one of true hydrophobia, ending in cure.

2. A girl thirteen years old, always previously healthy, was bitten by a strange cat. One month later she was taken with nausea, fever, and a feeling of weakness in the arms, and by the next day she was so unmanageable and excited that she had to be brought to hospital. Then she became delirious. She could swallow water only with the greatest effort, and the attempt caused spasm of the pharynx and esophagus. In spite of the exhibition of chloral and bromide of potash, she grew steadily worse. Death from exhaustion followed about seventy-two hours after her admission, preceded by wild delirium, spitting of frothy mucus, and total inability to swallow. The autopsy showed congestion of the brain and the upper portion of the cord, particularly the floor of the fourth ventricle. In the cortical portion of the brain the vessels were dilated and there were minute extravasations.

3. A boy, thirteen years old, had been bitten by a pup three months old. The wound was cauterized within five minutes, and the cauterization repeated twice afterward. The dog was killed one week later. After five months, during which the boy had read and heard much of cases of madness, he began to have difficulty in swallowing, the attempt causing spasm, and at the same time a rich flow of saliva and froth from the mouth. On admission his restlessness was remarkable. He frequently sprang up saying he could not breathe, and was afraid to have any liquids near him. His face was red and distorted. His respiration was rapid (36), irregular, and sighing. He constantly cried and complained of pain in epigastrium. In the night he became more restless in spite of chloral 1.2 by enema. He pounded his head on the wall, grasped his neck, and cried that he was suffocating. The respiration was labored, pulse very small, and from time to time there were spasms of neck and arms. In spite of a second enema of chloral the patient got so much worse that he had to be put in a strait-jacket, and it was only after a hypodermic of chloral (1.2) that he, toward morning, got about three hours' sleep. In the morning he was quiet, peevish, but free from pain. Pulse 130, small and weak; temperature 41.6°C . The urine, by catheter, had specific gravity of 1.030 and was full of urates, but contained neither albumen nor sugar. Although there were no more severe convulsions, the fever continued, and in forty-three hours after admission the boy died of exhaustion. At the autopsy, held three hours P. M., there was very great rigor mortis, marked congestion of the meninges and cortical portion of the brain. The membrane over the pons and bulbous was opaque, milky, and hyperemic. There was no increase of fluid in the ventricles.

The author does not consider this a case of true hydrophobia. Such apparent cases of hydrophobia give rise to the suspicion that the nerve centres of higher grade and higher functions are able to so control those of lower grade as to cause reflex convulsions of the nature of hydrophobia.

6. Bristowe: *Hydrophobia* (*Brit. Med. Jour.*, 1164). — The case reported by Dr. Bristowe may be added to those of Dr. Broadbent. A girl of seven years had been bitten by a dog. The wound was

superficial and had been cauterized within an hour. The dog had been immediately killed. Two months after this she began to have pain in the scar of the bite, and then pain in the abdomen and in swallowing. On the first day food was still taken. On the evening of the second day she became delirious. When brought into hospital she was pale and anxious-looking, and spit out a good deal of saliva. She could swallow water very slowly without very great effort and without spasm. The principal pain was in the throat. Respiration rapid; pulse 180; temperature 40.0° C. After a warm bath she became worse, struck, pushed, and tried to bite. There was some singultus and ructus and spitting of mucus. The urine contained one-third albumen, hyaline and granular casts, free epithelium, and crystals of uric acid and oxalate of lime. Ten minutes before death she became a little quiet, then there were twitchings in face and legs, she became suddenly livid, bent forward with the head thrown back, and died, five hours after admission.

The autopsy showed congestion of brain and cord.

7. Jackson: A Case of Spondylitis with Paraplegia Treated by Trephining (*Brit. Med. Jour.*, 1165).—A healthy-looking boy, with no family history of scrofula, was seized within four months with a paralysis first of the left, and then of the right leg, which rendered him completely helpless. On admission there was paralysis, emaciation, and flabbiness of both lower extremities. Sensation was normal, the faradic excitability increased. Defecation took place naturally, but urination was very difficult. No swelling or tenderness of the back could be discovered. Within two months he became so much worse that he could not turn himself in bed, and had incontinentia alvi et urinæ. In place of the flabbiness of the lower extremities, the now still more atrophic muscles were in a condition of tonic contraction, and every attempt to extend the strongly bent toes, the ankle, or the knee-joints caused him violent pain. The sensibility had greatly diminished. Three months after admission there appeared a swelling over the lower vertebræ, and Dr. Jackson determined to lessen the pressure upon the spinal cord by opening the spinal canal, in the expectation of finding an abscess. The opening was made under antiseptic precautions, but no pus was found, and through the hole in the bone could be seen only the spinal cord with its unbroken membranes. The wound healed within four weeks with only a slight rise of fever during the first days. Even one week after the operation the boy was able to voluntarily pass his urine and there was no more incontinentia alvi. The painful tonic contractions in the legs yielded. The patient was able again to move his legs, although the faradic excitability was considerably weakened. Sensibility became normal in all parts of both lower extremities.

8. St. Germain: Malignant Tumors in Childhood (*Rev. Mens. des Mal. de l'enf.*, 1883).—Before the thesis of Duzan (1876), there was no collected report of the forms of cancer occurring in childhood. Lebert (1851) mentions them in his work upon malignant diseases, but out of 471 cases notes only 15 as belonging to childhood. This, however, directed the attention of authors to the subject, and Duzan was able to collect 182 cases. According to Dr. von St. Germain, the form of cancer best studied in France is cancer of the dura mater (Aran). Next comes cancer of the eye (Giraldes et Brière). According to Henoch

(Dictionnaire encyclopédique), cancers may be divided as follows, according to the age: Out of 100 cases, 52 come between birth and the fourth year. There is an increase between the sixth and seventh years, and a great increase between the fifteenth and seventeenth years, just at the limit of childhood. Cancer of the tonsils first occurs at this latter age. Duzan cites several cases of fetal forms of cancer. The proportion of boys to girls is about 60 to 32.

St. Germain, in closing, reports the following cases from his clinic:

1. Embryonic sarcoma of the forehead.
2. Embryonic sarcoma of the left forearm. Cauterization and cure, with loss of radius.
3. Osteo-sarcoma of the left fibula. Cure.
4. Malignant fibro-plastic tumor of the vulva and mons veneris in a child five years old. Tumor removed with the écraseur. Relapse after one month. Death from cachexia.
5. Villous carcinoma of the bladder. Diphtheria. Death.

St. Germain remarks that in children it is rare to find the cancerous cachexia. The discoloration of the skin so characteristic of cancer in adults is entirely wanting in children. He also remarks upon the extremely rapid growth of the tumors in children.

9. Simon: For Cough (*Jour. de Méd. de Paris*, May 12th, 1883).—Jules Simon recommends for coughs:

Tr. aconite,

Tr. belladonnæ.....āā f 3 i.

M. Sig. Five drops morning and evening, to be increased by one drop daily up to twenty drops; or

Syrup belladonnæ,

Syrup codeine,

Syrup tolu.....āā f 3 ss.

M. Sig. Teaspoonful morning and evening, to be gradually increased.

The belladonna diminishes the secretion of the bronchial mucous membrane. The aconite decreases the nervous excitability of the children and the spasmodic symptoms.

10. Fournier: Changes in the Teeth Caused by Syphilis (*La Sem. Méd.*, May, 1883).—In regard to the formation of the teeth syphilis causes:

1. A delay in their appearance. Demarquay relates the case of a child of syphilitic parents who had no teeth when four and a half years old.

2. Changes in the structure of the teeth, as follows:

(a) Erosions, of which the most important is the nail-mark as first described by Hutchinson. This erosion is crescentic and occurs especially on the edge of the upper middle incisors. Then comes the furrows, the steps, the honeycomb, and the serrate erosions.

(b) Microdontismus or dwarfed formation of the teeth.

(c) Amorphismus, in which the teeth of each single group lose their characteristic marks.

(d) The easy destructibility, rapid using up, and early falling out of the teeth.

11. Parrot: The Relation of Hereditary Syphilis to Rachitis (*Sem.*

méd., Feb., 1883).—The syphilitic taint may show in children either upon the teeth, the skin, the mucous membranes, the tongue, or the bones. The bones, however, are organs which are earliest and most easily influenced by disease. We can upon the skeleton note a series of changes from the first moment of intrauterine life on to the second dentition, which, without any question, show the transition of syphilis into the so-called classic rachitis. Three types are especially marked upon the skeleton: the hard osteophytes, the gelatinous atrophy, and the spongy type. The first variety occur especially on the inner surface of the tibia and the posterior surface of the humerus, and appear between the first and sixth week. They often more than double the volume of the bone. After this period, this form of change is not again seen, but the syphilis now manifests itself through the occurrence of gelatinous changes in the peripheral portions of the diaphyses, the color of these varying from cherry-red to yellow. These changes cause atrophies and perforations of the cranial bones and fractures of the bones of the extremities. The osseous changes of the spongy type differ in no way from the rachitic processes.

Parrot recognizes as the fundamental cause of rachitis only syphilis. As accidental causes he mentions descent from too young or too old parents, too long continued or too short a period of nursing, the solution of the lime salts by an over-abundant amount of acid in the digestive juices, intestinal catarrhs, dampness, cold, etc.

12. Desplats: Salicylate of Bismuth in Typhoid Fever (*Bulletin gén. de Thérap.*, June, 1883).—After having carefully tried the usual antipyretic remedies, principally of the aromatic class, namely, carbolic acid, salicylic acid, salicylate of soda, and resorcine, in a series of cases of typhoid fever, Dr. Henry Desplats, clinical professor at Lille, began to use the salicylate of bismuth, in the theoretical hope that the bismuth might so coat over the intestines that the too rapid elimination of the acid by the kidneys might be prevented and its antipyretic action prolonged. He gave the remedy in twenty cases. The dose varied from one to two grams, and was repeated about five times a day. The remedy was given either in gummy syrup or in wafers or troches. It was well borne in most cases, and when it occasioned a little nausea, this was easily relieved by giving seltzer water with it.

In regard to its action, the author distinguishes immediate and remote effects. The immediate effect is like that of carbolic acid, but with a slighter fall of temperature. Sweating frequently precedes the fall of temperature. In contrast to the carbolic acid effect, the consequent rise of temperature is much less rapid. Frequently the salicylate of bismuth appeared to work almost abortively, and the patient rapidly (immediately, *unmittelbar*) became convalescent [too good to be true. J. F.].

In regard to the remote effects, the author shows the effect of the drug on the whole fever curve. The antipyretic effect seems to last some time. Frequently the temperature continues to fall in the morning, even when none of the remedy has been given during the night. Details of cases follow.

[I have lately used this remedy in the case of a young girl with typhoid fever and severe diarrhea and tympanites. It was given on account of the diarrhea; it had, however, but very little effect in that direction, but

a most decided effect in reducing and keeping down the temperature. I was particularly pleased with its action. J. F.]

13. Semmola: Glycerin in Acute Fevers (*Bull. Gén. de Thérap.*, June, 1883).—Doctor Mariano Semmola, of Naples, has given up the use of anti-pyretics in typhoid fevers, believing that in order to have any action against the disease they must be given in quantities which are otherwise hurtful, and he also restricts the use of alcohol to those cases in which there is great weakness of the heart. For these two kinds of remedies he substitutes a remedy to save tissue waste, chemically pure glycerin, given in the following form: \mathcal{R} Glycerini purissimi, 30.0–50.0; acid. citric., 2.0; aquæ dest., 500.0. *M. Sig.* Two (2) tablespoonfuls hourly. He claims that the daily excretion of urine is greatly decreased in this way, that the patient loses his thirst, and retains a remarkable amount of strength.

14 Collan: Prolapse of the Processus Vermiformis through a Penetrating Wound of Abdomen (*Abst. in Jahrbuch from Scandinavian literature*).—A boy, six and one-half years old, fell on an open knife which he had in his pocket, and it penetrated his abdomen. This was on February 8th, 1882. Although a portion of intestine protruded, yet he walked some distance home and concealed his wound until those around him noticed that in walking he dragged one leg a little. On the evening after the wound he vomited once. After this it did not occur again, and he had good appetite, good evacuations, and no trouble from his wound even during the trip made to the doctor. On February 12th, C. found a perforating wound on the right side of the abdomen, between the umbilicus and the anterior superior spine of the ilium. $6\frac{3}{4}$ cm. from the umbilicus, through which a portion of the bowel protruded. This was about 5 cm. long, $2\frac{1}{2}$ cm. broad, somewhat flattened and constricted at the base. Most of it was grayish-colored; here and there were spots of granulation. There was no redness or tenderness around the wound. The abdomen was not distended. Immediately at the wound there was a slight swelling, rounded on its lower side and having the contour of the cecum. Above this swelling, percussion gave a clear, tympanitic sound. C. sewed the base of the protruding processus vermiformis together with the wound, in order to get up an adhesive inflammation. The condition of the patient continued excellent, and the processus vermiformis became covered with granulations. On February 21st, C. tied a strong catgut ligature about the base of the protruding part, dividing it in halves. This caused the patient no pain nor any further disturbance. As the father was in haste to get the boy home, C. tightened the ligature only slightly and let him go. By the end of March he heard that the protruding portion had shrivelled to one-half inch in length.

The remarkable thing in the case is that there were almost no disturbances of the intestinal tract and that the wounded parietal peritoneum was not at all inflamed. This latter is probably to be explained by the fact that the protruding processus vermiformis so promptly and so fully filled the wound that there was absolutely no entrance of air. But it is still more remarkable that there was no extension of inflammation on the visceral peritoneum from the part which was exposed and inflamed.

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ORIGINAL COMMUNICATIONS.

NOTES UPON MALPOSITIONS OF THE UTERUS.

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(With four woodcuts.)

A WOMAN of thirty, virgin, presents herself complaining of painful and profuse menstrual flow, disability in walking or standing, pain in the back and legs, weight and heat in the pelvis, constipation and difficulty in defecation, irritable bladder and leucorrhea.

The touch reveals the os uteri within an inch of the introitus, pressed up against the superior wall of the vagina, its superior face flattened, and a border grown sharp and angular like the internal face of two opposing toes. Passing under the protruding posterior lip, the finger encounters the retroflexed uterus in Douglas' space enlarged and somewhat tender to touch.

The history is of long standing and slowly aggravating symptoms, and the physical changes exactly correspond to this idea.

Whatever may be the order of priority or predominance, it is certain that the vital disturbances will continue as long as the mechanical derangement continues.

It is equally true that medicinal and hygienic means addressed to the former, will never redress the latter. And yet there always have been, and are now, men of good and great repute who declaim against the use of pessaries as monstrous, uncleanly, infective, irritant, nugatory, or dangerous. Such is the language of Prof. T. Matthews Duncan, late President of the British Medical Association, a very able man, although somewhat slow to perceive the value of Sims' speculum or the occasion for and value of uterine trachelorrhaphy.

In 1864, the late Dr. A. K. Gardner published a paper in which were figured one hundred and twenty-eight different forms of pessaries, some of them curiosities of abomination, and within a year or two, distinguished men have brought out instruments which, to the writer, seem to deserve similar judgment.

It is obvious, therefore, that there has been much misspent labor in this direction, for want, as it seems to this writer, of appreciation of a few principles familiar enough to some, but evidently not to all gynecologists, or to the body of the profession at large. It is the purpose of this brief paper to state and illustrate a few of these.

The antithesis to a malposition of the uterus is a normal position of the uterus. The uterus has not *a* normal, but *many* normal positions. The norm of the uterus is to be a very movable organ, the most movable of any of the pelvic or abdominal viscera. Its distinctively normal position is that toward which it returns from all its excursions, and in which it rests in the equilibrium of all disturbing forces.

More than half of the uterine body stands free in the cavity of the pelvis, responding to every depression of the diaphragm, to all movements of the intestines, and to all forces applied to the abdominal walls.

The centre of motion in the uterus is near the junction of the cervix with the body, at the level of the os internum. The peritoneal folds known as the utero-pubic and utero-sacral ligaments pass around the uterine neck on either side. Between them the uterus is swung, steadied laterally by the broad and round ligaments. The utero-pubic ligament having a lower point of origin resists the excess of upward movement, and the utero-sacral having a higher point of origin resists a downward

displacement; but an upward and downward movement, and a forward and backward movement, each amounting to between two and three inches, is certainly physiological. Furthermore, upon a bilateral or transverse axis of the pelvis passing through the above-named centre of motion, the organ revolves in such a way that the fundus may approach the two utero sacral ligaments and Douglas' space, which they inclose, in backward reclination, or it may approach the two utero-pubic ligaments and the superior surface of the bladder which they inclose.

The normal unimpregnated uterus being a short tube with thick walls and a small lumen, does not readily take on a new bending on itself. Consequently, when the fundus is moving through this last-named comparatively large arc, the intravaginal portion moves in the *opposite* direction through an arc as much smaller as the proportionate distance between the os and the transverse axis is less than between the superior border of the fundus and the transverse axis. This would be strictly true if the axis of the whole uterine body were one and the same straight line. It is not such; it is a line curving forward in such a way that while the axis of that portion which is between the os externum and the os internum lies nearly parallel to and a little behind the axis of the superior strait, the axis of that portion which is between the os internum and the superior border of the fundus crosses the axis of the superior strait, making with it a superior angle of from 25 to 35.

Reference to the plate annexed will make these statements more intelligible. In the diagram, A B is the plane of the vagina; C D, the plane of the superior strait, and E F, its axis.

This sketch represents the cervix entering the vagina perpendicularly, and the fundus as making an angle of about 120° with the axis of the cervix.

The latest writers on gynecology, Fritsch, Hart and Barbour as well as the authors who have studied the special subject exhaustively by the aid of frozen sections and methods of precision as well as by clinical observations specially directed to the solution of the question, Pirogroff, Braune, Scanzoni, Aran, Panas, Martin and Schultze agree in giving to the body of the womb a position lower in the pelvis. Hart and Barbour in particular declare that the fundus normally lies "as near to the upper wall of the vagina as the degree of distention of the

bladder will allow, and that the intra-abdominal pressure of the viscera normally falls upon the *posterior* surface of the fundus," which is normally and positively anteфлекed to such a degree that the axis of the cervix and the axis of the body form an angle less than a right angle, the cavity of the uterus following a curved but not an angulated line.

My own clinical observation of twenty years, supplemented by frequent post-mortem observations, does not lead me to accept these extreme views, and in this particular corresponds more nearly with those of American investigators, notably Foster, Van de Warker, and Ranney.

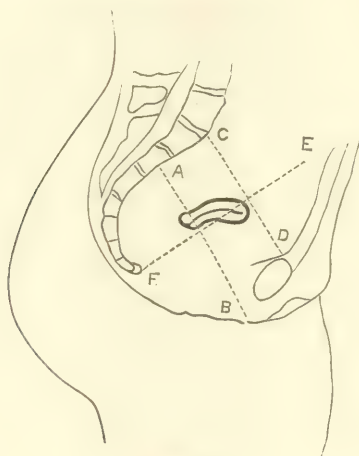


FIG. 1.

It seems to me that there are very considerable mechanical objections to this idea of a uterus recumbent on the superior wall of the vagina. The wave of intra-abdominal pressure in respiration, following the vertical axis of the trunk, impinges first upon the anterior abdominal wall somewhere between the umbilicus and the crest of the pubis, but nearer to the latter. Thence it is deflected downward and backward into the cavity of the true pelvis. When the perineum is drawn back by Sims' speculum, the anterior wall of the vagina is seen to move in correspondence with the respiratory impulse. The wave enters the vesico-uterine cul-de-sac, which it depresses. In much less degree does it appear to depress the uterus. It would seem probable that the uterus should be placed in the

best position to resist this force which is often a very powerful one. It would not suit this purpose that this impulse should fall directly upon the broadest surface, the posterior, and upon the long arm of a bent lever. But in the position given in the diagram it would present to this wave the crest of its thinnest superior border and the maximum resistance of its longitudinal fibres. The wave would be divided, a portion, probably the greater portion, would strike upon the angle where the bladder meets the uterus and where we see the maximum movement; and the remainder, gliding down the posterior wall of the uterus, would be spent upon the rectum. Both would by repercussion from the pelvic floor tend to keep the womb steady in the middle of the pelvis. But it is impossible to resist the concurrent testimony of so many competent observers to the effect that the normal ante flexion is far more than is shown in all but the most recent text-books, and my own observation fully supports this. Among the more recent contributions to the literature of the subject are those of Vedeler of Christiania, Norway.¹

This observer seems to have carefully studied the subject, examining a great many women of all physiological classes, and the sound as well as the sick.

In making his class of cases of ante flexion, he includes only such as, according to hitherto current ideas (which make the uterus nearly straight and erect at an angle approaching a right angle with the vagina), would be considered cases of displacement.

In 3,012 women he finds—

Virgins,	466.	With ante flexion,	322 = 71%.
Nulliparæ,	749.	“	495 = 66%.
2-3 months preg.	331.	“	266 = 80%.
Parous,	1,465.	“	565 = 37%.

Classified as those complaining of symptoms of displacement and those not so complaining—

Virgins:	ante flexion present in	71% of sound and 60% of sick.
Nulliparæ:	“	“ 71% “ 56% “
Parous:	“	“ 37% “ 38% “

He thus finds that advancing age and the changes conse-

¹ Archiv für Gynäkologie, Band xix., Part ii.

quent upon parturition and disease are attended by a rising of the fundus, and a straightening or a backward bending of the uterus as a whole.

Anteflexion, then, being constant before puberty, general in pregnancy, and present in two-thirds or more of all sound women, is the normal state. The treatment of moderate anteflexion by mechanical means *is uncalled for*, and it is a matter of general experience that it has hitherto been very unsatisfactory.

The proposition may be made more general, viz., any particular variation in the axis of the womb being found to be present in about the same proportion in the well and in the complaining, cannot be the cause of the symptoms, which must be sought elsewhere. I believe that this cause exists in *descent* of the uterus to a lower level in the pelvis, in the retroversion and retroflexion which generally attend this descent, in the approach of the womb to the vulva, and the congestion and friction which are entailed by these changes of position.

These changes may be amenable to very simple mechanical treatment which for me has become successful in proportion as my ideas of the pathology became thus simplified. Before discussing these, certain points in the anatomy and pathology of the pelvic organs, which go far to settle the question, when and how far mechanical treatment is available, are to be considered.

The upper portion of the womb stands free in the cavity of the abdomen, the lower portion is free in the vagina, but the middle or supravaginal cervix is closely invested. The peritoneum is stretched as a roof above it, the vaginal vault as a floor beneath. At this level arrive the great uterine arteries, curling through the tissues; hence depart the uterine veins which discharge the plexus that, like a cloak, enfolds the whole organ. The uterine nerves enter here, and here are the attachments of the sacral and pubic ligaments which limit the downward and upward movement; here is the web of connective tissue which binds it to the bladder in front and to the rectum behind, and here is the collar of circular fibres into which the whole system of oblique and vertical muscular fibres are inserted. Within, the lining membrane is spread over a series of corrugated folds, underset with a great body of muciparous

glands. From this area, as a centre of vital action, proceeds the secondary development of the organ as puberty advances. Like the diaphysis of the long bones, it throws off the vaginal portion downward and the fundal portion upward.

Within this area arise the greatest portion of the inflammatory affections of the womb; the endothelial catarrhs and the parametric exudations. The latter sometimes push up the floor of Douglas' cul-de-sac until its cavity is effaced, and sometimes they coarct and swell the utero-sacral ligaments until entrance to it is nearly shut off. The cervix being thus fixed, the fundus is pushed backward or forward, according as the effusion is greater before or behind the womb. Often *both ends* of the womb are pushed downward and forward, the middle portion being retracted toward the promontory. The cavity then becomes angulated, and a spastic action about the os internum is developed. When the congestion subsides, fibrillation of the exudate ensues, a brawny infiltration remains, and pending its absorption the uterus remains fixed in the direction which it has acquired.

The condition of the tissues just above the vaginal vault thus becomes an important field of study in all gynecological cases, and a careful appreciation of it by digital examination should precede every attempt to apply force in these regions. The most practised judgment does not always protect the operator from a serious mistake. Forcible retractions by the speculum or a bimanual examination, or the drawing down of the uterus to straighten the canal for the passage of the sound, or any other ordinarily innocent manœuvre, when untimely made, may rekindle a declining or exaggerate a commencing cellulitis.

Of equal importance is it to correctly appreciate the condition of the tissues beneath the mucous membrane of the upper portion of the vagina, for these are continuous with, and share in all the morbid processes which exist around the cervix. As the uterus in pregnancy undergoes a progressive development, to accommodate its growing burden, so the vagina undergoes a similar development to enable it to transmit the fetus; and both must undergo a *parallel retrograde involution* to the normal symmetry of the non-puerperal state.

If, as often happens, these processes are incompletely accom-

plished, relaxation and loss of elasticity in the uterine body and in the pelvic floor results; in the former it is most marked at the point of greatest vital activity, which, as before said, is at or about the os internum. Not only does the vaginal plane descend, but it becomes corrugated and prolapsed, particularly so if the perineal body has been overstretched or torn. The vagina is shortened as well as rendered more horizontal. From increased weight above and impaired support beneath, the uterus descends until it reaches a more or less fixed resistance, then the intra-abdominal pressure continuing, and further descent for the time restrained, it bends at the junction of the body and the cervix. Ordinarily, and unless there be exudations or adhesions in the vesico-uterine cul-de-sac, it bends *backward*.

When this bending has gone so far as to expose the anterior face of the uterine body to the wave of pressure reflected from the anterior abdominal wall, the bending is rapidly accelerated, and is limited only by the resistance, increasing from compression, in the tissues beneath.

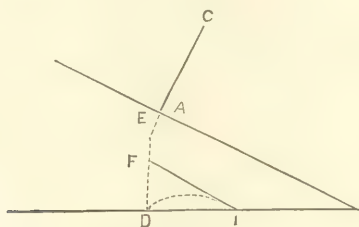


FIG. 2.

The order is: Descent, retroversion, retroflexion, then further: Descent, retroversion, retroflexion. *Descent is the prime factor.* These points are illustrated by the annexed mechanical diagram, in which A B is the plane of the vagina, and A C the axis of the lower part of the womb. B being fixed at the pubic arch, it is obvious that, as the plane descends, C tends to become vertical, and when A reaches D, C will be found at E.

But A, as it descends, moves forward to I, and when it reaches I, C will be found at F if there were no retroflexion, but in fact is found at D. In advanced cases the fundus falls between the utero-sacral ligaments, rendered tense by descent,

and as both the uterine and spermatic veins issue on the lateral borders of the womb, they are not only bent but compressed, and the return of blood so impeded. Thus it is observed that a greater degree of congestion and swelling attends an aggravated degree of retroflexion than an equal amount of ante-flexion.

Subjective symptoms depend upon the congestion and the tension rather than upon version and flexion.

It follows from what has been said that anteversion and ante-flexion rarely require or admit of mechanical treatment; and that the treatment of retroversion and retroflexion should begin with *the prime factor, descent*, and that to restore the tension and direction of the vaginal plane is the first desideratum.

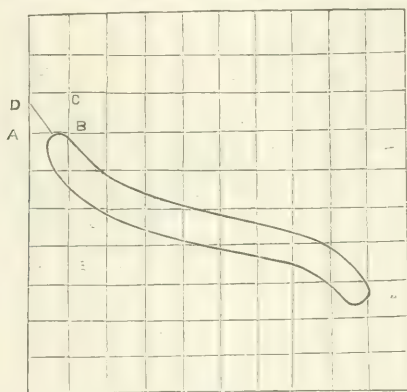


FIG. 3.

After trial of most instruments which have been given to the profession within the last twenty-five years, I find that in the great majority of cases the above indications are best answered by the form commonly known as the Albert Smith pessary, a ring compressed to the form of an obtuse wedge, the lower or exterior end being bent to a downward and the upper or anterior end to an upward curve. Several sizes, varying from two and one-half to four inches in length, are found in the instrument-shops. Many of these are far too much curved, and many have the arms or bars of so slender a circumference that they are easily imbedded in the tissues, which are thereby grooved, partially strangulated, and fretted even to ulceration. The best are those made for Dr. Emmet, in which the diameter of the bar is about one centimetre. As Dr. E. forcibly says, the

difference in the comfort of these instruments compared with those of slender make is the difference between riding on a fence-rail and on a broad saddle. A profile of the instrument, by itself, and in position, is shown in Figures 3 and 4.

It will be observed that the lower end finds its fulcrum in the comparatively rigid tissues of the urethra and through them on the inferior border of the pubis, and that the upper end presses in the direction B D against the wall of Douglas' space, and not in the line C D or against the uterus itself. It is in the fulness and firmness of the connective tissue which surrounds the vagina that the uterus finds much of its support,



FIG. 4.

and it is to distribute and equalize the pressure on this connective tissue which it is the object and the effect of the instrument to secure. *In all cases the womb must be repositioned before the instrument is applied.* It will then retain it in the correct position by imitating the action of the utero-sacral ligaments in *drawing the cervix upward and backward*, and thus rendering tense the utero-pubic ligaments, which *draw the fundus downward and forward.*

Even in the virgin or narrow vagina it is never necessary to use an instrument which is worn with pain.

If a suitable size be chosen and it be well lubricated, gentle and continuous pressure will carry it through an introitus which could not be passed by sudden force without a rupture. It should be entirely covered in by the collapsing vagina, because any mucous membrane exposed to the air degenerates.

It should be freely movable by the fingers, should be often removed, and in most cases the patient can be taught to do this for herself. Several of mine remove it at night and replace it in the morning before rising. Thus it does not impair marital relations and does not produce atrophy of the vaginal muscles by continuous extension. These advantages cannot belong to instruments of more complex outline.

The ordinary material is vulcanite. Some years since, Messrs. Otto & Son made for me a core of soft copper wire, covering it with celluloid. These were just as clean, smooth, and permanent as vulcanite, somewhat softer of surface and flexible by pressure only. They have not been popular with the instrument-makers, as it is said they become brittle by keeping. They never do so when in use, and I think I have lost more vulcanite instruments by mischance in heating than celluloid by breaking.

Almost all instruments for backward displacements are modifications of the original "closed lever" of Dr. Hodge. And most of them have the lower end resting against the internal surface of the os pubis. This gives a greater power of extension, but with more than compensating disadvantages. It disturbs the normal vaginal plane by pressing up the anterior wall, is apt to cause discomfort to the bladder and urethra, and being fixed cannot accommodate itself to the varying fulness of the tissues, and is therefore more likely to produce undue pressure. To sweep from the face of the os pubis to the posterior vaginal commissure, a curve approaching that of a semi-circle is required. The convexity of this curve depresses the vaginal floor too much. Often the curve is exaggerated with the purpose of bringing the upper bar to bear upon the posterior surface of the cervix. I have not found any advantage from this. Generally it has irritated the womb, and furnished a fulcrum *over which a more marked retroflexion ensued*. Still less have I liked the instrument known as Cutter's. It makes a fulcrum of the perineal body, retracts it, opens the vulva, does not keep its place on the posterior commissure, but settles down and presses the cervix forward.

It would be an invidious and unnecessary task to point out the disadvantages of various instruments.

As before said, Emmet's variety of Albert Smith's modifica-

tion of Hodge seems to me to embody the correct principles of treatment of posterior displacements, and is clinically far the most satisfactory.

When anteversion and anteflexion are combined in such degree as to require treatment, a condition for me not infrequent, the mechanical indication is to treat *the anteversion only*, by drawing the cervix forward in the vagina, trusting to the rigidity of the womb to elevate the fundus sufficiently. For this purpose, I commonly employ Fowler's pessary, which draws forward and fixes the intravaginal portion in its cup, while the broad and flat anterior plane furnishes a rigid floor to the base of the bladder and a fulcrum from which it can exercise its elevating force. This instrument, which incloses the entire intravaginal portion and fills the fornix closely, rendering the womb steady and preventing friction upon the neighboring parts, is of great comfort to an irritable cervix and a good support for a swollen and prolapsing ovary.

INVERSION OF THE CERVICAL STUMP IN PORRO'S OPERATION.

BY

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It is clear from the numerous and different methods devised and still practised for the management of the stump (pedicle) in the performance of the Porro and Porro-Müller operations, that no definite settlement as to the best method of treatment has yet been reached. It may therefore be pardonable, in one who confesses that he has had no practical experience with the operation, to suggest a method that has not, so far as I am aware, been either tried or mentioned.

What we want is, a method that shall (1) secure against hemorrhage during and after the operation; (2) prevent leakage of subsequent discharge from the stump into the peritoneal cavity; and (3) admit of perfect drainage and complete antiseptic cleanliness during reparation of the wounded surfaces.

To this end, I beg to propose *inversion of the cervical stump*, by which its incised surface is of course turned into the vagina, the positional relations of the parts being, afterwards, in about the same condition as they are after amputation of the womb when practised for irreducible chronic inversion. The number of recoveries, however, from this last-named operation, cannot of course be taken as any test of the value of the method I propose for the Porro operation, the circumstances and conditions attending the two operations being so widely different.

The advantages of the inversion treatment of the pedicle (if it be found practicable) are, apparently, these:

1st. The abdominal incision may be more securely and completely closed, with a better hope of immediate union. In the absence of any projecting pedicle at the site of the abdominal incision, healing of its entire length by the "first intention" would probably occur.

2d. The cervical inversion process would allow perfect drainage from, and antiseptic cleanliness of the stump, *per vaginam*, without any exposure of the wounded surface to the atmosphere.

3d. The patency of the cervical canal (which could be easily maintained after the operation), would allow complete drainage from the abdominal cavity, or even antiseptic irrigation of the peritoneal cavity, were this latter deemed desirable.

4th. Should secondary bleeding from the stump occur at any time, the application of styptic iron, or of the actual cautery, or of a styptic tampon¹ (?), etc., might still be practicable by the use of a vaginal speculum, and without any disturbance of the abdominal wound or invasion of the peritoneal cavity.

In short, by this method (I proviso again, if found practicable), we should gain all the advantages usually accruing where the pedicle is "kept out," and avoid all the damages to be feared as a consequence of its being "dropped in." (See Robert P. Harris, *Am. Jl. Med. Sci.*, Oct., 1883, p. 438.)

Furthermore, in the usual method of operating (the stump being "kept out"), the wounded surface of the cervix is *above*

¹Should a tampon be used to act *mechanically*, there would be danger of effused blood, etc., regurgitating through the cervical canal into the peritoneum, a danger from which a smaller *styptic* tampon would be perhaps not altogether free.

the peritoneum (the patient lying upon her back), and all discharges from the granulating wound tend to drain by *gravity* into the peritoneal cavity; by the cervical inversion method, on the contrary, the granulating stump, with its patent cervical canal, would be *below* everything else (as every drainage sewer ought to be), and would thus constitute a ready outlet for any fluid accumulation in the peritoneum, as well as prevent any discharge from the cervix into it.

(NOTE.—It must, of course, be understood, that the *kind* of inversion here referred to is *not* that of turning the womb (or rather its cervical stump) *upside-down*, through an opening made in Douglas' cul-de-sac, as is practised by Schroeder, Fritsch, and others, in the operation of colpo-hysterectomy, but in turning it *inside-out*.)

As to the practicability of the proceeding, while theoretically, in view of the dilated or dilatable condition of the os and cervix uteri at the end of pregnancy, inversion of the cervical stump after amputation may be considered easy, it must, on the other hand, be remarked, that the proceeding is a new one, with which nobody is familiar, and therefore difficulties that experience may demonstrate ought, if possible, to be anticipated and provided for. Hence the advisability of first executing the inversion process on a recently dead and *deformed* puerperal woman. It is evident that introduction of the hand into the vagina, and its freedom of movement therein afterwards, may be seriously interfered with by such degrees of pelvic deformity as may sometimes be present when the Porro operation is performed. The use of a vaginal speculum, and applications to the inverted stump *per vaginam*, unless the pedicle could be drawn down to near the vulva, may be curtailed in the same manner.

Before, however, attempting the inversion, either with or without introducing a hand into the vagina,¹ it is necessary to decide what shall be done with the stump while it remains *in situ*, immediately after removal of the uterus, and before the constricting wire or rubber band have been taken off. The various hemostatic remedies—ligation of severed arteries, styp-

¹ In either case the vagina itself should have been previously rendered antiseptically clean, free from all leucorrheal, bloody, and amniotic fluids, and it would be better that the hand operating in the vagina should not thereafter be allowed to manipulate the peritoneal surfaces.

tie iron, actual cautery, etc.—commonly resorted to when the pedicle is “dropped in,” would, of course, be available, but perhaps not without some risk that the manipulation required for inverting the stump would disturb coagula, eschars, etc., and so occasion renewed bleeding. Possibly it would be better, instead of, or in addition to, one or other of the means just stated, to pass a good number of silk ligatures—or rather stitches, sutures—through and through the thickness of the wounded cervical wall, thus firmly approximating the mucous and peritoneal surfaces and condensing the intermediate tissues of the cervix, as well as narrowing the area of the remaining cervical wound. Such a method would compress the blood-vessels, and at the same time so contribute to stiffen and contract circumferentially the wounded end of the stump as to facilitate its being drawn down and inverted through the still patulous os uteri afterwards. In fact, the proposed stitches or sutures, being many in number, and with their ends sufficiently long (a foot or more), the latter might be passed through the cervical canal into the vagina, and subjected to moderate traction in performing inversion, as well as in drawing down the inverted stump through a deformed pelvis toward the vulva, for the purpose of inspection or the application of whatever means might be subsequently required. Be it understood that, in applying the stitches, they must not cross the cervical canal so as to approximate one mucous surface to the other, but go to and forth, through the thickness of the uterine wall only, from mucous membrane to peritoneum and back again, and they are to be applied before the rubber band is removed. After granulation of the wound has sufficiently progressed, the stitches may of course be removed *per vaginam*. A drainage tube may be placed in the cervical canal should the latter itself not afford a sufficient outlet for the escape of retained fluids.

Doubtless practical operators may contrive other and better methods of executing the inversion mode of treatment than I have been able to do. My only purpose here is to *suggest* inversion of the stump—turning the remaining cervix inside-out, so as to bring the wound into the vagina. This has now been done.

A CONTRIBUTION TO THE ETIOLOGY OF SOME CASES OF
SEPTICEMIA ACCOMPANYING LACERATED CERVIX.

BY

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CASE I.—Age thirty-four; fourth pregnancy. Previous labors easy, and patient has always enjoyed good health. Dr. Wm. L. Cooper, of Troy, was called to the patient, at eleven o'clock P.M., September 26th, 1883, a couple of hours before which time the waters had broken. The patient's condition was good, os dilated for three fingers, and a presentation of the brow, with the occiput right anterior, was made out. Towards three o'clock A.M., September 27th, the pains became very much stronger, and finally, by a *single pain*, the brow presentation was converted into an O. R. A., and the head descended on to the perineum. In three or four more pains, the child was delivered, and the subsequent stages were apparently normal. Three days later, there was an elevation of temperature, with higher evening exacerbations, and Dr. Cooper, who had a sore finger and a rheumatic attack, refrained from an internal examination, but ordered vaginal irrigations, in addition to internal medication. On October 4th, Prof. Wm. P. Seymour saw the patient in consultation and found the uterus movable and a laceration of the right side of the cervix, together with a fusiform mass in the right broad ligament distinct from the uterus, about the size and form of a medium Carolina potato, the uterine end pointing to the laceration, although the laceration did not extend to the vaginal junction. The mass was smooth, hard, and tender. Dr. S.'s opinion was that it was a commencing cellulitis which had not proceeded far enough to fix the uterus. Prof. S. advised, in addition to the previous treatment, the washing-out of the uterus with hot solutions of chlorinated soda. Two or three days later, owing to the irregular contraction of the previously smooth mass, the mobility of the uterus, and the marked diminution of tenderness in it, both Drs. S. and C. were of the opinion that the mass was a hematoma produced by the laceration of the pericervical veins at the same time that the cervix was lacerated. From October 4th until October 9th, the uterus was washed out, twice a day, with a hot solution of chlorinated soda, and the pulse and temperature fell to normal, and the tenderness disappeared. Owing to objection on the part of the patient and friends, the intrauterine injections were omitted, and, in a couple of days, there began a second rise in temperature and pulse. The uterine injections were resumed, but met with opposition from family and patient, because of causing chills and uterine colic. A few days after his first visit, Prof.

Seymour introduced a sound into the uterus, and during the examination felt an opening in the right side of the cervix, near the level of the internal os, through which opening the sound passed into the right broad ligament, three inches in the direction of the hematoma. The sound could be felt distinctly from the vagina, passing almost at right angles to the axis of the uterus. The 12th of October, in the stools were found some masses which were regarded as coagula, and examination showed the hematoma to be smaller and far less tender, and the sound, on withdrawal, had a marked fecal odor. On the 13th, Dr. Wm. P. Seymour, assisted by Dr. Wm. L. Cooper and the writer, aspirated the mass, in hope of finding pus, the result of suppuration and cellulitis complicating the previously existent hematoma. About two ounces of pure blood was withdrawn, and an equal amount of a weak solution of chlorinated soda was gently introduced. The aspirator was then exhausted to withdraw the solution, without getting any fluid. Thinking this might be due to an obstructed canula, the aspirator was disconnected, and the canula cleared with the trocar. But instead of exhausting the air, I condensed it, by mistake, supposing the intention to be to blow out the rubber tube before again attempting to withdraw the injected fluid. My father, supposing the air had been exhausted, made the connections, turned the cock, and the condensed air was instantly discharged through the canula, with such a hissing noise as showed that a large portion of it had escaped, alongside of the canula, into the vagina. The patient, who had previously been decidedly restless, and had complained of things growing dark before her eyes, screamed aloud and went into an unconscious state, with cool skin, moderately rapid pulse, and accelerated respiration, and had a large liquid movement of the bowels. The immediate impression was that air had entered the veins. The patient, with the aid of stimulants, soon reacted and, on being questioned as to her sensations, responded that she had not been conscious of the injection, having lost consciousness before that time—showing that that which we, for the moment, regarded as a serious, if not a fatal, accident, was a simple attack of cardiac syncope. Uterine injections were continued until the 15th, and then given up, because of opposition of patient and friends. Up to this time, there was no tympanites nor peritoneal tenderness whatever, but on the 16th, peritonitis developed, and on the 19th the patient died.

Autopsy, the 20th.—Only the abdomen was opened. The body was in good condition. Owing to the undertaker having injected some fluid into the abdomen, the amount of abdominal fluid was not noted. It contained, however, some pus and blood-clots. There was little injection of and no lymph upon the intestines, save in the pelvic cavity. In attempting to remove the pelvic organs in mass, the attachments of the rectum were torn; so it could not be determined whether the pus cavity found communicated with the rectum. The uterine neck was lacerated on the right side, laceration nearly effaced, uterus four inches in length. The hard mass could not be felt, and, as before mentioned, the attachments

of the rectum were torn in removal. Starting with an indurated cord of tissue proceeding from the right side of the cervix, a cut into the broad ligament revealed, at the site of the previous hematoma, a collapsed pus cavity, three inches in diameter, but no communication with either uterus or rectum could be demonstrated. The fecal odor on the sound, the coagula in the stools, and the liquid stool following the injection of air make it probable that there was a communication of both the hematoma and the laceration with the rectum. Although the laceration through which the sound was passed in the direction and beneath the mass evidently did not connect with it, both were doubtless the result of concomitant, but distinct lacerations.

CASE II.—On October 16th, at 9:30 A.M., I was called to the patient, twenty-four years old, second pregnancy. External examination: back to the right and posterior, head in excavation; fetal heart 144. Internal examination: os dilated for two fingers. O. R. P. At 10:30 A.M., the membranes ruptured, and I found the lambdoidal suture partially straddling the right spine of the ischium; os apparently fully dilated. Recognizing the case as one where the occiput would rotate anteriorly, but slowly, as I thought, I was about to apply the vectis to produce flexion and anterior rotation, when a *single terrific pain* caused complete anterior rotation and descent on the perineum. In half an hour, a living female child of eight pounds was delivered; perineum intact; f. 3 i. fl. extract of ergot given, and placenta expressed. Uterus sluggish; hemorrhage sharp. I immediately gave a hot uterine injection, which caused prompt cessation of bleeding and uterine contraction. *After the hot-water injection*, I could feel an extensive laceration of the right side of the cervix. On the morning of October 19th (three days later), temperature 103.3°, pulse 125. Acting on a dictum of Prof. Seymour, the result of experience in the previous case, that “*no case of recent laceration of cervix is understood until examined with the sound*,” I examined the patient with the sound. The infravaginal laceration did not extend to the fornix, but internally there was a second laceration at the level of the internal os, and from the upper part of the laceration I could easily pass the sound into the right broad ligament one and one-half inches. Hot intrauterine injections of liquor sodæ chlorinatæ (1–20) were used twice a day, and hot vaginal irrigations of the same, four times a day. Quinine, liquor ferri chlor., and soda bisulph. internally. *The first intrauterine injection caused a sharp chill, subsequently I ordered to be given, an hour before each visit, at first twenty and later ten grs. of quinine, with the effect of banishing the chills.* The patient made an excellent recovery.

Nowhere save a single case in Winkel¹ have I found any

¹ Winkel on childbed, 1876, p. 91. Here a case of Hecker's is reported as hematocele ante-uterina extra-peritonealis occurring from partial rupture of the uterus.

reference to periuterine hematomata occurring during labor, and nowhere have I found any reference to these peculiar lacerations extending into the broad ligament, not even in the recent paper¹ on puerperal septicemia by Dr. Thomas, which may be assumed to be a complete resumé of the most recent teachings on the subject in the opinion of one, himself regarded as an authority.

These cases are valuable as instances of lesions until now, so far as I know, undescribed, one of which cannot be recognized without a careful examination with the sound. These lesions are doubtless commonly the source of septicemia in cases where the source of infection is held to be slight vaginal, perineal, or intravaginal lacerations of the cervix. They likewise force upon us the fact, long ago taught by my father, that *"the passage of the head or breech through an incompletely dilated os should be prevented by keeping back the presenting part and supporting the cervix, until the uterus and vagina are continuous, just as the presenting part is held back and the perineum supported to prevent its laceration."*

That puerperal peri-hematocoeles have not been oftener observed is because examinations are not often made during the first week subsequent to delivery and because afterwards they are regarded as vestiges of cellulitis. I have now under my care a case confined by a midwife, which, with a movable uterus of four inches, presents a large nodular mass of fist size in the left broad lig., without tenderness, from which I within a few days aspirated a couple of ounces of bloody serum. There is no tenderness of the mass, nor the board-like feel of cellulitis, and besides the symptoms of shock and fainting subsequent to an almost normal labor point to hematoma rather than to cellulitis. The chills following intrauterine injections I regard as chills of nervous origin analogous to the chill from catheterization of the urethra and not as due to the absorption of septic matters, such as thrombi dislodged by the injection, as maintained by Fasola.² In my experience, quinine in 10 to 20 gr. doses, from twenty minutes to an hour or so prior to the intrauterine injection, will absolutely prevent the occurrence of

¹ New York Academy of Medicine, December 6th, 1883.

² Centralblatt für Gynäkologie, October 6th, 1883, p. 683.

these chills and probably also of the uterine colic, from its oxytocic effect.

As solution for these injections I have used carbolic acid, bromide, thymol, iodine and chlorinated soda, and I have settled down to a solution (1-20) of the latter as the most satisfactory. The majority of my obstetric cases have been among the poorer classes, but I have myself in all septic cases washed out the uterus twice a day, and had a member of the family administer hot vaginal irrigations of the same solution every three or four hours, and I always precede the intrauterine injection by a thorough irrigation of the vagina to prevent the introduction into the uterus of septic matter.

Summary.

1st. Until the cervix is fully dilated, the presenting part must be kept back and the cervix supported to prevent its laceration by the presenting part or the after-coming shoulders.¹

2d. Hot-water injections will, immediately after delivery, make lacerations of cervix and vagina patent which otherwise would not be discovered by the touch for several days.²

3d. In every case of suspected cervical laceration, a careful search should be made with the uterine sound for supravaginal cervical lacerations.

4th. Large doses of quinine, twenty or so minutes prior to the uterine injection, will prevent chill and probably also uterine colic.

5th. A supra-vaginal laceration of the cervix should be treated by the armed probe and irrigation.

6th. Examination within the first few days after labor would probably show a fair percentage of periuterine hematomata from laceration.

¹ Probably, apart from instrumental cases, the worst cases of laceration of perineum or cervix are produced by the shoulders.

² See my second case and Prochownick: *Assembly of German Naturalists*, 1883, *Centralblatt für Gyn.*, October 6th, 1883, p. 646.

ON THE NEED OF COURAGE AND CAUTION IN CLEANSING
THE UTERINE CAVITY AFTER ABORTION.

BY

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COURAGE, for we are bound down partly by conservatism and partly by an instinctive avoidance of duties which are essentially unpleasant; and caution, because there are certain definite dangers to be apprehended from too great boldness. Several recent cases have impressed me with the need of more thorough and personally energetic work in the matter of cleansing the uterine cavity after it has been partly emptied of its contents either after abortion or delivery at full time, and I cannot choose a better text than the following quotation from Cazeaux:

“A lady, thirty-five years of age, felt a discharge from the parts after a suspension of the menses for two months and a half, which she at first mistook for a return of her courses, but which, after riding out in a carriage, was suddenly converted into a profuse flooding. Summoned immediately, I found the os uteri slightly dilated, and I forthwith employed various measures adapted to the arrest of the discharge, and, among others, ergot. The hemorrhage gradually diminished, and six hours subsequent to the invasion of the symptoms had entirely ceased. During the first five days, the patient did very well, but on the sixth I thought I detected a slight odor in the lochia, and at 3 o'clock in the afternoon a violent chill came on which lasted an hour. From this moment, all the phenomena of absorption were manifested. I immediately administered forty grains of ergot, but without effect, for nothing came away; and, notwithstanding the enlightened efforts of Messrs. Chomel and Moreau, who were several times called in consultation, this unfortunate lady died on the tenth day. At the post-mortem examination, we found the uterine tissue softened, and its cavity filled by the putrefied and still adherent placenta, which we could not separate without tearing.”

The text illustrates, in a notable manner, the inefficiency of many consultations.

Drs. Chomel and Moreau were undoubtedly men of eminence in their professional circle. The family of the deceased

probably considered that there could be no possible cause for reproach for lack of duty done, but the post-mortem told of fetid contents, and those fetid contents told of duty undone, and in striking tones warn us that a consultation should mean *action*.

Cases of abortion are among the undesirable but unavoidable ones which we are constantly called upon to treat. My query is, What should we, in view of the best knowledge of the present day, do ?

During the first two months of gestation, the fetus and placenta are so intimately bound together that an abortion at that period usually means that when one is expelled the other must be also. But, after a later date—and it is usually after the second month that this accident occurs—the expulsion of the fetus is an easy, and that of the placenta a much more difficult matter.

It is not my purpose to consider the means of preventing a threatened abortion ; but, assuming that the uterus contains dead tissues, tissues which have ceased to be nourished by the maternal blood, what course should we pursue ?

The first step, and one which not unfrequently is omitted, is to ascertain what there is remaining in the uterine cavity. It cannot be too well borne in our minds that statements of patients and their attendants are of absolutely no value ; a large blood clot figures for the placenta or a fragment for the whole. The actual examination of the uterus and its contents is the only criterion which can conscientiously be relied upon.

If we are called at an early date, we find a hemorrhage more or less severe, and the cervix but slightly dilated, with, perhaps, the unbroken amniotic sac or a portion of the placenta filling the os. In such cases, it is reasonable and right to allow of sufficient time for the natural effort at expulsion, especially as the dilatation of the cervix is a necessity in any event, and this requires a certain time. A delay of twenty-four hours ought generally to be sufficient. The hemorrhage may need to be controlled by tampon, and, as regards the tampon, the danger of septic decomposition of the blood retained thereby in the upper portion of the vagina and its subsequent absorption demands the frequent change and the thorough antiseptic cleansing of the vagina, a duty better done by the

responsible physician than by the generally incompetent and always irresponsible nurse or neighbor.

When the hemorrhage does not call for the use of the tampon, the same thorough cleansing of the vagina (and one douche daily is by no means enough) certainly tends to prevent the putrefactive process in the tissues within the uterus itself by making the access of septic germs less likely to occur, and ought invariably to be carried out.

In my experience, opiates have seemed to better facilitate the rapid as well as the less painful completion of an abortion than ergot, and they have the further advantage that they certainly are more likely to prevent the abortion if it is still preventable.

If, now, after reasonable time, the uterus has failed to expel the retained mass, or if, as is more commonly the case, the fetus has been expelled and the placenta retained, when the physician first arrives, the question for decision is, Shall we be active or passive? In favor of the passive plan are the facts that many women go safely through an abortion with no medical attendance whatever, or, what is practically the same thing, many women are and have been treated on the expectant plan, and no septic process has developed, no harm has come. The task of removing the contents, too, is often difficult, unpleasant, and tedious, and the physician's ease and the calls to other patients urge him to wait.

On the other hand, the one fact cannot be gotten rid of that there, in that uterus, lies a dead mass awaiting only the access of some septic germ to be converted into a reservoir of poison. Believing, as I do, that in case of constitutional diseases with local poisonous foci the fatal result usually comes from continuous poisoning, that, long before the symptoms of general infection show themselves, a continuous elimination or destruction of the small quantities of the poison first absorbed has been going on, and that it is only when constantly arriving reinforcements of the poisonous germs have overcome the reparative effort that the dreaded chill gives the signal of defeat, I cannot do otherwise than advocate immediate effort to remove the dangerous contents.

Granting this, we have to consider how it shall be done, and when.

It not infrequently happens that when first called we find the cervix dilated sufficiently to easily admit the finger, and a portion of placenta projects more or less into the vagina. It is very natural to get hold of this, and to make traction either with the fingers or the placental forceps. Occasionally, such a procedure brings away the entire placenta; but, unfortunately, the usual result is that some ragged shreds are torn off, and that which dilated the cervix is removed. The physician perhaps concludes to "give nature another chance," and, returning a few hours later, finds the cervix shut up and the placenta locked inside. If he gave a dose of ergot before leaving, such a result can be looked for with considerable certainty. All that he has accomplished has been to close instead of opening the way, and he has possibly on his finger or on the forceps introduced septic germs, up to that time absent from the uterine cavity.

Unless the whole placenta is to be taken away, it is simply folly to tear away the projecting portion. No! the time *when* is when we find the cervix open, and any delay is then a source of danger. And if dilatation does not occur spontaneously, or if the physician be called after the cervix has closed, after having already been dilated, artificial dilatation is and must be the first step to further action.

Tents, dilators, or the finger may be used; much can be done with the finger alone.

The instrument above all for the removal of the placenta is the finger. In the hands of an expert the curette is safe and generally efficient, yet even Dr. Mundé, himself an especial advocate of the curette, reports a case¹ where the fingers succeeded after the curette failed him. Further, I question seriously the ability of the general practitioner to so use the curette as to remove only the placenta. It is not, as Dr. Mundé thinks, a question of an inexperienced hand perforating the uterus with the curette, for such an accident would mark something worse than inexperience. But greater or less wounds of the softened uterine substance could readily be made.

By means of proper position of the patient so as to secure the utmost relaxation of the abdominal walls, and by external pressure with the unemployed hand, the uterus can almost

¹ AMERICAN JOURNAL OF OBSTETRICS, September, 1882, p. 270.

always be depressed so that the finger can reach the fundus. In cases of tense or thick abdominal walls, the administration of an anesthetic is an efficient adjunct and the depressing after-effect of the pain is an additional argument in favor of its use.

One case occurring in my own practice urges me here to give this warning: Avoid as far as possible crowding the uterus too hard against the sacrum.

The case in point was one where no anesthetic was given. The abdominal walls were lax, yet the complete removal of the placenta was extremely difficult and tedious. During the manipulations the sacral nerves were so seriously bruised that the patient suffered for several weeks from intense neuralgia of the sacral nerves and their branches.

In cases of extreme difficulty in reaching the fundus with the finger, more or less of the hand, and, in multiparæ, the entire hand, can be inserted into the vagina.

The finger once in, care should be taken to get every possible portion of the placenta loosened, and I would here mention that the less often, in the course of the manipulations, the finger is withdrawn and reintroduced, the less the suffering of the patient.

In case of the failure of the finger, the curette should undoubtedly be used, but with all care possible to avoid laceration of the uterine substance, and the same care to remove all portions of the placenta.

Of the placental forceps I can only say that they do not seem suited to the removal of adherent portions of placenta; and if they only succeed, as I believe they are apt to do, in tearing the placenta into shreds, they are of no actual use, and may do harm by misleading. As regards the thorough, complete, and prompt removal of the placenta after child-birth, no question exists.

So much then for the cleansing of the uterus of its grosser contents. It remains to consider the procedure to be followed when, after the delivery of the placenta at full time or after an abortion, there still remain substances in the uterus capable of putrefaction. In the case of removal of the placenta of abortion, as just outlined, it is not seldom that some detached shreds are left, or another physician may have left some considerable

fragment of placenta; or at full time the placenta may have been delivered and a portion of the membranes retained; or blood may be retained either from its forming clots too large to escape through the contracted os, or from a flexure of the uterus closing the exit. In all of these cases we have again the possibility of the formation of a centre of infection; and one principal object of this paper is to urge the more general use of the double intrauterine injection tube as a means of cleansing the uterine cavity.

It is futile to wash out the vagina; yet case after case occurs where disinfectant vaginal injections are trusted to, while the thing which ought to be gotten at is not touched. There is a natural and general fear of doing anything inside of the womb, which, together with certain recognized and acknowledged dangers, prevents many practitioners from ever invading that sacred precinct, in spite of the fact that the best authorities unite in earnestly urging that when the seat of danger is inside of the womb, there we must follow it.

My experience with intrauterine injections in the cases under consideration has been markedly and uniformly favorable. By the use of the double tube the danger of any of the injected fluid being forced up the Fallopian tube is practically none. The effect of disinfectant injections in reducing temperature, in cases where constitutional disturbance had already set in, has been unequivocal. In one case in which I was called to a patient in whom a portion of placenta retained had caused a temperature of 103.5° , the drop to 99° within three hours after thorough disinfection of the uterine cavity gave most distinct evidence of the efficacy of the treatment. While I have never as yet had any untoward effects from the use of intrauterine injections, it is right to add that it would seem that their use long (six or seven days) continued after confinement tends to increase the danger of late post-partum hemorrhage.

In conclusion, I would specially call attention to flexion of the uterus as a cause of retention of the lochial discharge, and thus of certain cases of puerperal fever. The mention of it at once suggests the reasonableness of prompt reposition and thorough cleansing.

THE IMMEDIATE TREATMENT OF ABORTION.

BY

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AFTER the conclusive review of the obstetric literature of abortion by Dr. Alloway, in the JOURNAL OF OBSTETRICS for February, 1883, and the convincing cases of Dr. Mundé, in the same number, additional testimony as to the efficacy of this plan of treatment may appear uncalled for; but, inasmuch as there are many physicians who still believe in the *expectant* plan of treatment, the following cases are detailed, as they are additional arguments in favor of the immediate removal of the secundines, etc.

I may state, in brief, the armamentarium I have found necessary as follows: head-mirror, speculum, large curette, applicator syringe, tenaculum, cotton, Churchill's tincture of iodine, and carbolic acid.

CASE I.—Miss M., æt. twenty, nullipara. Came to my office in February, 1880, to be examined in order to see if she was pregnant. Upon examination, and judging from the history she gave, I concluded she was at about three and a half months. I was called to see her the same evening and found her suffering from a threatened attack of pelvic cellulitis, the result of her efforts to induce abortion with a darning-needle. She was cautioned against making any further attempt, and, upon her promise to refrain, I prescribed for her. Late on the same night, a lady friend who was visiting the patient, deliberately ran across the room and jumped upon the patient's stomach. The next evening, I was summoned and found the patient flowing profusely. The fetus had escaped, and the placenta, which was very adherent, was removed with much difficulty, by manual efforts and with the curette. The patient made an excellent recovery.

CASE II.—Mrs. B., æt. twenty-six, IIpara, three months pregnant; May, 1881. Patient had induced abortion, on five previous occasions, with a darning-needle, and was equally successful this time. As a result of her preceding efforts, I had operated upon her for lacerated cervix, in December, 1880. Patient had been flowing for five days. Placenta retained, removed with curette; uterine cavity swabbed with Churchill's tincture, and tampon applied. No subsequent trouble.

CASE III.—Miss M., æt. twenty-four, nullipara, four and one-half months pregnant; September, 1882. Patient came to my

office doubled up with "cramps," and stated that she had been flowing for one month. I examined her and found a fetus protruding from the os. I immediately applied a tampon and sent for my friend Dr. C. E. Bruce, as I desired that so unusual a circumstance should not occur in my office without proper protection to myself. The fetus was delivered by the breech, and, owing to the putrefied condition, much difficulty was experienced in its delivery. The placenta was very adherent, but, after about twenty minutes' work with the curette, and by manual effort, it was removed in piecemeal. A tampon was applied, the cause of patient's maternity was sent for, and, after recommending her to seek some other physician, she was sent home. The next evening, on entering my office, I was surprised to find patient waiting for me. Expostulation was of no avail, patient declaring she felt very well. She was examined, and a thin sanious discharge only was discoverable. Churchill's tincture was applied, and no further treatment was necessary.

CASE IV.—Mrs. S., æt. twenty-one, nullipara, two months pregnant; September, 1882. Had been flowing three weeks. Had been attended to by a physician who advised her to wait. Patient was almost exsanguinated, and a decided stench was evident in the room. Fetus had escaped; uterus curetted, placenta removed, uterine cavity swabbed with Churchill's tincture, and tampon applied. No further treatment necessary other than for the anemia.

CASE V.—Mrs. S., æt. twenty-seven, Ipara, two and one-half months pregnant; September, 1882. Flowing five days. Os dilated, with embryo presenting. Uterus gently curetted, and contents removed. Same treatment as preceding case. Patient up in three days.

CASE VI.—Mrs. S., æt. thirty-three, IIpara, two months pregnant; July, 1883. Flowing six days. Fetus had escaped. Placenta retained. Same treatment as in preceding cases. No subsequent trouble.

CASE VII.—Mrs. M., æt. forty-three, XIIIpara, two and one-half months pregnant; July, 1883. Flowing twelve days. Fetus had escaped. Placenta retained. Same treatment. Patient up in two days.

CASE VIII.—Mrs. T., æt. twenty-seven, Ipara, two months pregnant; August, 1883. Flowing three days. Fetus had escaped. Placenta retained. Same treatment. No subsequent trouble.

CASE IX.—Mrs. D., æt. twenty-two, Ipara, four months pregnant; December, 1883. Flowing five days. Fetus had escaped. Placenta retained. Same treatment. No subsequent trouble.

CASE X.—Mrs. G., æt. thirty-four, IVpara, two and one-half months pregnant; December, 1883. Flowing one month. Uterus thoroughly curetted, and placenta removed. Patient up in three days.

CASE XI.—Mrs. J., æt. thirty-three, IIIpara, five months pregnant; January, 1884. Patient fell, in December, 1883, and

had been flowing one month. Condition the same as case III. Same treatment as in preceding cases. No further trouble.

CASE XII.—Mrs. N., æt. thirty-three, Vpara, two months pregnant; October, 1883. Flowing one week. Fetus had escaped. Portion of placenta retained. Same treatment. Recovery prompt.

CASE XIII.—Mrs. McC., æt. forty-two, VIIpara, four months pregnant; January, 1884. Flowing one month moderately, and five days profusely. Treatment as in other cases. Flow immediately ceased on removal of after-birth. No subsequent trouble except anemia.

In all of these cases, the beneficial effect of promptness is positively shown in the short duration of the flow after the immediate treatment. In none of these cases was there any bad effect, and in some of them the hygienic surroundings were of the worst. In all of them, the os was found sufficiently dilated to render the use of a tent unnecessary, especially as I have been in the habit of using a curette much smaller than that of Dr. Mundé, and I have found, to my mind, that it answers all requirements. I may say, in closing, that, in about one hundred cases reported, there has not been one death as a result of this plan of treatment, which is, I think, sufficiently conclusive evidence as to its safety.

154 E. 30TH ST.

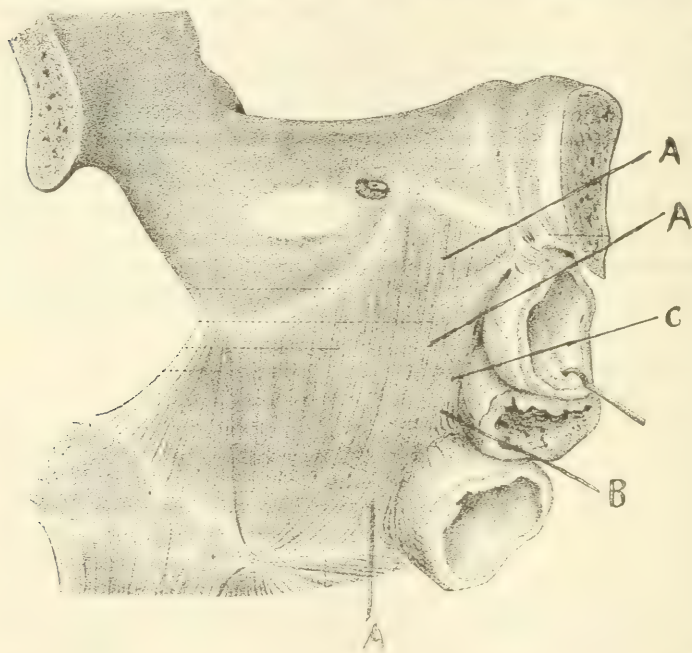
INJURIES OF THE PELVIC DIAPHRAGM AS THE REAL CAUSE OF DESCENT AND PROLAPSE OF THE WOMB.

BY
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THE simple fact that descent and prolapse of the womb may happen without the presence of injuries to the perineum, and even in virgins; and the reverse, that women who present the gravest forms of perineal rents retain the so-called normal position of the pelvic organs—I say this simple fact should teach us that the perineal ruptures have not as much to do with the changes in the positions of these organs as we are every day taught in our text-books. Only recently some of our great men have begun to doubt the correctness of the time-honored idea that the perineum is an organ of as distinct character and function as, for instance, the liver; only recently have they begun to withdraw the charge that the injured perineum was responsible for every kind of havoc to the whole

female architecture. So Emmet had the audacity to doubt; and though I do not subscribe to his views in all their special points, yet, strengthened by the weight of his authority, I feel less hesitation in making the following remarks.

I contend *that the perineum is no separate organ of itself; that it is nothing but an anatomical general name of a region which even by men like Henle and Savage cannot be distinctly described.* It consists of a number of *parts* of muscles which meet each other at this place. We say parts, because other



parts of the very same muscles lie outside of the perineum. It is merely accidental and due to the economy of the anatomical arrangement that so many muscular bodies of different functions meet there, and form a kind of muscular centre.

In the second place, what I wish to state is, that the true floor of the pelvis is what Luschka termed the pelvic diaphragm. We need only look at Savage's Plate¹ (which I take the liberty to copy), to understand at once that this sheet, arranged like the thoracic diaphragm of different strata, is the

¹ Savage's work on female pelvic organs, Plate xiv., Fig. I.

veritable floor of the bony pelvis, destined to close its inferior opening and to counteract the pressure brought on the pelvic viscera from above.

We see the three more or less tubular organs, rectum, vagina, and bladder, protrude and take their place below this diaphragm. The muscles, which partly support those organs outside of the diaphragm, and partly act as special functionaries to them—sphincters and openers—are to be considered as being glued to the diaphragm, and having an anatomical rather than a functional relation to the former. If this be correct, the levatores ani, or, as Savage calls them, the pubo-coccygei, do not belong to the perineum, but are a part of the diaphragm.

Thirdly, I contend that the womb, like the pelvic viscera in general, is not supported by the perineum directly, and will not necessarily fall when the perineum is torn, but that descent or prolapse can take place only when the pelvic diaphragm fails to act. We will see that there are circumstances under which a ruptured perineum can lead to descent of the womb, though it will be but indirectly. As a rule, the intact diaphragm, in its independency of the perineum, will prevent the pelvic organs from falling. *It requires an injury to it to allow the womb to descend.* This explains why the perineum may be rent without injury to the womb, and why we may have prolapse even in virgins. The diaphragm may become weakened aside from the effects of parturition.

As the point, whether there are such injuries and of what nature they may be, is the main object of this paper, I will go a little more into particulars, though I reserve a more exhaustive discussion for the future.

That there are injuries of the pelvic diaphragm from other than gynecological causes we are taught by every surgical text-book. There are obturator, perineal, pudendal, vaginal, ischiatic, even rectal hernias—all protrusions through the pelvic diaphragm, none of which can happen without separating some of its muscular strata. If such accidents may happen outside of child-bearing, then we can easily understand that in parturition they will happen much more frequently, where such an extraordinary strain is brought to bear on the pelvic diaphragm, which has to withstand the full force of the expelling powers,

and which must be stretched to its utmost during labor. But to understand the conditions in their full extent, I have to recall some anatomical points. We know that the two fellows of the levatores ani take their origin on the two pubic rami, that they run alongside the vagina, until they meet each other behind this organ, and between it and the rectum (in the diagram A being the levator, B the point of meeting, C circular fibres of the levator surrounding vagina.—Savage's Plate). There the two levatores form a sharp angle, and whilst the muscles then run towards the coccyx, leaving an opening for the rectum, they send a pretty strong bundle of circular fibres around the vagina also. We might term this bundle the internal sphincter vaginæ. This muscle forms, in connection with the angle of the levatores, the elevation in the vagina which Thomas calls very properly the "vaginal promontory." The space between the symphysis pubis and this promontory is a slit which allows the vagina and a portion of the bladder to protrude from the pelvis. It is partly narrowed by the sub pelvic ligament and by a strong aponeurosis, running from the pubic rami toward the anterior vaginal wall, inserted where bladder and urethra meet, forming an easily found transverse ridge (Fritsch).

This slit between the two levatores is to the pelvic organs what the inguinal canal is to the intestine: it is, under normal circumstances, not wide enough to allow descent or prolapse. The analogy even goes further. We might compare the two levatores to the two shanks of the inguinal canal. One of the old authors has called the descent of the womb a uterine hernia. There are normally certain organs passing through both the inguinal canal and through the pelvic diaphragm, and only under morbid conditions will more protrude. When in labor the child enters this slit, which will be stretched to the utmost, under untoward circumstances it will give way at the weakest point—the raphe—where the two levators meet. It will be immaterial whether the stretching is done by the circumference of the child's head, or by any sharp prominence of the child's body, such as the nose, elbow, shoulder, etc., which will directly cut into the angle of the slit. One thing is certain, that this slit is more exposed and has to withstand more force than the yielding perineum below. The vagina in its elasticity will remain uninjured, whilst the levatores may separate or

be ruptured. We see an analogous condition in the diastasis of the recti abdominis, which give way, whilst the peritoneum and integument remains undisturbed. Thus we might have a true diastasis or we might have a true rupture of one or both of the levatores. They might break loose from their pubic insertion, or be ruptured anywhere in their course. We might have a simple relaxation of the whole structure of the pelvic diaphragm by a morbid condition of the muscles and cellular tissue, or from over-distention by any force from above. We know how, by a forcible shock, a hearty laugh, the urine may involuntarily be expelled. Now, suppose a continued force should be exerted on the diaphragm; would not the above-mentioned slit be liable to become distended and enlarged, it having to bear the most of the pressure at its weakest point?

To sum up: There are three kinds of injuries possible to the diaphragm, viz.: general relaxation, ruptures, and diastasis. For our purpose, the levatores ani will be the only diaphragmatic muscles which we will need to study. If they give way, there will be so much power of resistance taken from the floor of the pelvic outlet as to allow the womb to protrude, and, finally, to fall through it. In order to strengthen my position by good authority, I give here the translation of part of a report made by Prof. Schatz at the last meeting of the German gynecologists.¹ He says: "The ruptures of the floor (diaphragm) are not yet sufficiently understood. In regard to dislocations of the pelvic organs, especially prolapsus, they are even more important than ruptures of the perineum. One can often see cases where the perineum is ruptured beyond the sphincter ani, and where, in spite of the rupture, the firmness of the pelvic diaphragm has not suffered, and no descent or prolapse has happened. . . . The perineum does indeed not belong to the diaphragm; it lies below it. . . . It is generally in labor that the pubo-coccygei (levatores ani) break loose from the pubic rami on either one or both sides, the rupture being caused either by forceps or by the advancing head. These ruptures may occur even whilst vagina and perineum remain intact."

In the discussion following these remarks, Hegar admitted

¹ Report of the 35th Meeting of German Physicians (Centralbl. f. Gyn.)

the occurrence of these ruptures, whilst Freund thought that they were not proven by post-mortem examinations.

As this subject was new, and as attention had not then been called to these injuries, Freund's objections do not amount to much; but I conclude from the facts further to be given that the ruptures of the levatores occur mostly not where Schatz locates them, but at the above-named angle, at their raphé, and that they are rather diastases than true ruptures. I, of course, admit that under certain circumstances perineal ruptures facilitate descent and prolapsus uteri, that is, when the vagina is short and gaping; when the cervix has no resting point on the posterior vaginal wall; when it is directed straight down towards the vaginal aperture; and when, finally, the diaphragm is relaxed. But, under all circumstances, this latter condition will play the more important part. It is a fact, which no honest gynecologist will deny, that even the best result of an artificially-formed or of a well-restored perineum will not restore the uterus to its normal position. Such an operation would enable the perineum to act as a barrier; it might narrow the vaginal outlet; it might do away with cicatricial ulcers, with protrusion of the vaginal wall; it might enable a pessary to stay in place, but it never would correct retroflexion or overcome the descent absolutely. The woman will feel no better after the most successful and handsome operation for a lacerated perineum, provided the suffering is due to retroflexion. The above facts, I claim, are direct proofs of my position.

I will now proceed to give clinical facts in support of this position. I call attention to a condition which will be found in a great number of such cases. *It is a painful, sometimes excruciating sensation, produced when we hook our finger behind the angle of the levatores, or where it ought to be, and when we pull the diaphragm toward the vaginal outlet.* It is best done by pressing the finger in the median line of the sacral excavation, and then carrying it close to the sacrum toward the vulva. The evident suffering from pulling the levatores, or even from the contact, is such a simple, plain symptom of an injury to this muscle that by itself it would be sufficient to form a correct diagnosis. There may be no perineal rupture, no malposition of the womb, still the woman complains of pelvic pain, of not being able to stand on her feet, of a constant dragging

sensation, whilst she feels well in the recumbent position. You are very apt then to find such a sensitiveness of the pelvic diaphragm, with or without retroflexion, or descent of the womb, according to the progress of the sequelæ of a ruptured or relaxed diaphragm.

A further guide to a correct diagnosis is offered by the vaginal promontory. We have seen that it is formed by the junction of the two levatores, and that it belongs to the diaphragm and not to the perineum. It can become injured, and even disappear, whilst the perineum remains intact. *This promontory will in some cases be found nearer the vaginal outlet; it will have fallen, and it will not offer to the touch the normal resistance. We will be justified in taking this for a symptom of relaxation of the diaphragm, either from a morbid condition of the tissues, from overdistention, or from rupture.*

But, in many cases, we will feel the promontory at its proper place; yet, in addition to the afore-mentioned sensitiveness, we will find that when we put our finger in its median line, and make a boring movement towards the sacrum, that the two muscles will separate, and that we will have a true diastasis of the levatores, the continuation of the floor being preserved only by the pelvic fascia. We will, in many cases, feel distinctly the ends of the two levator muscles, and if we, at the same time, put a finger in the rectum, we will feel them separate still more, so that an open space of one to two inches may be produced. In other cases, we will not be able to detect the termination of the muscles. We might then be warranted in presuming a disconnection to such a degree that the muscular strata have retired toward their pubic insertion. I am of the opinion that such a condition is the main cause of a rectocele.

Under all circumstances, we have clinical symptoms by which to make out diaphragmatic injuries. And if they, and not the ruptures of the perineum, are the causes of the changes in the uterine positions in view, we are certainly bound to seek for other means to restore the "normal" position than by operations for perineal ruptures. I do not wish it understood that I oppose the closure of a ruptured perineum, as, on the contrary, under certain circumstances, I deem it absolutely necessary. Yet I contend that these operations have nothing to do with the restoration of the uterine position, and that in descent

and prolapse they constitute only a very weak and indirect remedy, and one not more efficient than is a truss applied to a hernia.

If the position taken be correct, then the remedy has to be applied where it meets the cause. We will have to seek for a procedure by which we can contract the diaphragmatic outlet. Where this is a weakened condition of the tissues only, we know that judicious constitutional and local treatment, which tones up the patient, and the involved parts particularly, might do what we hope for. But in cases which we might term surgical, new operations will have to be found. It will be hazardous to cut through the posterior vaginal wall in order to seek the levator muscles and to sew them together—still, something of this kind must be done. I have in view an operation which was devised by my deceased friend, Dr. Dowell, of Galveston, for hernia. I believe that it will prove to be the correct procedure, as it promises reunion of the separated muscles and narrowing of the slit without any injury to the surrounding organs and tissues.

I will at the proper time report more fully on this point, but for the present would ask the profession to take the whole subject under consideration, and seek some method to remedy evils which, as yet, are beyond our control.

A CASE OF POST-PARTUM HEMORRHAGE, SPONTANEOUS IMMEDIATE REPAIR OF COMPLETELY LACERATED PERINEUM, AND PUERPERAL SEPTICEMIA.

BY

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THE following case is interesting as being illustrative of a subject that is now receiving considerable attention in the obstetric circles of this city.

Mrs. O'C., æt. twenty-six, primipara, was delivered with forceps after a seven hours' labor, on October 25th, 1883.

When the instruments were adjusted, the head was low down,

and the cervix fully dilated. The perineum and entire vulva were very edematous. The former structure was consequently inelastic and not distensible. The pains also were growing feebler, and the intervals between them longer. The cause of the dystocia was a markedly conical pelvis. As a result of the procedure mentioned, the perineum was completely torn through, the laceration involving fully an inch and a half of the anterior rectal wall.

After checking the hemorrhage from the raw surface, I quickly directed my attention to the uterus. It was not contracting at all, and from it the blood was pouring profusely. By a combination of external compression and manual efforts internally, the secundines were removed. I was in doubt about their completeness, and, as subsequent developments proved, justly so.

Further efforts were desisted from, in deference to the exceedingly exhausted state of the patient, and in view of the alarming amount of blood that was still gushing forth. The uterus finally contracted firmly and permanently, under the heroic use of ergot, ice in the cavity of the organ, sponges steeped in vinegar applied locally, abdominal kneading, and posture.

This danger over, the perineum was subjected to close scrutiny. There was no bleeding. I carefully cleansed it, tied the patient's knees together, and directed luke-warm carbolyzed douches (1-60) to be given every three hours, hoping by these means to gain some slight amount of repair.

During the two following days, she remained quietly on her back. There was no rise of temperature, and no discomfort beyond a slight soreness in the perineum.

On the third day she had a painless movement from the use of comp'd liq. powder. Her temperature in the evening $100\frac{1}{4}^{\circ}$. She was directed to change her position as comfort required, the knees being still maintained in close apposition.

From this time on to the septicemic crisis, she received four carbolyzed douches (1-120) daily, five grains of quinine t. i. d., and stimulants p. r. n.

On October 30th, milk appeared in abundance. Her appetite was quite fair. In a painless movement, she appeared to have control over the bowel. I noticed a spot on each side of the vagina near the vulva that sloughed superficially afterwards, but gave no trouble. The temperature varied between $99\frac{1}{2}^{\circ}$ and 100° , per os.

On the two subsequent days she continued to feel well. Her temperature and pulse were normal.

On November 2d, I was suddenly summoned, and found that she had had a succession of violent rigors. She was exhausted, perspiring profusely, and breathing rapidly.

The temperature was 105° in the mouth; her pulse was 120, very small and weak. This was her condition at 10.30 A.M. Stimulants were freely exhibited, gr. $\frac{1}{4}$ of morphia was given, and gr. v. of quinine was directed every hour.

I personally employed intrauterine douches at the same intervals. At 9 P.M., her temperature had fallen to $99\frac{3}{4}^{\circ}$.

The douches brought away shreddy debris, having a slightly foul odor. *But what excited my surprise and delight was this: I found that on this, the eighth day after confinement, the perineum had healed to a beautifully firm entirety.*

During the whole of November 3d, the douches were continued hourly, each time removing debris. After midnight, when her temperature was 101° , it remained uniformly below 100° . There was a decided improvement in her general condition. Quinine gr. v. was ordered to be administered thrice daily. On November 4th, the douches were used every hour and a half, the refuse water still containing debris. Her temperature was stationary at 100° all day.

On November 5th, the uterine cavity was cleansed every four hours with little, and at the close of the day no discharge of debris. As she had been perspiring very copiously since the fever set in, it was successfully checked by atropia (gr. $\frac{1}{20}$) used twice.

On November 6th, she received three douches, followed each time by a few small shreddy masses. After this day her temperature remained normal permanently.

In eight days she was up and about her room. She experienced no inconvenience whatever from inability to control either feces or flatus.

This case is an interesting one from several points of view. Not only was the complete restoration of the perineum *without operation* an unlooked for and phenomenal result, but too much credit cannot be given to the vigorous application of irrigating measures in the treatment of the septic condition of the uterine cavity.

There is no doubt but that some shreds of membrane remained in utero at the time of delivery. The very considerable amount of refuse cleansed from the uterus forces me to believe that, in addition, sloughing either at the site of adhesion or at the placental area was also a prominent factor in the production of the blood-poisoning. As for the gangrenous spots at the vaginal outlet, being under constant inspection, they gave no trouble. In the way of nourishment, she was allowed nothing but kumyss ad libitum during the critical period. It not only expressed a very concentrated and digestible form of nutrition, but obviated the vomiting that is so often present in these cases. Afterwards, she received four or five milk punches daily, and the usual tonics.

She was subjected to catheterization from the beginning. In

regard to the methods and means employed locally when the septicemia was active, as a slight digression, I will note that that they were identical in all the subjects of uterine toxic infection, seven, that I have had. None terminated fatally.

When necessary, as in this case, I repeat the intrauterine douche really every three quarters of an hour. A weak solution of carbolic acid is used (1-120). Dependence is placed not so much on this substance, as on the thoroughness of the cleansing and the quality of the water. Two quarts at least are employed, at a luke-warm temperature.

When possible, I insist upon having it super-boiled before use. It is accumulated in this shape, and heated as necessary.

I consider that water thus prepared is freed from many impurities, especially in summer. The irrigating tube that has given me the most satisfaction is Mercier's "cathetère à coude," 12 English. The quite firm elbow near the tip guides the instrument readily into the uterine cavity, and its just-sufficiently pliant consistency could cause no pain or damage.

With me the following method of introduction has proved simple, painless, and effectual. The patient lying abed in the usual position, and having the bed-pan under her, the left forefinger is passed along the upper wall of the vagina, palmar surface uppermost, to the cervix. If the os looks downward, providing there be no pain, simply flexing the finger brings the cervix forward. The catheter is introduced with the right hand along the dorsum of the left forefinger, easily passing into the uterine canal. If there be tenderness of the vaginal wall, slight traction on the anterior lip accomplishes the same end.

The syringe used is the fountain or Davidson's, whichever happens to be handy.

As a final note, apropos of the case described, I saw the lady on January 11th, 1884, three months almost since her confinement. Her condition as to the genitalia, and also systemic is most gratifying. The lacteal secretion, that was entirely in abeyance during her serious illness, is now in full activity.

TWO CASES OF PUERPERAL RUPTURE OF THE UTERUS.

BY

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CASE I.—*Laparotomy, Death*.—Mrs. C., aged twenty-nine years, taken in labor with her fourth child on the morning of Dec. 29th, at four A.M., 1883. Her previous labors had been tedious, especially the first one, which had to be terminated with the forceps. The present labor progressed naturally, under the supervision of a midwife until 11 A.M.

For two hours previous to this time the pains had been very severe, and the midwife stated that she was on the point of sending for a physician to terminate the labor when the accident occurred.

At 11 A.M., the woman, while standing by the bed, had a very severe pain, and “felt something give way.” She supposed that the child was coming to the world and called the midwife to put her in bed. At the instant of this severe pain the woman felt something rise up in her abdomen.

The midwife, on making an examination, now found that there was no part of the fetus presenting, when just previous to this pain she had felt what she took to be the vertex presenting in the lower strait of the pelvis.

I was then sent for, but being absent, another physician went in my place, and, thinking it to be a case of powerless labor (of course, the pains had wholly ceased), decided to wait my coming before interfering.

I saw the patient at 2 P.M., and after obtaining the history given above, made an examination with the following results: found the os externum well dilated, with the cervix hanging long and loose in the vagina.

Passing the fingers far up, the ring of the os internum could be made out except at its anterior part, where it was absent. By inserting the hand into the vagina and carrying the fingers far up behind the pubic bone I could just touch and make out the testicles of the child.

On placing the hand on the woman’s abdomen, the extremities of the child could be easily felt through the abdominal walls, and towards the mother’s left side could be felt a mass which I took to be the uterus emptied of its contents.

With such a history and such symptoms, there could be no doubt as to the diagnosis. The only thing peculiar in the case was the apparently slight effect such a terrible accident had had on the woman’s general condition; from her appearance I thought at first that I had to deal with an ordinary case of tedious labor;

the woman lay quietly in bed and answered my questions calmly and readily; she complained of no pain except in the epigastrium when she breathed, her pulse was a hundred and weak, her respirations, however, were short and catching, and forty to the minute. In a case of this kind seen by me previously, the sudden increase in the pulse rate, running from 90 per minute up to 120 in the space of a few minutes, was what first caused me to suspect some serious accident. In the present case this symptom was wanting, and all other general symptoms as well.

At 4 P.M., five hours after the accident, we were ready to operate, Drs. Partridge, W. F. Wright, Ashmead, and Bagley kindly assisting me.

Inasmuch as the child was wholly out of the uterus, it was decided to remove it through the abdominal walls. An incision was made in the linea alba from a point just below the umbilicus to within two inches of the pubes. On carefully cutting through the abdominal wall to the cavity, a mass of fluid blood and serum gushed out, and the placenta presented itself to view. When this was removed the breech of the child was seen lying to the mother's right side; the child was wholly outside of the uterus. The rent in the uterus extended from a point just below the os internum up to within an inch and a half of the fundus uteri; it was on the right side and anterior portion of the womb, and was three and a half to four inches in length.

Certain anatomical facts of considerable interest next presented themselves. First, the bladder, which before labor commences lies behind and below the pubic bone, according to Braun's plates, was seen to extend up nearly three inches above the pubes. This, too, when the bladder had just been emptied of its contents by the catheter. This confirms the observations made by Braun, Litzman, Hart, and others as to the change that takes place in the relations of the bladder and the connective tissue of the pelvis during labor. The same observation may be made in any case of tedious labor without opening the abdomen, where the abdominal walls are thin.

Another fact of importance anatomically was the thickness of the peritoneum covering the uterus; this was fully three times as thick as that covering the abdominal walls. This was easily observed as the peritoneum was stripped off from the uterus to some extent in the line of the rent, where, owing to its strength and comparatively loose connection, it could be easily handled and dissected off from the uterus, thus showing how rupture of the uterus might take place, as it does at times, without lacerating its peritoneal covering.

Another interesting point was that the whole uterus was not covered with peritoneum; this covering extended down to about the lower fourth of the body of the uterus and was then reflected across to the bladder, leaving the lower part of the body of the womb uncovered. This observation also confirms that made by Litzman, of Kiel, Germany.

The uterus, near the fundus, was thick and firm, but near the os internum the tissues were soft, loose in structure, and only one-third the thickness of those above. This was very noticeable when passing the needle at the different regions of the uterus.

After removing all the blood from the abdominal cavity and sponging it out with a solution of the bichloride of mercury, 1 to 2,000, the rent in the uterus was brought together with interrupted silk sutures, care being taken to approximate the torn edges of the peritoneum as closely as possible. Nine deep sutures were introduced and three superficial ones.

The abdominal walls were then united with wire sutures and the wound covered with a layer of iodoform. The operation lasted altogether one and one-half hours.

December 22d, morning succeeding the operation. Temperature normal, pulse 100 per minute and stronger than at the time of the operation. She had no pain during the night, and slept at intervals without the influence of a narcotic. External wound looks well. Drew off four ounces of dark-colored urine. Gave one-third of a grain of morphine hypodermatically to control vomiting which began towards morning. The patient was cheerful and appeared as would a woman after an uncomplicated tedious labor.

Evening—Temperature per vaginam 101 degrees. Pulse 108. The morphine has controlled the vomiting during the day, but towards evening regurgitation of a dark-colored fluid commenced. Repeated the morphine and ordered hot flax-seed poultices to the abdomen.

December 23d—morning temperature 103°; pulse 112 per minute. The regurgitation of the dark grumous-looking fluid recommenced. This regurgitation continued from this time on, except when the woman was under the influence of morphine given hypodermatically, until at last it could not be controlled by any means.

Evening temperature, 103°; tympanites has appeared; pulse, 112 per minute.

December 24th—morning temperature, 105°; pulse 120 per minute and weak. Tympanites quite marked. Lochia without odor. Mind rational. Evening temperature, 105°; tympanites more marked.

December 25th—morning temperature, 106½°; pulse cannot be counted; sinking fast. Died at 4 P.M.

Another case of this accident occurred in my practice some years ago when house physician to the Lying-in Asylum in this city. Its history will be given in brief.

CASE II.—*Laparotomy, Death.*—The woman was taken in labor with twelfth child in the evening; breech presentation. The labor was a very tedious one, as the child was quite large. At 3 A.M. the os externum was well dilated and the breech, dorsum anterior, was well down in the pelvis. At half-past three, although the

pains had continued strong, the child had not advanced. I could not hook my fingers into the groin to assist the descent, and, as the woman's pulse was good and the pains regular, I gave a drachm of fluid extract of ergot and lay down in the adjoining room.

Ten minutes later the nurse awakened me to say that the pains had entirely ceased. I felt my patient's pulse to see if her strength had failed and to my surprise found it to be 120 per minute, where ten minutes previously it had been barely 90 per minute. The breech was still presenting and had not receded in the least. An examination of the abdomen made the diagnosis plain, as the limbs of the child could be easily distinguished through the abdominal walls. They were plainly outside of the uterus.

This case was operated on by Dr. Chas. S. Ward, then visiting physician on duty at the asylum. The child was removed by the abdominal section in the usual manner. The child and placenta were wholly without the uterus and in the abdominal cavity. The rupture was on the anterior surface of the uterus and was about four inches in length. The case was reported at a meeting of the Obstetrical Society of this city.¹

Remarks.—The diagnosis of rupture of the uterus should not usually be difficult: in neither of these cases was it so, and yet, on looking over the history of the cases, I find but two symptoms common to both; namely, the sudden and total cessation of the labor pains before we should expect them to cease from exhaustion. Indeed it may be doubted if labor pains ever cease entirely while the child is undelivered and the mother lives, from any other cause than rupture of the uterus.

If such a symptom arise during labor, a careful examination should be at once instituted, even though no other symptoms be present.

The other symptom common to these two cases was the distinctness with which the child's extremities could be felt through the abdominal walls. This symptom is of value only where the rupture is complete, and in women without very much adipose tissue.

In one of these cases the presenting part receded from the outlet of the pelvis, this symptom, together with the cessation of the labor pains, is of great importance when it occurs, as the presenting part could hardly recede from any other cause than rupture of the uterus. In the other case reported, the presenting part did not recede very markedly, but it no longer pressed firmly down into the pelvic strait as it usually does, even in

¹ See JOURN. OBSTET.

powerless labor, owing to the more or less tonic spasm of the uterine muscular fibre.

The rapid pulse rate following this accident is laid down by most authors as a sign of great value. In the first case related, not only was the pulse rate no guide to the diagnosis (pulse 100 per minute), but it led me to doubt that so severe an accident could have occurred without causing more disturbance than was present when I saw the case. In the other case, the rapid pulse rate (120 per minute, having suddenly risen to this figure from 90 per minute) was what first drew my attention to the accident.

The consideration of the cause and treatment of rupture of the uterus would lead me beyond the intended scope of this article, which is mainly to place on record these two cases of this rare accident.

1 CHARLTON STREET.

CORRESPONDENCE.

THE IMMEDIATE UNION OF THE LACERATED PERINEUM BY ONE SUTURE.

TO THE EDITOR OF THE AMERICAN JOURNAL OF OBSTETRICS.

MR. EDITOR:—In the last issue of your JOURNAL is an article entitled “The Importance of Immediate Post-Partum Examination of the Perineum in Every Case of Labor and, when Lacerated, its Treatment by One Suture,” by T. Johnson Alloway, M.D., in which he says that he has introduced the suture without the patient or her friends being aware of the fact. Will he be kind enough to inform me *how* he does it? It is necessary to place the patient in position for the suture to be introduced, and would not that movement give rise to some inquiry either on her part or that of her friends? If the thing is possible, surely we will hail it with joy, for, as a rule, it is anything but pleasant to acknowledge the fact of a laceration and the necessity for the introduction of sutures.

“W.”

PHILADELPHIA,
January 20th, 1884.

TO THE EDITOR OF THE AMERICAN JOURNAL OF OBSTETRICS.

DEAR SIR:—I beg to acknowledge the receipt of letter from W. of Philadelphia, which you have kindly forwarded to me. In reply I would say that the operation is very easily accomplished. In all first labor cases, as soon as the second stage is setting in, request all but the nurse to leave the room. Under any circumstances it is exceedingly annoying to be hampered with a mother, mother-in-law, or husband in such cases. As a rule, the patient will be under the influence of an anesthetic—ether in these cases I prefer. Directly after expulsion of the child the physician should inspect the perineum. If lacerated, remove the placenta without loss of time, either by the method of Credé or by the introduction of the hand into the cavity. Then, considering the patient is lying on her left side across the bed, turn her over on her back into the lithotomy position. Separate her knees, tell the nurse to stand behind your right shoulder, holding a lamp or candle in her left hand, and steadying the patient's left knee with her right hand. The needle is then passed in the way indicated. If all this is done rapidly and with a determined hand, the patient will not have sufficiently recovered her senses to know what has taken place. I always, in such cases, have ready a Russian needle holder armed with an Emmet's long perineal needle and silk. If it is not required it can easily be put away again.

I attended a lady in her second confinement a few days since, who, on recovering from the ether, persistently declared she was "ruptured," and entreated me and my medical friend to "sew her up," then. It was useless for the time being to tell her she was *not* "ruptured." She would not accept any such assertion on our part, so strong was the impression left on her mind by a friend who *had* suffered a laceration, was left untreated, and afterwards to undergo the secondary operation to relieve much misery and wretchedness.

Thanking you very much, Mr. Editor, for your kindness, I am very faithfully yours,

T. JOHNSON ALLOWAY.

547 NOTRE DAME ST., WEST,
MONTREAL, CAN

VENESECTON IN PUERPERAL ECLAMPSIA.

DEAR DR. MUNDE:—I have been deeply interested in the published transactions of the Obstetrical Society of New York, and especially in the part reporting a death from puerperal eclampsia, as found on page 174 of this month's JOURNAL OF

OBSTETRICS. I would like to be informed whether, as the concluding paragraph states, "in the further discussion upon Dr. Dawson's case, the impression prevailed that venesection and transfusion would prove of doubtful benefit," *blood-letting* in the early stage of the case might not have turned the scale in the other direction. I fear that vague doubts flitted through the minds of some of the learned gentlemen present as to the possibility of a human life being saved if a free and prompt venesection had been resorted to. But possibly I am antiquated in my ideas, or, perhaps, I have followed the "traditions of the fathers" too closely. I have, nevertheless, the consciousness that in a practice extending to within a few months of a third of a century, I have never had to note a death from puerperal eclampsia.

When I enter a lying-in room, the teachings of my venerated instructor, the late Prof. Charles D. Meigs, Professor of Obstetrics in Jefferson Medical College, are ever before me, and one of the first questions put to the patient is: "Have you any headache?" If answered in the affirmative, and the pulse will justify it, I at once resort to a free bleeding, and am fully persuaded that all of the stages of labor are made lighter, and the complications, so often to be feared, are entirely prevented. I have not always prevented the accession of convulsions, but have often repeated the venesection amidst the most violent spasms and throes of child-birth.

A single case, in which I departed from my established rule, occurred on September 27th of last year, and, as it was pregnant with great peril to my patient and of unlimited anxiety to myself and the friends of the one who a few hours before had brought joy into the household because of the fact that a man-child had been born into the world, I will report it for the benefit of those who might be similarly situated:

Mrs. M. S., primipara, aged about twenty-two years, of delicate build and with soft tissues, was taken with labor-pains about 10 P.M. I inquired as to the condition of her head, and as to whether her feet or lower extremities, or any other part of her body, had been swollen. She informed me that for four or five days her feet were very much puffed up, and that she had pain in her head, but not at all severe. My first impulse was to bleed her, but her pulse was small and very compressible, and fearing that the loss of blood might weaken her own expulsive efforts, I informed her that she should at once apprise me of any increased pain in her head or if the room appeared more dark than it had been. After about four hours of active and pretty severe labor, she was delivered of a child that weighed about ten pounds. I

remained, as is my custom, for over an hour, and left her in as good condition as I ever left any patient. In two and a half hours after my departure, she was seized with a violent convulsion which continued about twenty minutes, for, on my arrival, about a half-hour after the attack, she was conscious, and inquired why I had returned so soon. I was again tempted to resort to blood-letting, but contented myself with ordering repeated doses of brom. potass., and again took my departure, about 9 o'clock. At 2 o'clock A.M., September 28th, thirteen hours after the commencement of labor, and ten hours after its completion, I was again hastily summoned to her bedside to find her in a violent convulsion, with her tongue, a portion of which she almost severed, fixed between her teeth. Her left side was entirely paralyzed, and it was with great difficulty that I had her held in the upright position until I abstracted about one and a half pints of blood. The arm having been secured against further bleeding, she was placed in the recumbent position, and about a quarter of a grain of morph. mur. was injected beneath the skin. By repeated, I might say almost continuous, applications of chloroform to her nose, I held the spasms in abeyance until 9 o'clock, after which time there was no indication of a return. She did not recover entire consciousness until the next day, and at the end of five or six days her paraplegia, which had gradually decreased, disappeared. She made a rapid and good recovery, and, at this writing, both she and her babe are in excellent health, the mother more vigorous than ever before.

In conclusion, permit me to express the opinion that the ultra-ism, into which the profession of the present day is so prone to run, might be, with safety, displaced by a revision of some of the older teachings and a following of some of the rules that are now considered obsolete.

Respectfully yours, J. L. SUESSEROTT.

CHAMBERSBURG, February 13th, 1884.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF NEW YORK.

Meeting, November 6th, 1883.

TAIT'S OPERATION.

DR. W. T. LUSK presented specimens and related a case as follows: The patient was sent to the hospital by Dr. Buchanan Burr, with the message that hers was "a good case for Tait's operation." She was twenty-four years of age, had been married

four years, was sterile, had previously always been well, and had menstruated regularly and without pain. Within a year past she had begun to suffer from paroxysmal pains, commencing on the left side of the pelvis and extending upward across the abdomen and down the left leg. The pains were excessively severe, came on suddenly, lasted for several hours, and then entirely disappeared : the patient would feel that she was entirely well, when another paroxysm would occur before the lapse of twenty-four hours ; they usually came on at night. Dr. Lusk, on examining her, found a tumor extending across the left half of the pelvis, and having its origin behind the uterus. There seemed to be obscure fluctuation : the consistence of the tumor enabled him to exclude fibroids : the absence of tenderness made it almost absolutely certain that it was not the result of pelvic cellulitis. The diagnosis then lay between a dilated Fallopian tube and a small ovarian cyst. The fact that the tumor was firmly adherent rendered ovarian cyst doubtful : in that case adhesions rarely formed while the cyst was small. On the other hand, it seemed impossible that the Fallopian tube could have attained to the apparent dimensions of the tumor. The patient was kept in the hospital a month to make sure that the pains were not hysterical, and that the tumor was not diminishing. As no relief was afforded, however, the patient was informed of the risks of an operation, and of the possibility of failure to produce relief. She concluded to have it done. An incision two inches and a half in length was made, the finger introduced, and the tumor recognized to be a dilated Fallopian tube. The extremity lay directly behind the uterus, and was firmly adherent to that organ. The entire tube, thus bent upon itself, filled the left side of the pelvis, and was, throughout its entire extent, adherent to the pelvic floor. The adhesions were separated with difficulty with the fingers. It was necessary to increase the abdominal opening to four inches in length before the tumor could be withdrawn. The appearance was very much like that of large intestine, and its true nature was determined beyond doubt only after careful inspection. Sponges were packed into the cavity where the tumor had lain to absorb the slight amount of oozing which was taking place. A ligature was applied around the pedicle, and the tube removed. All bleeding had ceased when the abdominal wound was closed. The patient made an excellent recovery, no untoward symptom having developed ; pain had since entirely disappeared. The origin of the trouble in the first place could not be explained : the patient had previously been healthy, had never suffered from pelvic peritonitis nor cellulitis, nor had she had venereal disease. There was said to be consumption in the family. The diseased tube contained pus ; the other was healthy and was not interfered with.

DR. LEE thought Dr. Lusk was to be congratulated on his ability to make an exact diagnosis in so obscure a case, and on the result of his operation. In such cases it was sometimes impossible to

make the diagnosis before the abdomen had been opened, and, in obscure cases in which patients suffered as Dr. Lusk's did, he believed we were justified in making the exploratory incision, and, finding our suspicions verified, in removing the diseased tube, this being the only means which offered the patient relief from intense suffering. He referred to a similar case in which he proposed to operate if the patient's consent could be obtained. She had first consulted Professor Braun, of Vienna, who believed her symptoms to be due entirely to constriction of the cervical canal, but she had an attack of pelvic inflammation following the examination, and could not undergo treatment. On her return to this country, she consulted Dr. Lee, who recognized a tumor evidently connected with the Fallopian tube and probably with the ovary, and recommended Tait's operation. The diagnosis was confirmed by Dr. Thomas and Dr. Emmet. The tumor had since suppurated and discharged pus through the rectum.

DR. R. WATTS inquired whether the discharge of pus should not be taken as an indication of strong adhesions, and contraindicate the operation.

DR. LEE replied that we might in any case be compelled to abandon the operation on account of the extent and firmness of the adhesions, but this could not be positively predicted from the patient's previous history. He had operated successfully in one case in which there had been repeated attacks of peritonitis, and on two occasions the abscess had discharged into the rectum. Dr. Hunter had had similar experience.

DR. LUSK remarked that nothing could have been more adherent than this tumor to the uterus, and he would probably have abandoned the operation had he not witnessed the immunity from danger which attended the digital separation of adhesions in the hands of Mr. Tait.

In reply to a question by Dr. Polk, DR. HUNTER said he agreed with the remarks made by Dr. Lee, that in such cases an abdominal incision was justifiable in order to arrive at a diagnosis. About two weeks ago he operated upon a patient who had recently returned from England, where she had been under treatment by Sir Spencer Wells for ovarian neuralgia. Galvanism and many other remedies had been employed, but without success. The patient had been married six years, had never been pregnant, menstruation had been steadily diminishing in quantity, and, during the past six years, had been attended with intolerable pain. During the past two years the pain commenced some time before, and continued for some time after the flow, the interval of rest being but a few days. The uterus was normal and not flexed. At the first examinations the ovaries could not be felt: subsequently enlargement of one ovary was made out with tolerable certainty. At the operation the right ovary was found much enlarged and cystic; the left was cystic, but only slightly enlarged; both tubes were slightly diseased. The ovaries and tubes were removed. The patient was doing well; there had been scarcely any rise of temperature, and pain had disappeared. It was, of course, too early to determine the permanence of the result. Dr. Hunter had found Fergusson's speculum useful in cleaning out the cavity with sponges on handles. He mentioned some cases in which the ovary was not diseased, but firmly bound down by adhesions, which in itself was sufficient to account for the pain from which the patient suffered during menstruation. In two instances in which the ad-

hesions were very firm, and required great force in breaking them up, considerable bleeding took place. One of the patients recovered. In the other, secondary hemorrhage occurred, and the patient died. The wound was reopened, but the bleeding was too deeply situated to be arrested. To illustrate the difficulty of diagnosis, he also mentioned a case in which enlargement of one ovary was diagnosed by several examiners, and at the operation the opposite one was found to be the larger.

DR. J. BYRNE gave the symptoms and physical signs in two cases similar to Dr. Lusk's, in which he had contemplated Tait's operation, but hesitated somewhat because of possible extensive and firm adhesions. He was glad to have this point brought out in the discussion.

DR. POLK asked the members whether they had had any experience with Hegar's method in the diagnosis of these cases: viz., exploration of the posterior surface of the uterus with two fingers in the rectum, the organ being pulled down with the tenaculum in the vagina.

DR. BYRNE thought there would be great danger of bringing on inflammation by such a procedure. One patient of his nearly lost her life from peritonitis brought on by manipulation during repair of the cervix.

DR. POLK said that pelvic inflammation would certainly be liable to result from Hegar's procedure, but he thought the risk might be assumed in cases in which opening of the abdomen would otherwise be necessary for diagnostic purposes.

OÖPHORECTOMY AND OVARIOTOMY.

DR. B. F. DAWSON presented specimens with the following histories: The first patient consulted him about two months ago, suffering from general nervous disturbance and neuralgic pains in the pelvis. The slightest pressure about the ovaries gave acute pain. The patient was subject to epilepsy, and had a seizure during the examination. Cystic degeneration of the right ovary was recognized. At the operation on the left ovary there was also found a small parovarian cyst, and this ovary was also removed. The patient made an excellent recovery, and was now sitting up.

Dr. Dawson also presented a second ovarian cyst, removed on the 24th of September. He first saw the patient a year ago, when he told her it would ultimately require removal, and he kept her under observation until symptoms called for operation. Considerable difficulty was experienced in detaching some firm adhesions at the base, but the patient made a very good recovery. The clamp was employed to secure the pedicle, on account of exceedingly large vessels making ligation seemingly injudicious. The cyst was of the dermoid variety, and contained a large mass of hair, but no bone or teeth.

A third specimen was that of an ovarian cyst of the papillomatous variety removed from a patient at the Woman's Hospital that afternoon. The tumor was recognized to be a polycyst with considerable adhesions. The diagnosis was confirmed by Dr. Emmet. About a gallon of fluid, containing odorless purulent fluid, was withdrawn from the three compartments into which the

cyst was divided, and the operation was difficult and protracted on account of extensive and very firm adhesions to the intestines, uterus, and bladder, requiring the actual cautery and numerous ligatures. Of a large number of operations which the house staff had witnessed, in none had they seen firmer and more extensive adhesions. Owing to the uterine adhesions being almost inseparable, Dr. Dawson thought the only safe procedure was to apply his large clamp to the thick pedicle formed by the fundus of the uterus. The patient lost but little blood, and reacted well.

DR. HUNTER asked whether it would not have been better, finding the adhesions so extensive, not to have proceeded with the operation.

DR. DAWSON replied that the firmness of the deeper adhesions was not recognized until the upper ones had been detached. In answer to a question by Dr. Lee, as to whether it would not have been better to extirpate a portion of the uterus than to apply the clamp, he said the patient was not in condition for the operation to be protracted by such a procedure; moreover, his experience with extirpation of the uterus in one case had prejudiced him against its repetition.

PROCEEDINGS OF THE NEW YORK ACADEMY OF MEDICINE.

Meeting, December 20th, 1883.

ALEXANDER S. HUNTER, *Chairman.*

DR. W. M. CHAMBERLAIN read a paper on

MALPOSITION OF THE UTERUS.¹

DR. F. P. FOSTER did not believe that any pessary acted as a lever. What a pessary does is this: A retroversion pessary acts almost always, if not always, by carrying the loose posterior wall of the vagina backward, usually somewhat upward, dragging the cervix in the same direction, and causing the body of the organ to take an opposite direction. In order for the retroversion pessary to act by making direct pressure upward and forward on the body of the organ, as stated by most authors, the pessary must be exceedingly long—much longer than he had ever seen in use—and, besides, it must have an exaggerated curve, even more so than that illustrated by Dr. Noeggerath's instrument which had been shown.

In the next place, if the limbs of the pessary are far enough apart to allow the cervix to hang free between them, it would be so wide that it would spread the utero-sacral ligaments apart, and, whatever support it might give temporarily, the last state of that woman would be worse than the first. For, as soon as its use is discontinued, the ligaments will be found still more relaxed than at the beginning of the treatment. As they constitute a sort of obstacle to retroversion of the uterus, there would be no natural

¹ See page 337.

tissue left to counteract such displacement. How, then, can stretching of the utero-sacral ligaments be avoided on the one hand, and, on the other hand, an instrument be employed which is small enough to go up between the ligaments, and not push them apart? Dr. Chamberlain has brought out this objection in all pessaries, and stated that their bearings should be small, and make pressure upon a limited area. A pessary small enough in its upper arch to pass between the utero-sacral ligaments without stretching them would not probably be well borne if worn continually. Now, that objection and several others may be gotten over by using pessaries which are not worn continually. Dr. Foster never employed a pessary which the patient could not introduce and remove herself, and she is always instructed to remove it at night. By this intermittent action, it is perfectly possible to use a pessary with small arms which pass readily between the utero-sacral ligaments, if necessary; but it is not often necessary.

The form of instrument which he used gave him opportunity to add to what Dr. Chamberlain had said with regard to celluloid and copper wire. He had used this material constantly ever since Dr. Chamberlain introduced it, and nothing else, and he had not known it to deteriorate by keeping. He had one patient who had worn one of these pessaries constantly ever since Dr. Chamberlain introduced the material, and it was yet in good condition. The instrument which he always used for retroversion is made for each patient. He had the instrument-maker simply make a rod of copper wire covered with celluloid, and out of these straight rods, which are from twelve to eighteen inches in length, he fashions a pessary for each patient, bending it in the middle for the pessary bow, then having it run out to the vaginal outlet almost straight, and then it curves down abruptly to form the part which impinges against the anterior surface of the perineum, one leg bending backward and the other curving forward. In this way, the lever action is avoided.

In regard to anteversion, he thought, with Dr. Chamberlain, that either anteversion or anteflexion is very seldom relieved by treatment. He did not believe that anteflexion could be at all influenced by mechanical treatment except by stems, which he never used. He thought it unwarrantable to treat these displacements mechanically. The only cases proper for treatment are those in which the physiological anteversion or anteflexion of pregnancy gives rise to a good deal of suffering, and, under such circumstances, the pessary may be used as a temporary measure until the uterus is sufficiently large to rise out of the pelvis.

In some instances, the pessary is of use where the displacement is not very prominent before it is applied; but it is not for the purpose of overcoming the displacement that the pessary is employed. Sometimes the pessary is beneficial by actually increasing the displacement. A great majority of displacements are due to contraction of inflammatory exudations. Now, if you have a band, say of organized material extending from the body of the uterus back to the promontory of the sacrum, you can readily imagine that there is a constant strain upon that, which gives rise to pain on any movement of the body, particularly any sudden movement brought to bear upon this sensitive pathological structure. Under such circumstances, a pessary can be so adjusted as to prevent any such sudden movement, and even exaggerate the existing displacement.

DR. PAUL F. MUNDE spoke first with regard to the normal position of the uterus. It seemed to him very curious that on this point so many authors disagree. For the normal position is a variable one, and it is not exact to say that it is either anteversion or antelexion, because it is simply one of degree, according as to whether the bladder and the rectum are full or empty, the patient standing or lying, expiring or inspiring, quiet or in motion. The position is a movable one, but in general it may be stated that it is one of ante-curvature with a slight degree of anteversion. The position described by Schultze and Fritsch as normal he had never found. The position which he had regarded as normal was that in which the axis of the vagina and that of the uterus meet each other at a slightly acute angle, and from this it is movable one degree forward and more backward. In other words, the position which Dr. Chamberlain had described was the one which he would accept, as it would give sufficient mobility for a remove one degree anteriorly and two degrees or more of normal mobility posteriorly. Dr. Mundé was unable to understand how Fritsch and Schultze had found the uterus lying flat upon the vaginal wall. Should he find a uterus in this condition, he would call it an antelexed pathological uterus. The position of the uterus varies according to whether the woman is standing or lying. In the standing position, there is more anteversion than antelexion, and the uterus is a little lower in the pelvis than when lying, and the fundus is slightly turned toward the abdominal wall.

Speaking of the relative frequency of the different malpositions of the uterus, he had found that antelexion, which he would call a malformation of the uterus, was the most frequent displacement in nulliparæ and the unmarried. The next most frequent form is anteversion with descensus occurring in married women, and the next most common displacement is retroversion with or without retroflexion in women who have had children.

He was thoroughly of the opinion, as had already been said, that the uterus first settles in the pelvis because the ligamentous support is relaxed, then the fundus gets below the promontory of the sacrum, then begins to be retroverted, and afterwards retroflexed more or less. Occasionally retroflexion exists primarily.

The significance of these various displacements is a point of a good deal of interest because it bears upon the question of mechanical treatment. Antelexions *per se* produce no symptoms. The congestion accompanying them, or the spasmodic contraction may produce dysmenorrhea, but the antelexion itself does not do so. He did not believe that the antelexed fundus pressing upon the bladder ever produced any particular symptom.

Anteversion will produce symptoms only in cases where the uterus is at the same time displaced downward to the extent of one degree or more. Symptoms are produced only when there is descent of the organ in both forms of anterior displacement.

With regard to retroversion, symptoms are present in a large proportion of cases, but not in every one by any means. A retroflexion produces more symptoms than does retroversion, because there is more congestion of the uterus, and also it is more apt to be accompanied by prolapsus of the ovaries.

There is another displacement which is not mentioned very much, but which produces symptoms, the chief of which is sterility. It is a combination of retroversion and antelexion. He had always taken it to be a congenital condition, the ligaments never having developed in their proper relations.

With reference to the causation of these different displacements, he thought that Dr. Foster had touched the key-note with regard to one cause, namely, the contraction of one or other of the ligaments, and chiefly the utero-sacral and lateral ligaments in the production of retro- or lateral displacements. He thought that a few anteversions depend on the contraction of the lower folds of the utero-sacral ligaments just above Douglas' pouch. These cases, however, are exceeding rare. He had seen retroflexion and retroversion in young unmarried women, but they are of rare occurrence.

With regard to pessaries, he should at once take issue with Dr. Foster concerning the lever principle. He would agree with him perfectly that one effect of the retroversion pessary was to push the posterior vaginal wall backward and therefore antevert the uterus, but he could not help feeling that when the Albert Smith or Hodge pessary is used, lying loose in the vagina with its anterior bow loose, pressed down upon in the erect position by the full or the empty bladder, and by the full or empty intestines, that this instrument must necessarily tilt up at the other extremity, and in that way he thought it illustrated the lever principle.

He uses but few anteversion pessaries, but that which he favors most is the open cup of Thomas. For antelexions he uses the closed cup, as it prevents the cervix and fundus from approaching each other. He had seen good results from gradually heightening the anterior portion of the cup of the pessary, and the displacement was thus gradually overcome. For cystocele he finds the larger-sized Gehrung the most efficient of all supports.

For at least ninety-five per cent of retroversions he prefers the Albert Smith pessary. If there is flexion, he prefers a pessary with a thick bulb; a simple Albert Smith pessary with a thick bulb behind. He might change the curve of these pessaries very often, but they were substantially the same instrument. Dr. Chamberlain and Dr. Foster had both said that sharply curved pessaries did not work well. To this Dr. Mundé thought there might be some exceptions, but as a rule he was of the same opinion. Of late he had found the Cutter pessary to serve him a very useful purpose in the treatment of retroflexion. He admitted that there was a tendency to push the cervix forward, but if the curve at the lower end of the instrument is almost at a right angle, this tendency will be mostly overcome. The Fowler pessary he had used, but he did not like it.

With regard to the substance from which pessaries are made, he had tried celluloid, and had had the same experience as Dr. Chamberlain, namely, that the instruments cracked and became unfit for use. Consequently he had returned to the use of hard-rubber.

DR. A. S. HUNTER said, with reference to the pessary acting as a lever, that it must necessarily do so if there is a fulcrum. If the broad ligaments are tense, the uterus high up, and the vagina keeps positively its contracted condition, then he could see how the Albert Smith pessary acts upon the lever principle. On the other hand, when the ligaments are stretched, and the vaginal wall is relaxed and depressed, he could not understand where the fulcrum could exist, and therefore the instrument does not act as a lever.

DR. CHAMBERLAIN, in closing the discussion, said he had no criticisms to offer to Dr. Foster's remarks except possibly upon the

lever action of the instrument. He felt confident that there is such a lever action, perhaps not to a great extent, nor exerted by all instruments.

With regard to the points made by Dr. Mundé concerning the normal position of the uterus, they were substantially what he had stated in his paper, and did not conflict with any position which he attempted to take. He thought there is a good deal in the idea that the position of the uterus in individual subjects is largely influenced by the contour of the abdominal wall, the impulse obtained from the descent of the diaphragm, together with the degree of the tension and weight of the intestines.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, December 6th, 1883.

The President, R. A. CLEEMANN, M.D., in the Chair.

DR. W. GOODELL exhibited two

CYSTS OF THE PAROVARIIUM,

And remarked: Both patients got well; he indeed had never lost a patient from whom he had removed a parovarian cyst. In both cases a correct diagnosis was made previous to the operation. One interesting diagnostic point was the complete absence of the *facies ovariana*. The color in the cheeks was good, and the countenance was free from the anxious expression present in cases of ovarian tumor. One tumor had existed for ten years, the other for one. Another important point in the differential diagnosis is not only the flaccidity of the tumor, but its variable degrees of flaccidity. Upon inspection, it is seen to reach to the sternum, and seems to occupy a large portion of the abdominal cavity, but when the hands are placed upon its sternal edge, it can be compressed to the level of the umbilicus. An ovarian cyst, on the contrary, is hard and uncompressible. Exceptions to this rule are very rare, that is, either a tense parovarian cyst or a flaccid ovarian one. A third important distinguishing point is the long time—ten years in one case—which the tumor existed, and, further, without marked deterioration of health. After being tapped, these tumors usually refill, but occasionally they do not, and a cure is thus brought about. The fluid withdrawn has been in every case limpid and generally colorless, but it has sometimes had in his experience an emerald tint. These tumors are generally free from serious adhesions, but if, in an operation for the removal of one, adhesions should exist where, for any reason, their forcible separation would be inadvisable, or the cyst were intraligamentous,

he would not hesitate to leave the adherent portion of the cyst-wall or the whole cyst itself, after making a big hole in it, as the fluid it secretes is bland and unirritating to the peritoneum.

Any one examining one of these cysts for the first time would consider it to be of ovarian origin; for it is only by patient search that the ovary can be found spread out over the cyst-wall. The microscope will decide with certainty in any otherwise doubtful case. The tumor is covered with a beautiful net-work of veins.

When a cyst of the parovarium exists on one side, the ovary of the opposite side is usually found to be diseased, and should be removed. In these cases the remaining ovary was seen to be enlarged, and the site of a small ruptured cyst was pointed out. The Fallopian tube was also enlarged, and the terminal vesicle of the Fallopian tube or the hydatid of Morgagni was enlarged and cystic. This hydatid sometimes attains the size of an orange, and often ruptures spontaneously without any bad effects. A few years ago one of these small cysts ruptured while he was making an examination of the patient to ascertain its character.

DR. GOODELL exhibited a

CANCEROUS WOMB REMOVED PER VAGINAM.

In view of the very fatal statistics of the operation for the removal of the womb for the radical cure of uterine cancer, he had been unwilling to perform it. In most of the cases where the disease had been seen early enough by him to give a chance of success, the patient had been unwilling to take the risk. On one occasion, when every preparation had been made to operate, the patient had a convulsion, and an examination of the urine showed a high proportion of albumen, in consequence of which he refused to operate. About a month ago, Dr. Charles W. Dulles called him in consultation to see a patient in whom the carcinomatous condition was limited to the anterior lip. The womb was movable. The case was put frankly before the patient, and all its dangers pointed out. The choice of them being given to her, she decided, after due consideration, to take the risks of the radical operation. The operation was not as difficult as he anticipated.

The first step in the operation was to scrape away all cancerous tissue, and to sear the remaining surface with Paquelin's cautery. The vagina was then thoroughly cleansed.

A stout thread was passed through the cervix to draw down the womb, instead of using a volsellum, the handles of which would be in the way. A circular incision was made around the cervix, and the tissues were stripped up anteriorly and posteriorly to the reflection of the peritoneum, and laterally to the insertion of the broad ligaments. Finally the peritoneum was opened and the womb retroverted into the vagina by means of the obstetric crotchet, passed over the fundus. A strong thread was now passed through the body of the uterus, by means of which to manipulate it more easily. A ligature was now passed around the broad

ligament of the right side and secured it *en masse*, and a second double ligature was passed through it and tied on opposite sides. This side of the broad ligament was then divided, the uterus drawn down, and the ligament of the left side secured in a similar manner and divided. The vaginal wound was closed and dressed with iodoform and cotton. A frank peritonitis set in on the third day, and proved fatal on the fourth. The result made him doubtful whether the operation is ever justifiable; he indeed felt disposed to avoid it whenever possible.

DR. E. E. MONTGOMERY inquired about the feasibility of using the galvanic wire *ecraseur* to divide the broad ligament, keeping the wire at a low red heat and dividing the tissues slowly, and avoiding the necessity for a drainage tube. Another method which he had been revolving in his mind was by means of the galvanic knife to dissect out the uterus, leaving the peritoneum intact, thus imitating to a certain extent the operation of Dr. Marion Sims of scraping and the use of zinc chloride.

DR. GOODELL thought that Dr. Montgomery's galvanic wire would get too hot as the loop became small, and would then divide rapidly like a knife, and incur the danger of secondary hemorrhage. He fears that the steam generated by the hot wire would penetrate the peritoneal cavity, and have an irritating effect. He has a galvanic cautery battery, but has not used it since the introduction of Paquelin's benzolin cautery, as he finds the latter far more handy and manageable. He thinks Dr. M.'s suggestion of shelling out the uterus a very good one.

REMOVAL OF THE UTERINE APPENDAGES FOR DYSMENORRHEA.

DR. MONTGOMERY exhibited, through the courtesy of D. W. H. Warder, the uterine appendages which had been removed from a young lady for the relief of dysmenorrhea, which had resulted in physical and mental failure. Menstruation had commenced at the age of fourteen years, had always been painful, and had developed hysterical manifestations. Bathing at the sea-shore had at one time stopped the periods for three months; after this her health failed, her mind had been seriously affected for the last three years, and she would run away or do herself some violence at the menstrual periods, if not closely watched. Examination: the uterus enlarged and tender; there was profuse leucorrhea. The os uteri was dilated and the uterine cavity scraped and cauterized with carbolic acid and bromides, etc., used internally, but no improvement resulted. The ovaries were removed to-day through abdominal section; catgut ligatures were used. The ovaries are very much enlarged and contain small cysts. The abdominal wound was closed with silk sutures and covered with an impervious dressing of collodion, cotton, etc.

HYSTERO-EPILEPSY AS A COMPLICATION OF PREGNANCY.

DR. WILLIAM H. SHIPPS, of Bordentown, N. J., sent the following contribution:

Briefly defined, Hystero-epilepsy is a term applied to an abnormal neurotic condition in which are manifested certain phenomena

characteristic both of hysteria and epilepsy. Out of 276 patients confined at La Salpêtrière Hospital, Paris, under treatment for various nervous affections, 32 were diagnosticated by Beau, a careful observer, as suffering from this disease. Among this number the malady assumed either a distinct or combined form, hence he very wisely groups the cases into two classes. In the first, the hysterical seizures and epileptic fits remain distinct one from the other; to this form he adapts the term given by Landouzy, and designates it as *hystero-epilepsy with distinct crises*.

In the second class, and the one of which this article furnishes an illustration, the hysterical and epileptic seizures are coeval, both developing at the same time; to this form the name of *hystero-epilepsy with combined crises* has been given. The object of this paper is, not to enter into a consideration of the disease as it is met with in general practice, but simply to examine it as a complication of pregnancy, a standpoint from which fortunately we are rarely called to view it.

During the early part of March, 1883, I was called to attend a woman said to be in a fit. Arriving at the house I found, lying on a bed, a young woman apparently seventeen or eighteen years of age, of fairly vigorous physique, who was striving against the united efforts of two or three neighbors who sought to prevent her doing herself bodily harm in the violence of her struggles. Examination showed entire loss of consciousness, eyes open and staring, pupils widely dilated, frothing at the mouth, which was then tightly closed, pulse full and bounding. Inquiry elicited that during the day she had been visiting a friend, herself the subject of some spasmodic affection, and whilst in her company became greatly exercised on some trivial occurrence and in this state of excitement returned home, which place she no sooner reached than she was seized with a convulsion. Her husband informed me that she was in the third month of pregnancy, and that prior to this morning had on one or two occasions attacks somewhat similar, though of less severity. I at once injected hypodermatically one-third grain of morphine, which in a short time was followed by a total disappearance of all spasmodic action, a state of stupor supervening from which in the course of three or four hours she aroused apparently well as ever. On the day following I was called to see her and found her in a state of high nervous excitement, sobbing and deploring the presence of some impending danger which she, in her imagination, insisted was threatening her. In a short time the stage of muscular contraction, loss of consciousness, stupor, etc., took the place of hysterical symptoms, finally terminating as before in a return to her normal condition.

Without attempting to follow the case step by step, taking in all its details and noting the many and peculiar phases through which it passed, it is interesting to note that prior to September 23d, 1883, covering a period of 200 days, not a day passed without the occurrence of one or more paroxysms. At times the hysterical phenom-

ena would be most marked and usher in the attack: then again the epileptic fit would take the precedence, always, however, accompanied by the undeniable imprint of the dual disease hystero-epilepsy. In the inter-paroxysmal period she enjoyed for the most part average good health.

On the morning of September 23d, I was asked to see her in an attack of more than usual gravity. When I reached the house she was profoundly unconscious and had been so for several hours. The time for her approaching labor being near, I made a vaginal examination, but found no evidence of commencing uterine action. I ordered a hot mustard bath, mustard to the extremities, and bromides the moment she should be able to swallow. In the evening when I again called, her condition was apparently unchanged. I then ordered a blister to the nape of the neck and left the patient, to return early in the morning. At 7 A.M. a messenger called stating that the woman was now perfectly rational and to all appearances in labor. I at once responded and found that she was having bearing-down pains of moderate intensity at intervals of five or ten minutes, mouth of womb dilating, vertex presenting. I remained by the woman's side until 3.45 P.M., when the child, a healthy female weighing nine or ten pounds, was born. The labor did not differ from ordinary labors, except that it was only by the utmost vigilance that the woman was prevented falling into one of her accustomed attacks. After the birth of the child, I gave it to the mother, at the same time remarking to her that, as she valued the life of the child, under no circumstances to allow herself to have another convulsion. She promised faithful obedience, and up to the present has not shown the first indication of her old trouble. It should be mentioned that during the entire period the patient was under observation, she had daily taken large doses of the bromides and other nervines, without any effect save perhaps in ameliorating the number and violence of the paroxysms. The case is interesting on account of the rarity of the disease as a complication of pregnancy; its persistence throughout the entire period; its resistance to all remedial measures, and the final disappearance of all symptoms after the termination of labor.

Two queries very naturally present themselves: What was the exciting cause of the attacks? Would the induction of premature labor in this and similar cases be justifiable?

In answer to the first query, I attribute the attacks to an action upon the brain and spinal cord, reflex in its nature and developed or excited by the fetus in utero.

The happy termination of the case would seemingly offer a negative to the second query; but better judgment will, I think, suggest the wisdom of the operation and the danger of refusing to employ what, theoretically at least, offers the only chance of relief: at all events in a similar case I would most certainly have recourse to the operation and expect from it the best results.

Stated Meeting, Thursday, January 3d, 1884.

The President, R. A. CLEEMANN, M.D., in the Chair.

BATTEY'S OPERATION.

DR. E. E. MONTGOMERY corrected a statement made in the report of the case of oöphorectomy narrated by him at the meeting of December 6th, 1883. *Silk ligatures*, not catgut, were used to tie the pedicles.

The convalescence of the patient had been very satisfactory. The temperature did not rise above 99° F. In performing this operation no antiseptic was used; all instruments, sponges, etc., were washed in boiling water, and boiled water was used in washing the wound and abdominal cavity. The mental condition of the patient was such that she was kept constantly under mechanical restraint, and on one occasion, when she had been left alone for a few minutes, she tore all the dressings off of the wound, and at the next visit it was found bare, but no bad result followed. The sutures were removed on the eighth day. Some pain in the lower part of the abdomen and slight fever commenced on the twenty-fifth day, but she has since again improved. Her mental condition is at the present better than it had been for a year before the operation, and she can now converse rationally.

SEPTICEMIA AFTER ABORTION.

DR. W. H. PARISH reported the following case: A young woman, twenty years of age, came into the Philadelphia Hospital in the finishing stages of an abortion which had been coming on for some days. The cause was unknown, but was probably instrumental. At the time I first saw her, three days after her admission, her temperature was 103° to 104° F., and pulse 150. She had had a chill before admission, her abdomen was distended and tender, and the uterus was very sensitive. The right parotid gland was swelled and painful. It continued to enlarge, and fearing septicemia, he gave fifty grains of quinine daily by rectum, as the stomach was too irritable to retain it: one ounce of whiskey was given by the mouth every hour, day and night. A small quantity of morphia was given to relieve the abdominal tenderness. The pulse and temperature fell rapidly, but the gland continued to be swelled and painful; it was quite hard, but was discolored. There were no chills now, but fearing the presence of pus, he made an opening by Hilton's method, incising the skin and using a director to tear an opening through the gland tissue; this opening was enlarged by passing a pair of forceps, closed, along the groove of the director and withdrawing them opened. This opening gave exit to two or three fluid drachms of pus. The gland now improved in appearance, but another abscess opened behind the gland and discharged freely. The two abscesses did not communicate. The patient is now convalescent.

TRANSACTIONS OF THE WASHINGTON OBSTETRICAL AND GYNECOLOGICAL SOCIETY.

Stated Meeting, December 7th, 1883.

The President, DR. S. C. BUSEY, in the Chair.

DR. J. C. BEALE read a paper on

PERSISTENCE OF LIFE IN INFANTS WHO HAVE NEVER BREATHED.

In the first case seen by Dr. Beale he was sent for to see a woman confined by a midwife. The patient died from hemorrhage just after the arrival of Dr. B., and one hour and a half after delivery. The midwife reported the birth of dead twins, and when asked to show them she replied, "I put them in a bucket and out in the shed, because they smelled so bad." On examination, one infant was found to be dead and the other feebly respiring. Every effort was used, without avail, to restore the child. It had been in the bucket about three hours, and the weather was very cold, it being the month of December.

In his second case there was also hemorrhage, which demanded his immediate attention. He thought the child was dead, and therefore did not pay attention to it until the safety of the mother was assured. Efforts were then made to revive the infant, and two inspirations actually rewarded the labors of the doctor, but beyond these he was unsuccessful. About eighteen minutes elapsed after birth before he was able to leave the mother and give attention to the child.

The third case was an infant born of an unmarried woman, and fifteen minutes elapsed before the child was made to breathe. The cord was knotted, which probably accounted for the suspended animation of the infant.

In the fourth case the labor had been hard, the breech presenting. The body was brought down, but difficulty was experienced in delivering the head. The pulsation in the cord had ceased, and the child was believed to be dead. After attending to the mother, the child, which had been removed into an adjoining room where the temperature was low, was examined. Dr. Beale, having a pair of scissors in his hand, pricked the child, and was surprised to see slight movement follow. Calling attention of the physician in charge to the probability of saving the child, efforts were made to resuscitate, which proved successful. Fully twenty-five minutes had passed since the birth of the child before any efforts were made to restore it.

In the fifth case nearly an hour was occupied in the resuscitation of the infant.

Dr. Beale called attention to the fact that, in the mammalia, the fetus which has not breathed can resist death from submersion much longer than those which have done so. Pups and kittens, immediately after birth, may be kept under water twenty-eight minutes with impunity; when five days old they perish in sixteen minutes; and when fifteen days old they die as rapidly as other warm-blooded animals of any age. The human fetus can probably live longer without respiration than any other mammalian fetus. He cited cases to prove that infants may be revived even under the most inauspicious circumstances. In one case a woman concealed her pregnancy, and when the child was born she buried it in a sand-pit, where it remained half an hour before it was removed and resuscitated. In another case the child was buried a foot under ground and was not exhumed for five hours, and then a surgeon worked on it two hours before evidence of life was manifested. In another case the infant was exhumed after an hour and recalled to life.

Life was discovered in another child, supposed to be dead, twenty-three hours after it had been born and placed in a coffin. It could not be saved. Another child was saved after being under ground three-fourths of an hour.

From facts noted in the cases mentioned, Dr. Beale concludes that the colder the atmosphere to which an infant is exposed, the longer may life be sustained and the greater the chance of resuscitation. The ability of the child to resist asphyxia explains why, in cases of death of the mother, it may be saved by abdominal section even after the lapse of considerable time. The lesson to be learned from these facts is that asphyxiated infants should not be placed near a fire or kept in a warm place, as their chance of living is better when they are placed in a cool locality.

Dr. Beale referred to the legal aspects of the cases under consideration, and discussed the question of survivorship when both mother and child die. Also the subject of still-birth and the evidence to sustain that position. He gave rules for making the autopsy of infants found dead, and concluded his paper by trying to explain how the circulation of an infant is maintained in the absence of respiration.

DR. A. F. A. KING.—The question of interest was, how is it possible for the still-born to live for hours without breathing. The condition appeared to be analogous to that of trance, with the exception that the child had never breathed. During labor the circulation was obstructed by the contractions of the uterus, and every child was born in a state of partial asphyxia, produced either by pressure on the cord, placenta, or brain. In regard to the contractions of the heart, we all know that the carbonic acid in the blood is a stimulant to the involuntary muscular fibres. Thus, in hanging, there is a strong action of the heart before death; and it might be that this condition of the blood exerts the same influence

on the heart of the child. Because we do not hear or feel the heart's movements, it is no proof that there is not some feeble contraction. In the course of his practice Dr. King had seen some remarkable cases where there was, apparently, no life, and where he had been able to resuscitate children after working an hour and a half. This was in the early days of his practice when he used to give ergot, and he had no doubt that many children were destroyed by that drug. He thought that an instrument might be devised for inflating the lungs. It might be made on the principle of the hypodermic syringe, the point being passed into the trachea through the integument, the epiglottis being held down by the finger, air could be forced directly into the lungs. Or oxygen might be used pure or mixed with air. We might also supply oxygen by rectal injections or by the skin. In cases of resuscitation after burial, or after leaving the child in a cold room or upon the floor, the question was whether the cold air did not, after all, produce the favorable result. It was good practice to open the window, when endeavoring to revive these cases. Again, some children were no doubt destroyed, or resuscitation prevented, by ligation of the cord, for it was a well-known fact that this act leads to distention of the right heart, by damming back the blood. Hence many children had been saved by cutting the cord and permitting it to bleed.

DR. MAGRUDER said that in one case, a difficult labor with breech presentation, Dr. Beale and himself felt little hope of saving the child, and, after working for over an hour, were on the point of giving up, but as the mother had lost five out of six of her children, they were anxious to save this one, and continued their efforts which were, finally rewarded with success. He found that he had resuscitated seven children during the last six years. In the majority of cases he had been unable to detect cardiac movements; the children were cold, and he, as a rule, laid them on the floor or a table perfectly naked, which seems a benefit. He also resorted to friction, douches of cold water, and artificial respiration.

DR. J. T. JOHNSON said the paper contained some points for the obstetrician, viz., to teach him the wisdom of not being too hasty in effecting delivery, when afraid of losing the child from pressure on the cord, for Dr. Beale had shown that there was a better chance when the child had not breathed, before efforts at resuscitation were made. In one case of prolapse of the cord, the pulsations had ceased, and he was too hasty in delivering, tearing the soft parts of the mother. He felt sure the child was dead, but it cried at once. Septicæmia resulted from the laceration of the parts which he could not stitch together, because of their bruised condition. In a case of breech-presentation, he thought that the child had been lost by efforts to secure the rapid delivery of the head. It did not seem to him, in view of what Dr. Beale had said, that there was any necessity for such great haste in these cases. Another lesson enforced by Dr. Beale was the necessity of resorting promptly to the Cesarean operation to save the child, when the mother has died in labor. He concurred in the necessity for prolonged efforts at resuscitation, and cited a case of forceps delivery, where it was thought the child was dead. Attention had been given to the mother who had suffered a laceration of the perineum, and was bleeding freely. Just as the attendants were about to leave, the child, who had been pushed under the bed, began to cry.

DR. KLEINSCHMIDT, in corroboration of the fact brought out by Dr. Beale, that a child might live a long time without breathing, cited a case where the child was born with the membranes intact, the mass lying between the mother's legs half an hour, yet, upon rupture of the sac, the child promptly began to breathe. He inquired of Dr. Beale as to what would be taken in our courts as evidence that a child had breathed.

DR. BEALE said he did not know.

DR. KING said, as to the evidence of life or death of the child, as given in court, there was one comfort to all of us, viz., that the lawyers were as ignorant of medical jurisprudence as the doctors. The hydrostatic test, if *properly* applied, showed whether there had been *perfect* breathing. There was a distinction to be made between *perfect* and *imperfect* breathing. In the latter, the air only entered the bronchi. The hydrostatic test was to cut the entire lung into small pieces; to wrap each piece in a cloth and squeeze the air out. If inflation had been perfect, all the air could not be squeezed out, and the pieces would float.

DR. BUSEY inquired as to the experience of members with the different methods for resuscitation.

DR. KING said he took hold of the knees with one hand, with the other under the neck of the child, and pushed the thighs up, and then bent the body backward.

DR. ADAMS had succeeded, in one case, by swinging the child by the feet.

DR. BUSEY said there were two classes of cases, those of anemia and of hyperemia of the brain. The former were the most difficult to manage.

Stated Meeting, January 4th, 1884.

DR. C. S. BUSEY, *President, in the Chair.*

DR. T. C. SMITH read a paper on

OCCLUSION OF THE OS UTERI DURING PREGNANCY.¹

DR. J. T. JOHNSON said he had never seen a case of occlusion of the os at the time of labor. He thought a wise course had been pursued in the first case by waiting until nature had overcome the obstacle. He was glad attention had been called to the results of bad nursing, and hoped that, in the future, we would be able to secure the services of skilled nurses. Another point was the character of the fever in the first case which Dr. Johnson believed to be due to septicemia and not malarial fever. (Dr. Smith, interrupting said, that he had not assumed the fever to be malarial, but fever of a peculiarly intermittent type.) Dr. Johnson, continuing, said the high temperature showed a bad condition of things, and intrauterine injections would have suggested themselves in the treatment. As to the terrible labor pains, in the second case, showing an agony never before witnessed by Dr. Smith, he thought they were due to the misdirected force of an oblique or anteverted uterus, and not produced by the occlusion. Or they might have been due to the original metritis, which caused the occlusion, the sore muscles contracting with great pain. In Dr. Smith's case the agony continued even after the membrane had been ruptured, and the question arose, how could it be caused by an obstruction which had been removed? The frequency of the affection, as as-

See Original Communications in a future number.

serted in the paper, was new to him, as he had always thought these occurrences very infrequent. He thought the force of the uterus ought to be sufficient to break through obstructions such as had been described to exist in the cases reported, unless we were willing to credit the membrane with extraordinary powers of resistance. The force exerted by the uterus had been estimated at fifty pounds; and Houghton held that, by the additional action of the abdominal muscles, the total force expended might amount to five hundred pounds. Duncan, however, combated this view. Referring to the question of diagnosis, he said Dr. Smith had spoken of an elongated anterior lip obscuring it. This Dr. Johnson thought likely, for we often saw it swollen and hanging over the os in cases where it had been compressed any length of time between the hard head of the child and arch of the pubis.

DR. D. W. PRENTISS said the paper called to his mind a case of occlusion of the vagina, and, it was thought of the os uteri, seen by him some years ago while on a pleasure trip in Virginia, but as he did not see the case through, he could only speak of it in part.

The finger could be passed about three inches, where it met a wall of cicatricial tissue, beyond which the head could be felt by passing the finger into the rectum. The physician in attendance thought the case a proper one for the Cesarean operation, but Dr. Prentiss dissuaded him. He afterwards learned that the uterine efforts proved sufficient to overcome the atresia. The patient had been previously delivered by forceps and the vaginal walls torn, which accounted for the obstruction.

DR. C. E. HAGNER, referring to a remark made by Dr. Smith on the liability of the obstruction to recur in successive pregnancies, related the case of a young woman in whom there was no opening at the os uteri apparent, simply a dimple; he punctured and dilated the narrow channel and brought on a miscarriage without knowing that the patient was pregnant, which, in fact, she had no legal right to be. The miscarriage, we would think, would cure the obstruction, yet in a few months she suffered from dysmenorrhœa. Three years after, she was again pregnant, and had an abortion produced on her in Baltimore. Some weeks ago, he found on examination that the os was as small as when he first saw the case.

DR. SMITH, in answer to Dr. Prentiss, would say that he had purposely avoided reference to cases of atresia of the vagina, as such cases were rather often met with. Referring to Dr. Johnson's remark, he said that after rupturing the membrane, in the second case, the uterus seemed to act like a sore muscle made so by the violent efforts to overcome the atresia. As to the position of the uterus, he stated that, when the pains came on, the organ seemed to tilt forward. The patient was a primipara and the walls of the abdomen were not flaccid, as in multiparæ; he was unable to prevent the uterus from assuming this abnormal position. He did not think that the pain was due to inflammation.

Cazeaux and others had expressed surprise that the uterus was not able to rupture these thin membranes. He had also cited Dugès as to the liability of making an incorrect diagnosis, when the posterior lip overlapped the anterior and thus prevented the os from being recognized. In order to enter the os in these cases, it was necessary to pass the finger from before backward, and not from behind forward. In the first case, he thought the fever was septic: the lochial charge was offensive. He had merely referred to the peculiarity of the intermittence.

TRANSACTIONS OF THE GERMAN GYNECOLOGICAL SOCIETY.

(Concluded from page 319.)

Third Day—Afternoon Session.

KEHRER (Heidelberg) *in the Chair.*

KALTENBACH reported

A CASE OF PREGNANCY IN THE RUDIMENTARY CORNU OF A UNICORN UTERUS.

The specimen I desire to present was sent to me from a medico-legal source. It was obtained from a IVpara pregnant in the fourth month, who was reported to have been ill-treated by her husband, and had died a few days later with symptoms of internal hemorrhage. At the autopsy, blood was found here and there in smaller superficial patches between the omentum and the abdominal walls. When the omentum was lifted, there appeared between the coils of intestine, but particularly in the most dependent parts of the abdominal cavity, in both lumbar regions and in Douglas' pouch, a larger quantity of partly fluid, partly coagulated blood. In the right half of the lower abdomen lay a large clot, on the removal of which was found a fetus corresponding in development to the beginning of the fourth month; it was still connected by the funis with the placenta, one-half of which projected from an ovisac situated by the side of the uterus. No satisfactory conclusion as to the site of this ovisac had been reached at the post-mortem, which is readily understood in view of the great rarity of the anomaly in question.

The case is one of gestation in the rudimentary cornu of a uterus unicornis. You see here the developed unicorn with its decidua; on one side it bears the normal uterine appendages, on the other side it appears bare. At the height of the internal os, it is connected with the ruptured ovisac by an altogether solid cord the thickness of the little finger. The ovisac bears on its posterior side tube and ovary with a beautiful corpus luteum. Laterally from it, but drawn more to its under surface by the direction taken by the growth of the sac, extends the round ligament.

Of the three possibilities of impregnation of a rudimentary cornu—penetration of the semen through a canal in the connecting cord, external transmigration of the ovum, external transmigration of the semen—the latter obtains here; for the corpus luteum is in the ovary of the rudimentary cornu.

The termination of gestation in the rudimentary cornu of a unicorn uterus is nearly always fatal to the woman.

Between the third and sixth, generally in the fourth month of pregnancy, the sac ruptures, and fatal internal hemorrhage, with or without concomitant peritonitis, ensues. The idea of arresting the hemorrhage by extirpation of the ruptured sac after laparotomy appears quite rational in itself. However, such a bold therapeutic procedure can be of use only quite early, that is, immediately after the occurrence of the rupture: for subsequently the effused masses of blood are transported and distributed over the whole abdominal cavity through the peristalsis of the intestines, and peritonitis succeeds as a rule. This early interference, however, will probably be prevented by the uncertainty of the diagnosis.

Thus far two cases are recorded in which the ovum died early and was changed to a lithopedion. In a case reported by Fritze, the fetus had remained quiet for thirty years in the ossifying ovisac, until sloughing occurred in its interior with consequent fatal peritonitis.

In a second case, Köberlé extracted such a fetus twenty-one months after its death by laparotomy.

Finally, in most recent times, two cases have been reported by Turner and Litzmann, in which the pregnancy went to term. In the former case, pains set in which ceased after some days; six months after, death from phthisis. In the other case, Litzmann extirpated the ovisac after the death of the fetus.

Uterus unicornis with one rudimentary cornu claims a prominent clinical interest, not only by the possibility of pregnancy occurring in it, but also by the retention of menstrual blood which may take place. Every one is probably familiar with the two observations of Hegar, in which a peculiar form of lateral hematometra was caused by an accumulation of menstrual blood in the closed rudimentary cornu.

The anomaly may also become of great importance to the medico-legalist, as is shown by the present case. It is said that the internal hemorrhage, the rupture of the ovisac, occurred here in immediate connection with ill-treatment which, to the best of my knowledge, consisted in blows on the head. There is no doubt that every ill-treatment, even when the injury does not affect the abdomen directly, may, by the general exercise of the muscles connected with the defence and the increase of intra-abdominal pressure, indirectly give rise to rupture of a thin walled ovisac naturally predisposed thereto. How difficult, however, to the medico-legal physician is the answer to the question whether the ill-treatment was the immediate cause of the rupture or whether the concurrence of these two circumstances was accidental! It is certain that the ill-treatment to which the woman was subjected by her rude husband was not the first and only one during her present pregnancy, and, on the other hand, the rupture occurred exactly at the time when experience teaches that it occurs spontaneously in the anomaly in question.

Nor is the decision facilitated by the fact that there was an interval of several days between the ill-treatment and the fatal accident, for it might well be that at first a very small laceration occurred at the thinnest part and gradually enlarged under the subsequent contractions of the sac, thereby leading to the detachment of the ovum and its escape into the abdominal cavity.

Finally, I desire to point out that also former cases of pregnancy in a rudimentary cornu have occupied the medico-legalist, because the pains which the women define as colicky, occurring suddenly with the rupture of the sac, as well as the rapid collapse and the occasional presence of terminal convulsions, led to the suspicion of poisoning.

KEHRER (Heidelberg) stated that a similar specimen of rupture of the rudimentary left cornu is in the collection of the Heidelberg clinic for women. The patient died suddenly.

NEUGEBAUER, JR. (Warsaw), read a paper on

THE MEDIAN VAGINAL SUTURE (MEDIAN COLPORRHAPHY) FOR THE
OPERATIVE TREATMENT OF PROLAPSUS UTERI.

He gave a short synopsis of the history, the nature, and the execution of this operation, first performed in 1867 by his father, L. A. Neugebauer, at Warsaw, but which, in French and German text-books, is erroneously cited as Le Fort's operation. He exhibited a number of drawings, and appended a review of the clinical cases, thus far comprising seventy-six. The paper will be published in full in connection with another on the same subject read before the Berlin Obstetrical Society. In reply to a question by Kalténbach, how long after the operation the patients had been under observation, N. stated that in some of them the cure had been permanent after two or three years.

BANDL (Vienna) has delivered a woman operated on by Neugebauer's method. He cut through the artificial septum, but thinks that eventually the labor would have proceeded spontaneously. He is unable to state whether prolapsus had recurred subsequently.

FRAENKEL (Breslau) had assisted Spiegelberg in four operations performed after N.'s method. Sp. combined median colporrhaphy with posterior colporrhaphy or perineorrhaphy. In two cases a relapse occurred.

HEGAR (Freiburg) sees in the fact that Spiegelberg combined N.'s operation with posterior colporrhaphy a proof that Sp. did not expect a satisfactory result from the former alone. H. is of the same opinion. The operation will be effective only where the posterior vaginal wall has sufficient consistency. Where this is not the case, he would resort to posterior colporrhaphy only.

NEUGEBAUER, JR. (Warsaw), made some

REMARKS ON SPONDYLOLISTHESIS.

He submitted a series of specimens and original illustrations, a part of which had been formerly repeatedly described by him, and reported as to the latest stand-point of the views on the origin of the deformity; the illustrative cases comprising thirty-seven

observations, to which three Freiburg pelves must still be added; while in Charpentier's recently published text-book of obstetrics, for instance, the total number of spondylolisthetic and spondylizematic pelves together is given at twenty-seven only. Of the forty cases, N. has personally examined sixteen, eight of which were anatomical preparations and eight in the living subject. The description of six of these forty cases will soon appear.

KUESTNER (Jena) exhibited

TWO HUMERI OF AN EIGHT-WEEK-OLD CHILD,

on one of which it was readily seen that the epiphysis is attached to the shaft twisted around an angle of 24° . The child was born in breech position, and the arms had to be brought down. Though this had been very easy, the epiphysis had become detached from the left. Owing to the peculiar insertion of the rotator muscles at the humerus—the internal rotators being attached only to the osseous shaft of the bone, the external rotators, however, to the cartilaginous epiphysis—union, after separation of the attachment, takes place in such a way that the epiphysis stands in maximal external rotation, the diaphysis in maximal internal rotation. K. believes this lesion to be frequent, and that it has frequently been confounded with paralysis of the nerves (suprascapular). The prognosis of this injury he states to be very unfavorable.

FRAENKEL (Breslau) has repeatedly found detachment of epiphyses when dissecting macerated syphilitic children, and once also in a living one of that class. He inquired whether the autopsy in K.'s case had yielded any evidence of syphilis.

KUESTNER (Jena) answered in the negative. He likewise, in a large number of experiments on the cadaver, had observed that the separation is much easier in syphilitic or macerated than in other children.

HEGAR (Freiburg) exhibited two skeletons in which the diaphyses and epiphyses were separated. One person was twenty-five years old. There were no evidences of syphilis present in that case.

KEHRER (Heidelberg) spoke on

THE EMPLOYMENT OF CORROSIVE SUBLIMATE AS A DISINFECTANT IN OBSTETRICS.

¶ He briefly discussed Koch's well-known experiments with bichloride of mercury, and pointed out that that investigator had had with no other disinfectant such excellent results as with this one.

The speaker himself had employed corrosive sublimate extensively at his institution since April of the preceding year. Of 221 parturients, there appeared in 4 only an eruption of urticaria on the thighs which spread thence over the whole body. It disappeared again after three or four days. Only 1 parturient and 3 gynecological patients were attacked by stomatitis. The former had previously been subjected to a course of inunctions, and two of the latter had taken mercurial preparations internally. In view of this experience, K. advises caution in the employment of sub-

limate irrigations with persons who had been formerly treated in any way with mercury. He first employed solution of 1:2,000, later of 1:4,000. For women in labor he had ordered a vaginal irrigation even before the first examination, in view of the fact that the cervix may be injured during the exploration, and vaginal bacteria thus inoculated. In the puerperium he believes the sublimate irrigation to be indispensable. Only the attendants must be experienced, so as to avoid the reopening of small wounds with the uterine tube, with possible inoculation of vaginal germs of infection. K. had seen cases in which the fever continued in spite of the injections, and disappeared only with its suspension.

As to the results obtained by K. with corrosive sublimate, two-thirds of all parturients remained altogether free from fever; in the other third, more or less intense fever supervened. On the other hand, two-thirds of all parturients showed symptoms of fever before the introduction of the bichloride. K. appended brief statistics of the forms of disease during the year 1881-82.

As further advantages of the corrosive sublimate the speaker finally enumerated its cheapness as compared with carbolic acid, its ready solubility, and its odorlessness. The disadvantages are very slight. The sublimate roughens the skin, and also the vaginal mucosa. In view of this fact, the frequency of the irrigations should be limited as much as possible. It is quite annoying that metallic instruments are attacked.

PROCHOWNICK (Hamburg) inquired whether K.'s attendants had not suffered by the sublimate, as had been the case in the surgical ward of the Hamburg City Hospital. Aside from slighter affections, eczemas, etc., the nurses and even the assistants had been seized with dysentery. Possibly the very plentiful employment may be to blame for this. P. cautioned against giving concentrated solutions into the hands of midwives.

KEHRER (Heidelberg) had observed nothing of the kind at his clinic. The only complaint was, that the hands became rough.

KUESTNER (Jena) had for some time past been employed with examinations of the uterine secretions; he had succeeded, by means of peculiarly shaped and perforated glass tubes, in obtaining the secretions of the body and the cervix separate. These methods he described more in detail. As to the presence of micro-organisms, K. found them always both in normal and in pathological secretions, whether from the body or the cervix, and they were invariably exceedingly manifold in form and minuteness. After carbolized irrigations of the uterus, the prompt disappearance of the cocci could *not* be demonstrated, *but they vanished very promptly after sublimate irrigation*. The examinations for micro-organisms had been made by the speaker together with Dr. Krynsky, of Jena.

HEGAR (Freiburg) confirmed Kehler's statements, and gave a brief synopsis of the results obtained with bichloride of mercury at the Freiburg clinic (comp. Wiedow, *Centralbl. f. Gyn.*, No. 37, 1883). H. had observed severe salivation after a laparotomy in which sublimate had been used. Still he thinks that the latter should be ascribed, not only to the mercury, but to a nervous predisposition, for the salivation continued nine months.

SCHATZ (Rostock) called to mind that Martin, the elder, had already resorted to strong solutions of corrosive sublimate for vaginal irrigations. Sch. himself uses a five-per-cent solution, a teaspoonful of which he adds to a litre of water. He had seen but one case of salivation; in this, sublimate had been retained after the injections.

SCHMALFUSS (Hamburg), on the strength of his own experience had during six months' activity at the Hamburg City Hospital, disapproved Prochownick's statements.

KEHRER (Heidelberg), in answer to a question by Müller, stated that he was unable to give any further information regarding the species and the pathogenesis of the cocci present in the vagina.

FRÄNKEL (Breslau) is working, as he had just learned from Küstner's statements, in the same direction as the latter, and in a similar manner; only he devotes himself especially to gonococci. In the main, he is able to confirm K.'s results. The forms of micro-organisms which he found were bacilli and diplococci; in acute gonorrhea, gonococci. F. attempted to diagnose a latent gonorrhea in para-, peri-, and endometritis by the microscopic examination of the uterine secretions, but the result was negative. He had also made experiments with bactericide and particularly gonococcicide agents, with iodoform, corrosive sublimate (1:2,000), finally with cauterization of the urethra, vagina, and cervical canal with concentrated solution of silver nitrate or lunar caustic. The cocci disappeared most rapidly under the sublimate treatment. He stated that he had been able to demonstrate the gonococci not only in the secretions, but also in the epithelia. To an inquiry by Hegar, whether the gonorrhea had been cured by the lunar caustic treatment, after the disappearance of the gonococci, F. replied in the negative.

BADLEHNER had observed that sublimate solutions of 1:4,000 for vaginal injections still produce some irritability. He thinks that solutions of 1:10,000 are sufficiently active. As for the personnel preparing the solutions, similar precautions as in looking-glass factories are indicated in his opinion. To give concentrated solutions into the hands of midwives he holds to be reprehensible.

KUESTNER (Jena) points out that the presence of gonococci in the secretions of women affected with gonorrhea is by no means constant, as had been established already by Fürbringer.

KEHRER (Heidelberg) again emphasized that as yet we are unable to distinguish the species and the pathogenetic importance of the bacteria present in the secretions.

KALTENBACH (Giessen) stated that his experience with corrosive sublimate is equally favorable as that of Hegar.

KEHRER (Heidelberg) reported a

PORRO OPERATION

In a XIIpara with an osteomalacic pelvis. She was operated upon in extreme collapse, almost moribund. Great ascites was present. An offensive discharge issued from the vagina. Child dead. The operation was not complicated. The pedicle was treated extraperitoneally. In the second week the stump became sloughy. The temperature rose. Subsequently a small ileac fistula formed. The patient died ten weeks after the operation, the ascites having again increased. K. exhibited the stump.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF LONDON.

Meeting, Wednesday, December 5th, 1883.

DR. GERVIS, President, in the Chair.

THE EFFECT OF THE FORCES AND RESISTANCES OF LABOR IN PRODUCING LATERAL FLEXION OF THE FETAL HEAD.

DR. GALABIN showed three diagrams to illustrate the view as to this subject which he had formerly brought before the Society: viz., that wherever the head was so shaped, by prominence of the parietal tubera, that the biparietal diameter was greater than oblique diameters slightly inclined to it, so that a lateral obliquity secured a mechanical advantage by bringing into any diameter of the pelvis opposed to the head, a smaller diameter than the biparietal, and when also there was any notable pressure upon the head at the ends of its transverse diameters, then the effect of this pressure upon the head was to promote lateral obliquity up to the point at which mechanical advantage was gained, and beyond that point to counteract it. Diagram 1 showed a head engaged in the pelvis, with a lateral obliquity of about 7 degrees. Here it was shown that both pressures and propelling force tended to increase the obliquity. Diagram 2 showed a head engaged in the pelvic cavity, with a lateral obliquity of about 15 degrees. Here it was shown that the pressure tended to diminish the displacement, but the propelling force to increase it. Diagram 3 showed a head arrested above the brim, with a lateral obliquity of about 15 degrees. Here it was shown that both resistances and propelling force tended to increase the obliquity. The mechanism producing this lateral obliquity was analogous to that which produced chin flexion.

THE MECHANISM OF LABOR, MORE ESPECIALLY WITH REFERENCE TO NÆGELÉ'S OBLIQUITY, AND THE INFLUENCE OF THE LUMBO-SACRAL CURVE.

By DR. ROBERT BARNES.—The author quoted Nægelé's description of the obliquity of the head as it presents at the pelvic brim. He next examined the theory of those who hold that the axes of the pelvic brim, uterus, and fetus coincide. He showed that while Nægelé held the same view as to the inclination of the pelvis to the horizon as his opponents, he did not deduce from it that the uterus and fetus presented with their axes coincident with that of the pelvic brim. The author examined this view, and, appealing to the frozen sections of Braune and Chiara, showed that the uterine axis forms a considerable angle with the axis of the pelvic brim. He demonstrated the difference between the heart-shaped

brim of the pelvis, and the circular cavity of greater capacity than the brim, and therefore that the head passing the brim must, under the law of accommodation, and of movement in the direction of least resistance, rotate under the promontory. He then described the lumbo-sacral curve, the influence of which upon labor had received inadequate attention. This curve was represented by a line drawn from the promontory as a centre with a radius intersecting the middle of the plane of the brim. This he proposed to call *Barnes' curve*. It is the counterpart for the brim of Carus' curve for the outlet. The resultant of the two forms a sigmoid curve beginning at the fundus of the uterus and ending at the outlet of the pelvis. He called it the *parturient curve*. He showed that the driving force acts in a line forming an angle behind the axis of the brim, and that therefore, under the combined action of the convex lumbo-sacral curve, of the relation of the uterine and fetal axes by an angle behind the brim axis, that the head cannot enter synclitically, that is, with its base or transverse section parallel with the brim plane. He showed that, from the curve of the parturient canal, the irregular shape of the head, and the non-coincidence of the three axes, all the condition of true synclitism are wanting. He then referred to the part borne by the *planes of the uterus*; the first, resting on the lumbo-sacral curve, helps to guide the head to the brim in obliquity; the second guides the head backwards into the sacral cavity and under the promontory, completing the lower course of Barnes' curve; the third, or perineal plane, throws the head forwards to the outlet in Carus' curve. He next demonstrated, from Galabin's and his own measurements, that there is a distinct gain in presenting the oblique diameter instead of the biparietal to the brim, the point especially insisted upon by Nægelé. He illustrated the theory of normal obliquity by comparison with that of labor with brim-contraction, showing that the importance of the lumbo-sacral curve increases with the degree of contraction, but that the same law prevails throughout all labors. He concluded by submitting that the objections urged against Nægelé's obliquity are vitiated by erroneous assumptions, that there is an adequate reason for this obliquity, and an adequate mechanism to produce it; and that it is a real and necessary result of the combined action of the factors working in the mechanism of labor. The memoir was illustrated by several diagrams.

THE PRESIDENT thanked Dr. Barnes for his erudite and interesting paper. He (the President) had in his earlier years, influenced much by Dr. Tyler Smith, accepted Nægelé's view as to the brim obliquity. Subsequently, the writings of Duncan and others had led him to doubt it. After studying Dr. Galabin's paper on the subject, he had come to the conclusion that while, in the case of a well-formed pelvis and head of average size, the head entered the brim perpendicularly to the brim plane, yet that, when the brim was even slightly contracted, new conditions prevailed, and there was a gain in the obliquity of Nægelé. He expressed the gratification of the Fellows present in seeing Dr. Wiltshire again among them

DR. MATTHEWS DUNCAN said that the mechanism of natural and unnatural labors had very little in common with a view to Naegelé's obliquity. The "curve of the false promontory" or "Barnes' curve" had no importance in natural parturition. In the flat pelvis, the head followed this curve. The question could not be settled with mathematical exactness. It was one for simple observation, not for ingenious argument. As a matter of fact, he did not find the right parietal bone enter the pelvis first, nor did he find the caput succedaneum of early labors form upon that bone; but upon the vertex. It was only in late labors that it was formed on the right parietal bone. He held that the axes of the pelvis, uterus, and fetus were practically coincident. The frozen sections appealed to by Dr. Barnes were not faithful representations of the state during life. The uterus, during the contractions of labor, erected itself, bringing its axis into coincidence with that of the brim. It was not pushed back towards the spine, but became more prominent. In the "bearing-down" action accompanying labor, the recti muscles were not the only ones which acted: the diaphragm and its crura acted as well, forming a dome which supported and assisted the uterus.

DR. GALABIN had frequently observed Naegelé's obliquity, not in easy labors, but in cases in which there was no deformity, but the head met with considerable resistance. He ascribed it chiefly to the lateral pressures on the head in the pelvis, as shown in the diagrams he had exhibited. A head with a large biparietal diameter thrown directly across the canal was in a position of unstable equilibrium, like a head in a position of brow presentation. He could not understand how Dr. Barnes considered that the posterior obliquity of the uterus helped to produce Naegelé obliquity. Such uterine obliquity would, until the resistances came into play, tend to produce the opposite of Naegelé obliquity. But he did not think that posterior obliquity of the uterus was nearly so great as might appear from frozen sections. He could not accept Dr. Barnes' account of the action of the anterior uterine valve, for he did not think that displacement of the os uteri backwards was a regular occurrence. The effect of uterine obliquity in producing obliquity of the head was only in operation while the force was transmitted through the condyles. While the liquor amnii was retained, the force acted in the axis of the pelvis, and had no tendency to produce obliquity.

DR. CHAMPNEYS agreed with Dr. Galabin that the first effect of posterior obliquity of the uterus would be to produce the opposite of Naegelé obliquity. He pointed out that the condition known as "pendulous belly" was generally recognized as a cause of exaggerated Naegelé obliquity. How could these two opposite conditions (anterior and posterior deflection of the uterus) produce the same effect, viz., Naegelé obliquity?

DR. ROPER remarked that, although a slight advantage was gained by the Naegelé obliquity, yet its practical importance, even in contracted pelvis, was very small. The other obliquities, *e. g.*, flexion and extension, were of infinitely greater importance.

DR. BARNES held that the curve of the promontory was important both in natural and unnatural labor, the difference being simply one of degree. It was necessary for synclitism that the axes of the uterus, fetus, and pelvic brim should be absolutely, not merely practically, coincident, for the slightest deviation would be enough to cause obliquity of the head. He thought that the frozen sections were essentially true representations of nature.

There was no evidence that the crura of the diaphragm contracted in such a way, or that the diaphragm had the power to drive the uterus forward, against the abdominal muscles. If during turning the direction of the force were observed, it would be found to lie behind the axis of the pelvic brim. He had observed cases of labor very carefully, and had observed the presence of the Nægelé's obliquity from the beginning of labor.

Meeting, Wednesday, January 9th, 1884.

DR. GERVIS, *President, in the Chair.*

HYSTERECTOMY.

MR. KNOWSLEY THORNTON showed an enlarged uterus, surrounded by multiple fibroids, the right ovary and tube attached, the stump of a large fibrocystic tumor of uterus (also shown) and a partly calcified fibroid of the size of a fetal head which had to be enucleated in order to apply the *serre-nœud*. The parts came from a single woman aged fifty-five, from whom, on account of excessive hemorrhage and rapid growth of the tumor, they were successfully removed.

HEMATO-SALPINX.

MR. KNOWSLEY THORNTON also showed the ovaries and tubes removed from a married lady aged twenty-nine. She had suffered from repeated hemorrhages which followed a severe emotional shock; and tumors were discovered to the left and right of the uterus. These were removed, and proved to be the tubes full of tar-like blood and firmly adherent. There was also an ovarian cyst with commencing papillomatous growth. After removal of the diseased parts, the discharge gradually ceased, and the patient did well. Mr. Thornton thought the case either one of tubal pregnancy, or of hemorrhage the result of emotional shock, the blood being prevented from escaping.

PELVIC HEMATOCELE FROM MALIGNANT DISEASE OF PERITONEUM.

DR. PLAYFAIR exhibited this specimen, taken from a girl of sixteen, from whom he had removed an ovarian tumor, in April 1883. At the end of November, she was admitted into King's College Hospital with symptoms and signs of a retroperitoneal hematocele so large as to compress and obstruct the bowel. This was first aspirated, then freely incised and drained, with immediate but temporary relief to symptoms. On autopsy, the peritoneum was found studded with masses of soft cancer, one of which had ruptured and caused the hemorrhage. No trace of the ovariectomy ligature could be found. He had not seen recorded a case of hematocele from such a source. He believed its association with the ovariectomy to be only that of coincidence.

OBSERVATIONS ON PUERPERAL TEMPERATURES.

By MR. E. S. TAIT, M.B.—The patients observed were in the general Lying-in Hospital: 60 were primiparæ and 65 multiparæ. The day after delivery on which the highest temperature most

often occurred was the third, then the fourth, then the second. In 25 cases the highest temperature occurred during the second week, often from nervous causes. Lacerations of the perineum did not appear to affect the day of highest temperature. Mechanical interference during labor did not seem to affect the result. The average temperature was lower in those cases in which there was no tear than in those in which deep lacerations had occurred. In primiparæ the temperature appeared to be raised by labial tears, deep perineal tears, and the use of forceps; but in multiparæ no such effect could be traced. Slight perineal tears seemed to scarcely effect the temperature. The introduction of the carbolyzed hand into the uterus during the third stage did not affect the average temperature. In six cases, there were urticarious or erythematous rashes which did not affect the temperature. The temperature was highest in the latter part of the day, lowest in the early morning. When the 10 P.M. temperature was higher than that at 6 P.M., there was often inflammation present. The temperature frequently rose without any physical cause to account for it; and in such cases it was often found that something had happened to disturb the patient's nervous system, such as fright, bad news, etc. Accounts were given of instances of such "nervous temperatures" as they might be called.

DR. ROUTH said that in disease the temperature might be as much as three degrees hotter in the vagina than in the axilla. Unless, therefore, the temperature in the puerperal state were taken in each part, error might follow.

DR. JOHN WILLIAMS thought that Mr. Tait had established two facts: that high temperatures were more common when the perineum had been deeply torn, and that high temperatures were associated with nervous conditions during the puerperal state. It was frequently said that tears of the cervix were causes of fever; he (Dr. Williams) thought they were really so. The relation of fever to fetid discharge was important; in many cases the fetor came on after the fever and not before it, and might therefore be inferred to be its result and not its cause.

DR. GALABIN thought it would be of interest to compare, if possible, the temperatures observed in private practice with those of hospital patients. His own impression was that the range of temperature was not so high as that observed by Mr. Tait. He doubted whether, in cases of rise of temperature apparently from nervous causes, such causes were always unmixed. It might be combined with some septic disturbance. He related a case in illustration.

DR. GRAILY HEWITT thought the facts brought forward as to rise of temperature from emotional conditions most important. He mentioned a case in illustration. He suggested that it was probably due to temporary relaxation of the uterus, and consequent absorption of septic matter. All that he had ever seen favored the idea that the septic material was absorbed before the fever began.

DR. DALY thought that fetid discharge preceded and caused fever. This was proved by the effect of treatment. He thought

the range of temperature was, as a rule, higher in hospital than in private practice.

DR. PLAYFAIR had time after time observed in private practice cases of high temperature in which he was satisfied that Mr. Tait's explanation, that they were due to emotional disturbance, was the true one. It was important to distinguish these from the more serious pyrexia due to septic mischief. In the latter an initial rigor was rarely, if ever, absent. Nervous high temperatures were not confined to the puerperal state; and temporary high temperature might be produced in the puerperal state by other than nervous causes. He knew of no facts to justify Dr. Williams' theory of the relation between fetid discharge and pyrexia, and he hoped no one would be induced by it to underrate the importance of fetid discharges, for no woman could be considered safe in whom such existed.

DR. GODSON mentioned a case of pyrexia due to an emotional cause. He considered it exceptional for rise of temperature from such cause to be preceded by a rigor.

DR. PRIESTLEY said the records in the paper of pyrexia demonstrated to be due to emotional causes showed the importance of shielding the puerperal woman not only from physical, but also from mental disturbance. They also showed that high temperature was but a unit among other symptoms, and unless accompanied by rigor, abdominal tenderness, or other signs, did not invariably indicate danger.

DR. MATTHEWS DUNCAN called attention to the value of a regularly constructed chart as against isolated observations. He was sure that in private practice as great variations of temperature occurred as in hospital. All his ideas were unfavorable to the reception of the doctrine that the fever caused the putridity of discharge. Putridity was not an essential part of the worst kind of infecting material. Infection by putridity began with the taking in of the poison, and ceased within a few hours after stoppage of the supply. This was sapremia without septicemia.

DR. CHAMPNEYS said that about the third day, on which the milk, the bowels, and surgical injuries combined to raise the temperature, diagnosis was very difficult, often impossible. Pelvic inflammation, which was often quite ephemeral, could not be excluded without vaginal examination, which had been made in the cases recorded in the paper. The evidence he had, although small in amount, showed that the range of temperature in private practice was as high as that in hospitals. The proof that "nervous temperatures" were not septic was that they only lasted a few hours, and that no infection of other patients took place. The mortality in good lying-in hospitals was becoming less than that in private practice. Fœtor of discharges was useful as a means of attracting attention.

DR. WYNN WILLIAMS said that if septic matter came in contact with a wound and was absorbed, it was followed by rigors with diminished temperature.

THE PRESIDENT said that from his experience in private practice, and from observations made in the St. Thomas' hospital maternity charity, he believed the average temperature in private practice was not so high as that in Mr. Tait's cases. He could quite understand that, in the class of cases in which infection was brought from without, offensive discharge due to necrotic endometritis might follow and not precede the fever.

MR. TAIT said that the observation of higher temperatures in hospital than in private patients he thought due to the greater frequency and regularity with which the temperature was taken in the former class of cases. Dr. Hewitt's explanation of the "nervous temperatures" he thought was negatived by the great rapidity of the rise and fall of the temperature in these cases. In many of them the patient looked and felt well, so that the rise of temperature would have passed unnoticed had it not been for the diligent use of the thermometer.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Meeting, December 21st, 1883.

PREVENTION OF PUERPERAL INFECTION.

DR. H. J. GARRIGUES read a paper on the above subject, and said that, so far as he had been able to determine, the first who methodically used antiseptic treatment as a prophylactic against puerperal fever was Stadfeldt, of Copenhagen, in 1870. He used carbolic acid. Dr. Garrigues had used carbolic acid regularly during the last eight years (washing hands, instruments, vaginal injections, etc.). In private practice it is comparatively easy to avoid infection. When he began his service as visiting obstetric surgeon to Maternity Hospital, he introduced this treatment, and although of some value, the results were far from satisfactory. After referring to the general features of hospital practice, the danger for lying-in women in these institutions, and giving statistics concerning the rate of mortality at the maternity hospital, with the diseases which developed, Dr. Garrigues gave a detailed account of the plan adopted for the use of the bichloride of mercury as an antiseptic in obstetric practice in the same wards where carbolic acid had heretofore been used. He did not, however, trust to the new antiseptic alone, and the other measures may have contributed somewhat to the splendid results obtained. Rapid alternation in the wards was secured as far as possible. If any patient, in either the pavilions or in the convalescent ward, showed any sign of infectious disease, she is immediately transferred to the department for the sick puerperal. The doors between the wards were locked and the chinks filled with tow and pasted over with paper. Doors to water-closets were furnished with springs and the windows in the water-closets were arranged so that they could not be closed. As soon as a ward was empty it was fumigated with sulphur, and the floor and all the furniture cleansed and disinfected.

When the patient is brought to the delivery-room she is given an

enema and a bath as heretofore, but after that her abdomen, genitals, buttocks, and thighs are carefully washed with lukewarm solution of the bichloride (1 to 2,000). At least two quarts of the same fluid are injected into the vagina by means of a fountain syringe, consisting of a glass bucket, a rubber tube, and a straight glass tube, and in protracted labor these injections are repeated every three hours. The rubber sheet covering the delivery bed is washed with the undiluted solution immediately before every delivery.

No nurses except the one in charge of the waiting ward and the head nurse in the pavilions are allowed to make vaginal examinations, and even these are held not to repeat them more frequently than required for notifying the doctors. The doctors are urgently requested not to introduce their finger into the womb in common cases, but to be satisfied with such information as is derived from abdominal palpation and the examination of the part presenting at the external os.

At the beginning of a labor, doctors and nurses wash their hands in the solution, using soap and large, stiff nail-brushes; and if more than one patient is in labor at a time, the same precaution is taken before going from one to the other. No other lubricant is used than the fluid adherent to the fingers, except in operations in which the whole hand has to be carried into the genitals, or the forceps are to be used. Then he uses glycerin with bichloride of mercury (1 to 2,000). The glycerin itself possesses antiseptic properties, which are still enhanced by the addition of the bichloride.

When the head appears at the vulva, a piece of lint, soaked in the solution, is applied to it, and kept there all the time. After the expulsion of the child, the genitals are kept covered with a similar compress.

The placenta is expressed by Credé's method. If in exceptional cases it becomes necessary to introduce the fingers, the vagina is washed out with the solution; otherwise not.

Intrauterine injections are used only when the hand or instruments have been introduced into the interior of the womb, or in case of the birth of a macerated child.

He never allows any part of the placenta or the membranes to remain in the womb. With good antiseptic treatment the introduction even of the whole hand into that organ is of no account compared with the danger of hemorrhage and septicæmia by leaving any part of the secundines behind. The intrauterine injection is given immediately after the removal of the secundines; a large amount of fluid is used (from two to six quarts), and so hot that the hand just can be held in it.

If no indication is found for intrauterine treatment, the douche is only applied to the vagina. After this little digression, we will return to our supposed case of normal labor.

After the expulsion of the secundines the patient is washed with the solution and the vulva covered with a *dressing* consisting (1) of

a piece of lint, six by eight inches, folded lengthways, so as to be three inches wide; (2) outside of that a piece of oiled muslin, nine by four inches; (3) outside of that a large pad of oakum; and (4) the whole is fastened with four pins to the binder in front and behind by means of a piece of muslin eighteen inches square, and folded diagonally so as to form a kind of boat, five inches in width, for the reception of the other pieces of the dressing.

This dressing is put on with the same care as we dress a wound after a capital operation, and renewed four times in the twenty-four hours, twice by the day nurses, and twice by the night nurses. At the same time the parts nearest the genitals are washed with the solution. Before each washing the nurses disinfect their hands as before labor. Any substance such as cotton, lint, etc., brought in contact with the genitals, is beforehand thoroughly soaked in the solution. No vaginal injections are given, except in rare cases in which the lochia become offensive. By the effective antiseptic treatment at the entrance, preventive injections become superfluous, and thus one great source of infection is avoided.

All strangers are excluded from the wards. The doctors and the nurses are forbidden to enter either the wards of Charity Hospital proper or the dead-house. In the convalescent ward the dressing is continued so long as the patients are kept in bed. Ninety-seven women have been delivered since the introduction of this treatment, none have died, and there has been scarcely any disease among them, and especially no trace of diphtheritic inflammation which formerly occurred very frequently. Dr. Garrigues believes that the bichloride of mercury possesses great advantages over carbolic acid. Although the bichloride is a better antiseptic than carbolic acid, he attributed the success obtained to the dressing, in a great measure, by which the drug was so applied that air did not gain access to the genitals without passing through the disinfectant.

The discussion was opened by DR. R. A. MURRAY, who had been especially interested in Dr. Garrigues' good report, because, despite all the precautions used while he was in charge of the service, the mortality from puerperal fever was nearly eight in one hundred cases of confinement. The form of fever was very similar to that met with in Bellevue Hospital. Puerperal peritonitis was seldom seen, but puerperal diphtheria occurred very frequently, usually beginning at the vulva and extending up the vagina, but in only a few cases was it seen in the uterus. It was also seldom ushered in with marked symptoms. The ward was disinfected from time to time, but it was finally given up, and when Dr. Garrigues began his service it was a new ward so far as confinement cases were concerned, and probably that fact influenced the results somewhat at least. Yet he questioned whether so good results would have been obtained without the practice of antiseptic midwifery. Dr. Murray thought the details given by Dr. Garrigues would be difficult to carry out in private practice, but the material part could be adopted by thoroughly disinfecting the napkins, and washing

out the vagina before and immediately after delivery, and treating all lacerations with bichloride of mercury solution or iodoform. He had seen cases in which the parts had been actually cauterized with carbolic acid, and yet the diphtheritic exudation returned as quick as ever after the removal of the eschar. It might seem that such details as had been given by Dr. Garrigues are meddlesome and not warranted, but in hospital practice they must be adopted in order to prevent the spread of puerperal fever, although he thought it was unnecessary to adopt them in private practice.

DR. W. GILL WYLIE said the paper by Dr. Garrigues confirmed the views which he expressed in a paper read about six months ago. He was thoroughly in favor of the use of antiseptics, even in private practice, as it is well known that the most successful obstetricians have, occasionally at least, had cases of puerperal fever develop in their practice. Since he began their use in Bellevue Hospital he had employed them in private practice; that is, had caused the vagina to be washed out with a solution of carbolic acid—usually one to thirty—the body of the patient washed with the same solution, all the bed linen, two sets of blankets, and napkins to the number of thirty at least, all carbolized, and in only a single case had he seen any signs of septic fever, and that was one in which he used forceps in a dry labor. Now he would use the bichloride of mercury solution in place of carbolic acid.

In all that had been said recently on this subject he thought sufficient stress had not been placed on *perfect drainage*, and especially after abortion. This can be secured by dilating the cervix, and, if necessary, a drainage tube should be used. He then referred to a hospital case in which a decomposing placental mass remained behind partly in the vagina and partly in the cervix, and he proceeded, not as he probably would have done some years ago—namely, remove it at once—but had administered antiseptic injections for twelve hours and then removed it.

DR. W. R. GILLETTE said that while there might be some difference of opinion concerning the exact methods by which the principles of antiseptic treatment were to be applied, it was scarcely necessary to dwell upon them. With regard, however, to the compress or "dressing" presented by Dr. Garrigues, he was unable to understand how it could be of special service with the woman lying in bed; certainly it could not be kept in close coaptation with the parts, and therefore would not prevent the entrance of germs. This theoretical objection, however, should not weigh very much against results obtained by actual use. He sincerely hoped the good results obtained by Dr. Garrigues would be continued, but his experience in the same hospital allowed him only to hope for their continuation. He thought the fact that Dr. Garrigues' service began in a new ward, as already mentioned by Dr. Murray, may have had considerable to do with the immunity from puerperal fever. He also wished Dr. Garrigues had gone back to 1875, and given the statistics for that year, when the service was transferred from Bellevue, and the prophylactic measures were simply cleanliness, bathing with carbolic-acid water, and keeping them as far apart as possible in the lying-in room. Of the 613 women confined during that year, only three had diseases at all referable to puerperal fever; a record even cleaner than that given by Dr. Garrigues, and it was obtained by perfect cleanliness.

DR. A. S. HUNTER was in accord with the author of the paper as to the value of cleanliness and the use of antiseptics, and there was

no question but that better results had been obtained since their adoption than were secured before special antiseptic measures were introduced. He was also in accord with Dr. Gillette concerning the compress to be applied to the vulva, and thought the rubber muslin would be liable to get folded so as to do more harm than good. Moreover, he thought it impossible to keep it in close apposition with the external genitals. Besides, he thought clots would be retained in the pad of oakum or cotton, and in that way the door opened for the entrance of germs. He also preferred to use Dr. Lyman's silver tube rather than the glass tube, and Davidson's rather than the fountain syringe. He thought that to carry out the suggestions made by Dr. Garrigues was not at all difficult. In private practice he would reject the compress, would use vaginal injections in cases of septicemia, or intrauterine injections, and in addition would have a napkin, partially saturated with carbolic or bichloride solution, laid against the external genitals.

DR. S. BARUCH congratulated the members on having had an opportunity to hear a paper in which the praises of antiseptic injections in normal labor had not been sung. In about 900 cases of labor, with very many complications, he had not had but one death preceded by febrile symptoms. He had not used prophylactic injections very much until Dr. Wylie read his paper, and since then had followed the directions given, but his experience in their adoption had been somewhat singular, as in each of the six cases in which they were employed fever developed, undoubtedly due to septic process, occurring from the fifth to the eighth day after labor. Dr. Baruch then spoke of the objections to antiseptic prophylactic injections, such as disturbing the woman, displacing clots, and thus opening avenues for auto-infection, tearing of recent adhesions, etc., all of which had been seen in hospital practice. He believed that simple cleanliness before, during, and after labor would do all that can be done to render labor safe, and that, if it was necessary to meddle, the introduction of iodoform pencils into the uterus or vagina was probably the least harmful.

DR. F. V. WHITE cited an instance, when an interne in Bellevue Hospital, where obscure symptoms of puerperal fever developed in one of the lying-in women and she was transferred to another ward. The vaginae of the remaining women were washed out daily with liquor sodæ chlorinatæ, and he continued in his service, but no other cases developed. The patient who was transferred died two weeks subsequently, and autopsy revealed extensive peritonitis.

DR. J. C. PETERS thought it had been demonstrated that bichloride of mercury is a more certain germ destroyer than any drug which had been used.

DR. GARRIGUES said his statistics were based upon experience in the *old* wards, and not the new one first occupied. He emphasized the necessity of adopting precautionary measures in private practice. He also stated that the compress had been found by actual measurement to be adapted to the distance between the thighs as the woman was lying in bed; that it was applied by the nurses with the exactness with which any surgical dressing was applied, and that, while it was not absolutely air-tight, it was as much as could be used with the presence of a lochial discharge. Dr. Garrigues then read from his paper where he had made a quotation from Dr. Gillette's report, and it appeared that he had in-

cluded the year 1875, but the figures varied considerably from those given by Dr. Gillette. A metallic tube cannot be used if the bichloride solution is employed. He would never use intrauterine injections except for cause, and for him that cause would be fear of entrance of septic material, as in manual or instrumental interference, or the presence of a dead child. He thought that under ordinary circumstances vaginal injections may have done a great deal of good in hospital practice, although he had given them up because he had reached the same result by other means. In private practice he thought there was no danger in their use, and at the same time he thought no harm would follow if something else was substituted, but if not, they should be resorted to. The dressing recommended rendered vaginal injections superfluous, and the nurses had nothing to do with the genitals of the patients. Of course, cleanliness is proper, but it is not sufficient. He agreed that labor is a normal process, but a woman confined in a hospital is in an abnormal situation, and statistics have established the fact that antiseptics have markedly reduced the mortality in lying-in hospitals.

REVIEWS.

TRANSACTIONS OF THE ST. LOUIS OBSTETRICAL AND GYNECOLOGICAL SOCIETY. 1882-1883. Pp. 121.

Whilst, with perhaps one, or possibly two, exceptions, the proceedings of this Society, as recorded in these transactions, will in no wise add to our special knowledge, it is evident that a live interest is taken by its members in matters of paramount interest to woman. The opening paper, by DR. MCPHEETERS, on COPREMIA as a Complication of Labor, calls attention to the importance of carefully regulating all the functions of the pregnant woman before delivery, so that at that time and during convalescence she may be in the best attainable condition to resist possible, and yet in a large sense preventable, complications. In the two cases which are introduced as illustrative of his subject, there were symptoms closely simulating septicemia, which a cathartic proved were dependent on fecal accumulation in the colon. Dr. McPheeters explained the symptoms on the assumption that the patients were being poisoned by the gas generated by the impacted feces.

DR. WALTER COLLES publishes two papers. The first is the report of a case of LARGE FIBRO-CYST OF THE UTERUS AND OVARIAN CYSTOMA, CO-EXISTING WITH PREGNANCY. Some months after delivery, though uncertain as to the exact nature of the tumor, Dr. Colles operated and successfully. From the absence of a pedicle he was obliged to enucleate the growth. The patient had a slow convalescence. The lessons of practical value to be deduced from this case are, the necessity of free drainage, the advantage of careful washing out of the abdominal cavity through the drainage tube, and, finally, that such tumors can be removed, with hope of success, if strict cleanliness and attention to minute detail be a rule with the operator. This case and its results must be considered the gem in these Trans-

actions. Dr. Colles' second paper, on the Management of Abortion, has already received extended notice in the columns of this JOURNAL (November, 1883).

The remaining papers are: MORPHIA AND THE MORPHINE HABIT, by DR. T. L. PAPIN, in which, through the medium of two illustrative cases where the habit was acquired during pregnancy, the possible results and accompanying horrors of the habit are graphically pictured. EIGHTEEN CASES OF PLACENTA PREVIA, by DR. M. TARNALL, with six maternal deaths, teaching the reporter that immediate delivery, where possible, is the wiser course to pursue. THE TREATMENT OF CHRONIC UTERINE AFFECTIONS, by DR. L. CH. BOISLINIERE, a far too superficial treatment of a most important subject; and, lastly, a DISCUSSION OF TRACHELORRHAPHY, its necessity, its indications, its results, evoked by DR. BARRET's report of cases which had occurred in his practice. In the course of this discussion certain ultra positions were assumed by some of the speakers, and certain statements, not to be substantiated by facts, emanated from others. DR. BARRET's statement that, with the exception of Dr. Pallen, he was the first ever to perform the operation, must be accepted *cum grano*, particularly since this "first operation" dates only from 1869 or 1870. It is usually conceded that Emmet devised this operation, and first performed it in 1862, at least seven years before Dr. Barret's first operation. Exception must also be taken here, as indeed was very properly done at the time, to the same gentleman's statement that there is no case of laceration which ought not to be operated on. It is just such rash statements, and the abuse of the operation consequent upon them, that have caused the profession in the old world to look with suspicion upon the operation, and refuse, until quite recently, to put it to the test of personal experience. It is well, too, to protest against the statement that the operation is rarely followed by fertility. There are now on record too many cases where fertility has followed the operation, although sterility existed before. Since, however, statistics in this particular instance have failed to establish the fact satisfactorily, recourse to a *priori* argument is allowable, and on such ground it can be maintained that, if the operation be properly performed, the chances are greater in favor of fertility than they were before the closing of the rent, and this, too, in face of Dr. Gehrung's assertion that he knows "of a good many cases of extensive laceration where there was almost too much fertility;" so that it seems to him that the laceration appears "to conduce to some physiological condition which favors conception." For usually, on the contrary, it is a pathological condition, whether cervical catarrh, corporeal catarrh, or displacement, which prevents conception.

EGBERT H. GRANDIN.

ABSTRACTS.

1. Auvar: The Incubator for Infants (Reprint from the *Archives de Tocologie*).—This apparatus was first used at the Maternité by Tarnier in 1881, for the rearing of prematurely-born children. It consists of a

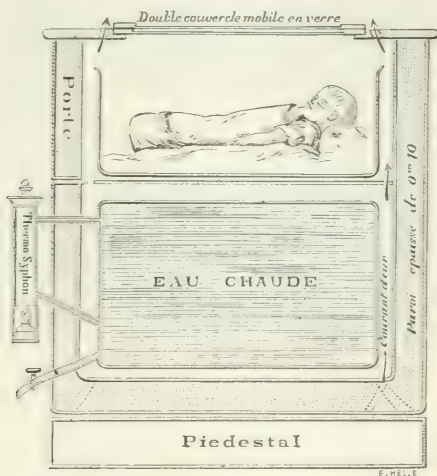


FIG. 1.—Section of hospital incubator.

wooden box with walls 0.10 to 0.12 cent. thick, filled in with sawdust to prevent loss of heat. A central partition divides the box into two compartments, the one for hot water, the other for the infant's cradle. A metal case, of a capacity of about 71 litres, fits into the lower compartment, leaving a space of 2 to 3 centimetres between its walls and those of the box for the free circulation of air from below upward. The capacity of the upper compartment is about 86 cubic centimetres; there is free circulation of air between it and the lower compartment, and it is in communication with the outer air by means of two openings, the one, on its upper surface, shut in by a double plate of glass, the other, laterally, opening like a door, and allowing exit to the cradle. In each corner of this upper compartment is a hole for the escape of the heated air from below. To the lower compartment, containing the hot water, is attached a thermo-siphon by an upper and a lower tube. When the lamp under this siphon is lighted, the heated water flows through the upper tube into the chamber, displacing an equal amount of water which flows back to the siphon. Thus a current is established, the temperature of which can be raised to the desired point. In cold weather it has been found necessary to light the lamp three times daily, allowing it to burn each time about two hours. The lamp should be extinguished when the temperature in the upper compartment is about two degrees above the heat desired. The registering thermometer may be laid alongside the infant.

For use in private practice Auvard has modified the instrument as follows: In place of the thermosiphon and the hot water reservoir he has substituted hot-water flasks. The partition between the two compartments is incomplete so as to allow free circulation of hot air. He

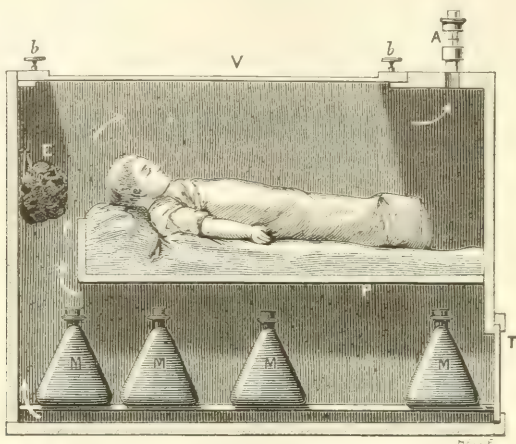


FIG. 2.—Section of incubator for private practice.

has found that by occasionally changing the flasks and keeping them filled with boiling water, the desired uniform temperature may be obtained. The flasks are placed in the lower compartment. Should the

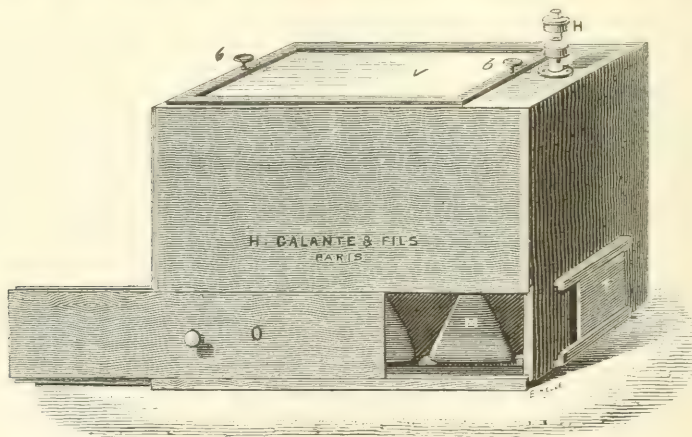


FIG. 3.—External appearance of Fig. 2.

temperature in the upper compartment rise too high, it can readily be lowered by opening its door. The apparatus is to be placed where no draught can strike it, and should be on a level, else the ventilator he has adapted to the upper compartment will not work properly.

In general, a temperature of 30° C. will suffice. It may rise to 34° or 35° without danger. At the *Maternité*, if the infant be strong enough, it is nursed at regular intervals; if it be premature, it is fed with asses' milk every two or three hours. The infant is taken out of the incubator

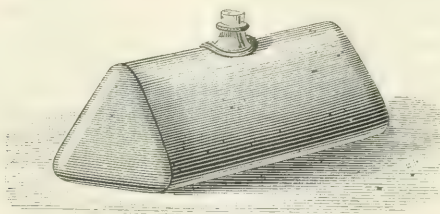


FIG. 4.—Hot-water flask for Fig. 2.

to be fed; its clothing is essentially the same as though it were reared externally; it receives a daily bath. The following table shows at a glance the results obtained at the *Maternité* since 1881.

	TOTALS.	LIVING.	DEAD.
Premature births.....	93	62	31
Feebleness	6	4	2
Cyanosis	5	5	0
Edema	25	21	4
Difficult respiration.....	5	2	3
Apparent death.....	4	3	1
Marasmus	3	1	2
Syphilis	4	4	0
Instrumental delivery....	2	2	0
Fracture	1	1	2
Congenital defect.....	3	0	3
Totals.....	151	105	46

To mention more particularly some of the cases: A premature infant (six months six days) remained in the apparatus forty-six days, and then was taken out alive and in good health; a seven months' child, weighing 1,400 grms., remained twelve days and came out in good condition; an edematous seven months' child, weighing 1,910 grms., and one of six and a half months, weight 1,650 grms., remained eleven days in the incubator, and were saved; only four deaths out of 21 cyanotic infants, whilst ordinarily the mortality is about 16 out of 20, according to DePaul. To put the statistical results in a different light: Given all the children under 2,000 grms. at birth, born at the *Maternité de Cochîn* in 1882, and at the *Maternité de Paris* from the first of April, 1879, to July 31st, 1881, and comparing the mortality rate with that of those reared by the incubator, the result for the two first is 66 per cent to 65 per cent against 38 per cent for the last—a vast improvement.

The remainder of A.'s paper is devoted to a consideration of the alteration of pulse and temperature affected by a sojourn in the incubator, and a comparison of the results obtained from the use of the continuous bath—an idea original with Winkel—and from the Incubator. In general, he found that both pulse and temperature ranged higher whilst the

child was in the incubator, the difference being less the nearer the infant is born at term. For instance, an eight months' fetus placed at birth in the incubator had a pulse rate of between 150 and 160 for the first four days, the respirations attaining a mean of 90. About the sixth day these functions approached the normal, the respiration, however, remaining a little more frequent. This increased heart rate is obviously of value in certain cases, as, for instance, where the infant is suffering from edema. A.'s conclusions in regard to the continued bath are that whilst it is more effective than the incubator in restraining loss of temperature, still, owing to the greater difficulty in applying it, owing to the necessity of more constant watching, and again owing to the fact that it cannot be used as continuously as the incubator, preference should be accorded to the latter.

A valuable bibliographical index is appended to the paper.

EGBERT H. GRANDIN.

2. Megrat (Luneville): The Occurrence of Peritonitis during Pregnancy (*Annales de Gynécol.*, October, November, 1883).—The paper is based on a collection of thirteen cases, of which seven are personal. The occurrence of a peritonitis during pregnancy is thus seen to be a rarity, no further cases being found on record after a careful search. The pathogeny of the affection is still obscure, owing to the limited number of autopsies of women affected with peritonitis during pregnancy. It is probable, however, that the affection is rarely idiopathic, usually being secondary to the rupture of a cyst of the ovary or tube, often simply a reawakening of an ancient peritonitis. No special determining causes exist. Trauma aside, we must often look for an explanation of its occurrence in the fact that the pregnant woman is peculiarly susceptible to inflammatory action, owing to the great increase in weight of the uterus and its annexes, the greater absorptive power of her lymphatic system, and the larger amount of fibrin in her blood during the latter months of pregnancy. The woman's greater susceptibility to moral impressions may also have a predisposing effect. M. was unable to find a greater susceptibility in primiparæ than multiparæ, or *vice-versa*. The symptomatology in nowise differs from the ordinary. Of the cases noted by M., in three only was there an initial rigor. Pain is a constant symptom, localized usually, in the beginning, to the right of the uterus, radiating across the abdomen. In one case the pain began simultaneously in the epigastric and hypochondriac regions without spreading to the pelvis. Constipation or diarrhea existed indifferently, in certain cases being relieved or ceasing on the expulsion of the fetus, as though the lessened weight of the uterus not only allowed free passage to the fecal matters, but also relieved the congestion and thence the irritability of the intestines and stomach. In two cases the diarrhea lasted up to death. The peritonitis appeared under its two forms—general and localized, the former most frequently. The plastic infiltration in three cases completely masked the spherical contours of the gravid uterus, in one case so entirely that the enlargement of the uterus at the sixth month of gestation was only discernible on deep percussion. In one case, sudden, intense pain, followed by death in twenty-four hours, were the only symptoms of a peritonitis due to rupture of a tubal abscess. Three cases of acute peritonitis terminated fatally in six, eleven, and fourteen days; one case of

the like nature recovered in eleven days. In six cases there resulted cure either by absorption or limitation. Of eight cases, collected by M. Nepveu, where the peritonitis was due to rupture of ovarian cysts during pregnancy, there were five deaths. As for diagnosis, it is an easy matter when there exists no complication. Far otherwise when, as in certain of M.'s cases, there exist, as complications, either acute gastro-enteritis, typhoid fever, salpingitis, etc., etc., or when the onset of the pains accompanying the premature labor results from the same cause as the peritonitis, or follows on the peritonitis itself. Under prognosis it is necessary to consider (A) the influence which the inflammatory process may exercise on the pregnancy, and (B) the influence of the pregnancy on the peritonitis.

(A) Three subdivisions are necessary here:

1. The influence on the course of the pregnancy. 2. On the life of the child. 3. On the life of the mother.

1. Whilst by appropriate and timely treatment the inflammation may be controlled and the pregnancy go on to term, this is, unfortunately, the exception. Once generalized, however, the inflammation inevitably causes miscarriage. Sudden death of the mother may occur from perforation before the product of conception has had time to escape. In another case, the beginning of the miscarriage may date from the onset of the peritonitis. Frequently it is not the peritonitis alone, but its complications, which induce the miscarriage. As for the influence which a partial peritonitis, arising from the simultaneous presence of the gravid uterus and large ovarian cysts, may exert on the course of pregnancy, the data are as yet insufficient for positive statement.

2. The inflammatory process may either cause the expulsion of a living but not viable fetus, or else kill it in its mother's womb. From M.'s observations of ten fetuses, six were born alive—two at term and four prematurely—one at five and a half months, one at six, and two at seven months. Of the four deaths resulting immediately from the mother's acute sickness, two were dead before the Cesarean section could be performed, one at eight months, the other at term. That the death of two others was not due to any cause acting during labor is proved by their presenting the appearances of death antedating by several days the onset of the labor. That there are other causes of death besides the mere fact of peritonitis is proved by a case of twins where the fatal issue was evidently due to the septic condition the mother was in, added to the inflammation.

3. Although the peritonitis may prove fatal to the mother, either before or sooner or later after delivery, it need not necessarily leave any trace of its existence or affect in any way the future health of the mother. It is, of course, apparent that a peritonitis, awakened by the presence of large ovarian cysts, may leave traces behind which would prove troublesome later on during the performance of ovariectomy. To return to M.'s observations, of the thirteen mothers, six survived, two going to term, and the remainder aborting at from five and a half to eight and a half months. Eliminating three cases from the total as being not explicit enough, the corrected result is ten deaths, three survivals. Of the fatal results, however, a closer analysis shows that only two died of uncomplicated, simple peritonitis. The prognosis for the mother is the more unfavorable the farther advanced the pregnancy; and the

prognosis will vary according to the causal factor of the peritonitis. For instance, if it be caused by rupture of tubal abscess or ovarian cyst, death may occur immediately. Again, it is evident that a simple, uncomplicated peritonitis is less grave in its omen than a complicated. Likewise, a localized peritonitis allows of a more favorable prognosis than a generalized.

(B) Pregnancy has a very unfavorable reciprocal influence on peritonitis, and this influence is the more unfavorable the further advanced the pregnancy. The simultaneous occurrence of premature labor and peritonitis renders the prognosis especially bad.

The treatment may be summarized in a few words. For an acute attack, leeches, blisters, opium; when the attack is adynamic, the result of sepsis or complications, cold applications, mercurial inunctions to the point of salivation are indicated. Internally, in both cases, quinine, pills of carbolic acid, stimulants. Should an attempt be made to prevent miscarriage? Should it be allowed to occur? Should it be induced? The remedies above mentioned, whilst having a benign influence on the peritonitis, will likewise tend to prevent miscarriage. M.'s observations would seem to prove that when miscarriage takes place spontaneously, the effect may be a good one; but the induction of miscarriage, on the strength of these observations, cannot be assented to.

[It is impossible to do full justice to this interesting paper in the course of an abstract. The salient points have alone been noted. The paper, particularly the reported cases, deserve careful study.]

EGBERT H. GRANDIN.

3. André Petit: Conception during Amenorrhea (*Annales de Gynécologie*, March, April, June, August, October, 1883).—After a historical retrospect and a discussion of the ovular theory of menstruation, the conclusion is drawn that amenorrhea is most frequently symptomatic of absence of ovarian function, and, in consequence, is accompanied by sterility. Seeing, however, that impregnation not uncommonly occurs without that sanguineous flow which is the accompaniment of ovulation, this conclusion must be so far modified as to include cases where ovulation takes place without any coincident red uterine discharge, or true menses. What is the most rational explanation of this curious anomaly in the functions of reproduction? Ordinarily, in the adult female normally constituted, from puberty to the menopause, when the Graafian follicle ripens, the ovarian excitement spreads to the genital organism in general, and congestion of this organism ensues, accompanied by sensations known as the *molimina*. When this congestion reaches its height, there occur in the uterus vascular ruptures and epithelial desquamation followed by more or less hemorrhage, which constitutes the menstrual flow. Now, owing to uterine or general conditions, this hemorrhage may not occur, but instead a white discharge, coming, probably, from the uterine glands. Such a discharge will usually be found in amenorrhoeic women who are capable of conception. It lasts from four to six days, with the ordinary menstrual *molimina*. It is not invariably white, but may be reddish in varying degrees, only stopping short, as it were, of the habitual menstrual color. In addition, there are cases where there is no discharge at all. The following abstract of a case from the practice of Dr. Burdel is an instance of the kind. He was called

in haste to see a nullipara, æt. twenty-one, and found the infant lying between her legs attached to the inverted uterus through an adherent placenta. This was removed, the uterus reduced, and all hemorrhage stopped. A few days after, the patient was attacked by an acute puerperal metro-peritonitis. Her convalescence was tedious, owing to an accompanying intermittent fever, phlebitis, angioleucitis. At the end of eighteen months, she was restored to normal health. The menses never reappeared, however, and yet at the end of two and one-half years she was delivered at term, and still a third time in sixteen months to be followed by a fourth child at the end of six years. In this woman's case, no leucorrhœa was ever noted. M. Gallard, to whom the case was related, explained the anomaly on the supposition that the uterine mucous membrane was so profoundly affected at the time of the first labor that it was never able thereafter to respond to the ovarian stimulus, and that the absence of any white discharge was due to the fact that the uterine glands had been destroyed. Still another cause of the absence of the normal menstrual flow is the presence of a profound anemia. That women so affected still ovulate is sufficiently proven by a case related by De Sinety concerning a young woman who died of phthisis, and at whose autopsy a recent corpus luteum was found, though she had not menstruated for many months. A similar case has been published by Dr. Vermeil. These women, besides, retain their sexual appetite. (This is hardly a proof of the continuance of ovulation, for women whose tubes and ovaries have been removed do not in consequence lose their sexual appetite.) P. himself relates some striking cases where white menstruation accompanied ovulation. As for the amenorrhœa of lactation, it is physiological, and the same is true of the amenorrhœa of pregnancy. Most authors agree that during pregnancy the ovaries cease to functionate. And yet there are cases on record where menstruation has continued during pregnancy, and in others where the menstrual flow has first appeared during the gravid state. It is an open question as to whether in such cases the flow is really menstrual in the sense of coming from the uterus. (The following cases are interesting instances, whatever the true explanation.)

A patient of Barbieri's, married at seventeen, conceived at twenty-three, though she had never menstruated: at the fifth month the function was established and continued periodically up to term. It then ceased to reappear: similarly during the next pregnancy. She had no supplementary flow.

Dr. Gérard Marchand reports the case of a patient of twenty-five, nullipara, who conceived and went to term without entire suppression of menstruation, though the flow was only slight in amount, roseate in color, and followed by leucorrhœa. (The query suggests itself: Did this woman have an erosion of the cervix?)

Finally, a case of Négrier's, where the patient had never menstruated, though she had all the prodromata every twenty-five days, and yet became pregnant and had, during the first three months, a bloody flow coincident with the molimina, which, in her case, took the place of menstruation.

The fact that conception may occur during the physiological amenorrhœa of lactation is indisputable, and is sufficient proof that ovulation need not necessarily show itself outwardly by a bloody flux. In nurs-

ing women, a large part of the blood is attracted to the breasts, to be utilized in the formation of milk, whence the woman is in a condition of relative anemia—a sufficient cause, as has been seen, of amenorrhea.

A further cause of amenorrhea noted by P. is the existence of Basedow's disease. He relates a striking case of a woman regular in her menstruation, and bearing children before the development of the goitre; whilst after, though she conceived and hence must have ovulated, there was no return of the menses.

P. then relates several interesting cases of conception during amenorrhea. Since this fact alone concerns us here, it is unnecessary to note other points in their histories, except the fact that, at the periods corresponding to the menstrual epochs, the patients had the customary molimina, in some with, in others without any white discharge.

The last question propounded by P. is: What is the proper advice as to marriage to give a young woman who either has never menstruated at all, or else, after several menstrual periods becomes amenorrhoeic?

In the first case, if the sexual organs are present and normally formed, it is possible we have to do with tardy appearance of the menses, or there may exist some diathesis or other whose effect is to retard ovulation. It may be, too, that the young girl has molimina accompanied by leucorrhœa—white menses. If now the result of examination leads to the belief that ovulation has not as yet taken place, it is well to dissuade from marriage, to temporize and await the first awakenings of the function of reproduction. If, on the other hand, the girl has molimina and white menses, marriage may be allowed, with a strong probability of fruitfulness. Perhaps the very fact of sexual intercourse, through its excitator effect on the whole sexual system, will cause the appearance of true menstruation (in the sense of being red).

The second alternative (menstruation and then amenorrhea) offers less difficulty in the way of prognosis. The only question to be answered is: Why the amenorrhea? The uterine mucosa may be at fault: the ovaries may have ceased to functionate, or, though functionating, they may not stimulate the uterus sufficiently to cause a red discharge. We must find out the circumstances which ushered in the amenorrhea. Was there an ovaritis? Is the general health at fault? Is she pregnant? Is there disease of the uterus of any kind? Such are the questions to be settled, in order to form an opinion as to the possibility of conception. If the girl has molimina, if she has white menses, if both constitutional and local vice are absent, marriage may be allowed. It is impossible, of course, to affirm she will conceive, even as it is impossible to affirm this of a woman who menstruates regularly and normally. All that can be said is, she may become pregnant and ultimately the mother of many healthy children.

E. H. GRANDIN.

4. Keith: Report of Cases Treated in Ward XIX. of the Royal Infirmary, Edinburgh.—The period of time covered by this report is three years. The operations, with a single exception, were ovariectomies and hysterectomies. There were sixty-nine of the former and ten of the latter. The complete Listerian system was not used; that is to say, the spray was dispensed with “as being of doubtful efficacy in preventing septicæmia, and as sometimes being in itself a source of danger in certain long operations and in some conditions of the constitution.” To analyze

the cases in general: Of the seventy-nine cases there were five deaths; the hysterectomy cases were all successful. The cautery was used fifty-six times in the treatment of the pedicle; the pedicle was treated extra-peritoneally in all the hysterectomy cases but one; where the cautery was not used ligatures were, in a few cases followed by the cautery. In the majority of the cases the adhesions were firm and extensive; thirty-one of the cases had been tapped from one to nine times, but "no harm ever resulted from tapping;" on the contrary, great improvement in cases of large tumors. The weight of the tumors varied from five, the lowest, to seventy-seven, the highest, pounds. Both ovaries were removed nine times; the hysterectomy cases were all for fibrous tumors, and the ovaries were removed at the same time. The time required for operation varied from seventeen minutes to two hours and a quarter. Of the five fatal cases, one was a hysterectomy and double ovariectomy combined; four were ordinary ovarian cases, and died severally of rapid septicemia, two supposedly as the result of carbolic acid poisoning (in one acute nephritis was the secondary cause, in the other no cause could be determined); in the fifth case the cause was obscure, and it was the only death amongst the cases where the spray was not used; whilst the three other cases had had the benefit of a 2½% carbolic spray. The difference in the mortality of cases operated under pure Listerism and without is (the number of cases being increased to eighty-two by the addition of two cases of removal of the uterus and tubes, and one case of interstitial pregnancy):

CARBOLIC-ACID SPRAY CASES.

	CURED.	DIED.	TOTAL.
Ovariectomy	18	3	21
Hysterectomy for fibroids	2	0	2
Batley's operation	0	1	1

Twenty-four cases with four deaths (16.66%).

BORO-GLYCERIDE SPRAY CASES.

	CURED.	DIED.
Double ovariectomy with hysterectomy	0	1
Hysterectomy for fibroid	1	0

Two cases with one death.

NO SPRAY.

	CURED.	DIED.	TOTAL.
Ovariectomy	46	1	47
Hysterectomy for fibroids	7	0	7
Batley for fibroid	1	0	1
Case of interstitial pregnancy	1	0	1

Fifty-six cases with one death (1.78%).

To give brief abstracts of a few of the cases:

CASE III.—A feeble, half-starved patient, æt. thirty, married. Had a large ovarian tumor, which has been tapped once. An ulcerated uterus lay between her thighs; rectocele and cystocele. As distress from tension

was very great, operation was performed two days after admission. Adhesions very firm and extensive. The ribs were elevated, and under them a large, unlooked-for mass of semi-solid tumor. All was removed through an opening of six inches. The pedicle was cauterized; tumor weighed forty-two pounds; length of operation, fifty minutes. Patient discharged in thirty days in a feeble condition. Nine months after, confined at term of a healthy child.

CASE VII.—Æt. twenty-eight; moderate-sized ovarian tumor surrounded by free fluid. This fluid microscopically was pronounced to come from a burst cyst. Pregnancy in a retroverted uterus existed. The displacement was rectified. The tumor weighed twenty pounds. Pedicle treated by the cautery. Confinement at term seven months after operation.

CASE XI.—Æt. twenty-two. Tumor of five years' growth and sarcomatous, filling the abdomen; the bladder drawn up to umbilicus; rectum not implicated; before operation no trace of uterus. Length of operation, two hours and one quarter; nearly one hundred vessels had to be ligated. bladder and ovaries and uterus seemed completely incorporated in the tumor. The mass was dissected away, and weighed forty-one pounds; Convalescence was slow. Patient discharged in three months, cured.

The hysterectomy cases were all of fibrous tumors. The steps of the operation were essentially the same in all the cases. In some of the cases the spray was used, in others not; in others the fact is not noted. Its use, however, apparently did not influence the results at all. Extreme cleanliness was a cardinal rule. Soft iron wire was usually the ligature of the broad ligament.

[A few noteworthy points in these cases are: 1st, the small amount of ether required for unusually long operations. For instance (Case VI.), though the operation lasted one hour and three-quarters, only one ounce and three-quarters of ether were used; and, 2d, the carbolic spray was abandoned because in two of the fatal cases the result could not be laid to any other cause. It is but fair to add, however, that the spray apparatus was placed very close to the patients, and likely enough, had the surrounding atmosphere been simply antisepticized, instead of the patients' abdominal cavity being freely and for a long time washed by the spray, the result might have been different.

This brief abstract simply aims to give, generally, an idea of the operations undertaken by Keith. All the cases are worthy of careful perusal.]

E. H. GRANDIN.

5. Behm (Berlin): The Combined Method of Turning in Cases of Placenta Previa (*Ztsch. f. Geb. u. Gyn.*, Band IX., Heft 2).—This method of operation was suggested by Braxton Hicks in 1861. The literature of it includes the experience of Hicks, Kuhn, Fasbender, A. Martin, Schröder, Kaltenbach, Witzel, and Hofmeier. In the author's extensive experience of fifty-three cases of placenta previa, all but the first thirteen were treated by Hicks' method. Of these thirteen, which were treated by tamponade and internal turning, the mothers died in four. Fourteen children were born, of which ten were either dead at birth or died soon afterward, and these results may be considered the average for both mothers and children by the old method. In Hofmeier's publication of thirty-seven cases, all of which were treated by

the combined method, only one mother died and sixty-three per cent of the children. Of the forty cases treated by the combined method by the author, there were no maternal deaths, and seventy-seven and five-tenths per cent mortality among the children. In eleven of the forty cases, the placenta was marginal; in nineteen, lateral, and in ten total or central. Bad as this showing is, as far as the children are concerned, the author thinks that this must continue to be the most approved method of operation in cases of placenta previa, on account of the excellent results which it gives for the mothers. It can be no mere accident that, in the author's first series of thirteen cases, four mothers were lost, while in the next forty cases none perished. The reasons for these results are formulated by the author as follows: (1) The combined method of turning affords an opportunity for intervention at the earliest possible moment, *i. e.*, as soon as the os uteri is penetrable by one or two fingers, and when the patient has, as yet, lost very little blood. (2) The breech which is brought down and fixed with only moderate traction affords a complete tamponade. (3) Sepsis is avoided. The period which follows the turning, in which the cervix is dilating, is not less devoid of peril than its antecedent one, since it is then that dangerous rents of the cervix may occur, and also the purely atonic hemorrhages which may be quite alarming. The combined method here again proves its advantages, for it does not necessitate immediate extraction, and thereby (4) sufficient time is gained for the application and action of proper restoratives, and the gathering of the patient's strength. Also (5) those severe hemorrhages are obviated which have often proved fatal to mother and child, and which have occurred when one was wholly occupied in effecting a rapid extraction. In most of the cases detailed in the author's table, turning was accomplished before the rupture of the membranes, as is always desirable, on account of greater facility. In cases in which the fetus is mature, the author advises that two fingers be always introduced to effect the work in utero, while in cases in which the fetus is immature, one finger may suffice. Preliminary tamponade is not favored by the author, as it has not prevented hemorrhage in his experience. When the placenta is lateral or marginal, enough of it is to be pushed aside (in preparation for the combined turning) to admit of the necessary manipulations. If the placenta is central, the author believes that much less blood will be lost by passing the fingers directly through it than by any other means. In this view, he is in accord with Hofmeier, but not with Spiegelberg and Kuhn. This procedure may result in the separation of much or all of the placenta and the death of the child, but the mother's life depends upon it. Such was the author's experience, his record being seven cases of placenta centralis, seven children lost, seven mothers saved. The tampon is to be avoided, in the author's opinion, excepting in the earliest hours of labor, before the os is dilated. A continuation of it until there is sufficient dilatation to admit of turning (by the old method) is pretty certain to result in puerperal fever or some form of sepsis. The length of time which may elapse, after turning has been accomplished, before extraction can be safely attempted varies (in a table of twenty-four cases given by the author) from half an hour to eleven hours. In the majority of cases, however, it was under three hours. Injections of ether and the various forms of transfusion have not proved effective resuscitants in the author's hands, and the evil con-

sequences which may follow their use suggested the forcible comparison of *driving out the devil by Beelzebub*. The atonic after-hemorrhages which sometimes occur are mostly from the lower uterine segment, otherwise known as Duncan's zone. These are most effectually stopped when the turned breech of the child securely tampons the passage, compression and thrombosis of the open sinuses quickly following the detachment of the placenta, as already described. Supplementary to the cases already given, the author mentions five additional and recent ones, in one of which there was internal hemorrhage after turning by the combined method. The author thinks this may have been due to the fact that the operation was done in the absence of all uterine activity, and he proposes, in the future, to operate only when uterine pains are present.

A. F. C.

. Veit (Berlin): The Cause for the Peculiar Form of the Female Pelvis (*Zeit. f. Geb. u. Gyn.*, Band IX., Heft 2).—In addition to scientific investigation, such a subject must involve historical and speculative details. Litzmann enumerates as the factors in producing the form of the pelvis, 1st, the original position, development, and growth of the elements of the pelvis; 2d, the weight of the superimposed body; 3d, the resistance of the bones and cartilages of the pelvis; 4th, the traction and pressure of the attached muscles. Fehling, in his work upon this subject, concludes, 1st, that the original direction of the fetal pelvis is transverse, and that this position is assumed very early in fetal life; 2d, that differences in sex are early indicated by differences in the form of the pelvis; 3d, that the pelvis demonstrates a bending in the long diameter of the sacrum; 4th, that the analogy of the transverse trend, etc., with similar peculiarities in rachitic pelves in adults, goes to prove that there is, in such cases, only a permanency of fetal conditions. With these conclusions the author takes issue, stating, and giving reasons therefor, that the fetal rachitic pelvis is of no value in its bearing upon subsequent shapes of that structure, and that neither the conclusion as to permanency of form nor that as to the action of muscular force in this direction is authorized. The study of the pelvis of new-born infants is valuable, however, in connection with the subject in hand. The following fact is emphasized, viz., that the *promontory* which at first lies *beyond* the superior strait, at a later period lies within the plane of the same, and this being the case, the original form of the pelvis cannot be permanent. In addition to the conclusions already mentioned, the author finds that the normal pelvis of the adult differs in its conformation from the normal pelvis of the new-born, the factors which produce the change being extra-uterine; that pathological formations of the pelvis occur in the new-born in connection with general and local deformities, and that these last-mentioned conditions stand in nongenetic relation with the similar conditions in the adult. For certain varieties of faulty pelvis, Litzmann's theory of the effect of the pressure of the superimposed body offers a sufficient explanation, but the other elements in his theory (muscular traction, etc.) do not as yet rest upon sufficient grounds of proof.

A. F. C.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY . . . GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

TRACHEAL DIPHTHERIA.

BY

T. B. CAMDEN, M.D.,
Parkersburg, W. Va.

IN the December number of this JOURNAL I notice Dr. J. Fewsmith, Jr.'s, article on the above disease. My experience of a similar disease dates back to 1856-8, at which time I met with a number of cases, and with few recoveries. It showed itself in a sporadic form, but all the children of a family would have it. I remember that three died in one family. Then another case would show itself from three to nine miles distant, about the same time. Two years subsequently the first case occurred fifty miles away, and there were three cases in one family, all of which died. Most of the cases were in the country, in the mountainous parts of West Virginia, the first cases on the top of a mountain, where the air and surroundings would be considered very healthy, whence it jumped from one locality to another miles apart. I cannot think the disease was due to local poison, but rather to atmospheric causes. One or two cases only were in town. At the time of the appearance of this disease little was known of, and very little was heard of, diphtheria. Very soon, however, epidemics of this nature were noted, and I had no doubt then, and have none now, that I encountered the "croupal form of diphtheria," which is the most intractable and fatal disease I

have ever met. The initiatory symptoms were usually slight febrile disturbance, slight sore throat, which gave little cause of trouble or alarm to either patient or friends until about the seventh day, when the peculiar ringing, croupous cough set in, which gradually became tighter until about the ninth day, when suffocation ended the scene. So constant was this march of symptoms that it was known in the country as "nine-day croup." Very little exudation could be detected, as I remember, until about the seventh day, when a slight whiteness could be seen on the epiglottis. The disease appeared to be situated in the larynx and trachea; whether travelling up from trachea or down from larynx was hard to determine, but the effect was the same—strangulation. Whether "laryngeal" is not a more comprehensive name than "tracheal" diphtheria is to be considered. The glands of the throat were not swollen. The age of persons attacked was from one to sixteen years.

Various modes of treatment were tried, but without satisfactory results.

Perhaps had the disease been recognized early the results would have been otherwise; but so insidious was its progress that its true character would not be known until the peculiar shrill, ringing, croupal cough, like a knell, sounded the doom of the sufferer, and then the physician was called.

One recovered by general treatment and a blister over the throat, which showed a heavy deposit of the diphtheritic membrane. Whether this translation diverted the formation of membrane in the trachea is a question. One recovered under general treatment, together with sol. persulph. iron to throat. I opened the trachea in one case which was dying from strangulation. It revived, and lived two days; ate, and was bright and cheerful, but the membrane and mucus filled the opening, and it died from exhaustion. Since about the year 1861 I have not seen a single case of the "croupal form" of diphtheria, and sincerely trust I may never meet it again.

MORPHINE BY SUBCUTANEOUS INJECTION IN INFANTILE CONVULSIONS.

BYC. C. P. CLARK, M.D.,
Oswego, N. Y.

REFERRING to the accumulating and now conclusive testimony in current obstetric literature to the almost absolute control which morphine, in sufficient quantity and subcutaneously injected, has over eclampsia, I desire to call attention to its equal efficacy in arresting infantile convulsions.

Not many of your readers will deny that the customary treatment of this frequent and serious affection by warm baths, emetics, purgatives, mustard plasters, friction, and so on, has proved very unsatisfactory. It is seldom that we can feel confident of having determined by them the duration of the attack; and in most instances the best we can do is to beguile the anxiety of the friends of the patient by one device and another till the fit passes off of itself.

Besides the fact that the convulsion is often over before we reach the patient, the parents of the little sufferer so constantly have strong objections to seeing a cruel needle thrust into its arm, and even more to the injection there of so powerful and dangerous a medicine as morphine, that it is not every day that we can get a chance to try this treatment. Only two opportunities have I myself had since I reached the determination to use it in my next case.

In August, 1882, I was asked to visit the daughter of Dr. M., aged about eighteen months. She had been in an unintermitted convulsion, though with some remissions in its violence, for about two hours; the various remedies tried by a very intelligent practitioner who was in attendance having shown not the least effect. No probable occasion of the attack could be ascertained, and it was surmised to be of cerebral origin. With the approval of the attending physician I injected under the skin of the arm about one-sixth of a grain of sulphate of morphine. Within two minutes the convulsion entirely ceased, and after a quiet sleep of three or four hours the child awoke apparently well. Some twenty-four hours afterwards some threatenings of its return

showed themselves, but these readily yielded to moderate doses of Dover's powder. It was discovered by the mother, however, before many days, that the faculties of the child had fallen back into the condition of early infancy. Her skill of speech, for instance, in which she was well advanced, had altogether deserted her, confirming our diagnosis of the centric origin of the disease. Though now of bright appearance, and constantly improving, she is not yet quite herself.

The other case was that of Master M. M., four and a half years old. He had been sick for some days with a sort of bastard diphtheria and inflammation of the middle ear, but had so far recovered that I had discontinued my visits to him, when I was called out of bed early one morning, February 2d, 1884, to visit him in a "fit." When I reached his bedside he had been for half an hour in as violent convulsion as I ever saw, with no abatement or sign of termination. I injected a good dose of morphine, and in less than one minute—within thirty seconds, his father says—he was as still as the proverbial mouse, nor had any threatening afterwards of the return of the disease. This attack, I had little doubt, was induced by some spread or transfer of irritation from the inflamed ear to the brain. He has now recovered altogether.

Since these cases I am almost prepared to tell my employers, as did the other quack, that if they will throw the sick baby into fits, I can cure him.

P.S.

MR. EDITOR:—In your communication, you suggest that I "make it more complete by adding a few words on the subject of the susceptibility of children to opium;" adding, that my "treatment with morphine is so bold and unusual that few would dare follow it simply by the report of two cases."

(1) In obedience to this suggestion I have to say that no one can be better aware how easy it is to kill babies with opium, having myself unwittingly hurried out of life two or three of them by this means; but this was long ago.

(2) Extreme susceptibility to the influence of this medicine is confined to early infancy, when nervous energy, as well for resistance as for effort, is hardly yet awakened; I hope I should be properly cautious in administering opium in whatever form or way at this period of life. Nevertheless, I should not hesitate to give one thirty-second part of a grain of morphine hypodermically to a child four or six months old when I found it in persistent or recurring convulsions, nor to repeat it in fifteen minutes if the convulsions should continue or return.

(3) This period of non-resistance passed, I have still not dis

covered in children a degree of sensibility in this direction out of relation with what I shall venture to christen "the size of their vitality." That is, a child of two or five years old will stand as much opium, in proportion to the development at that age of the energies of the nervous system, as an adult. And when I am told of cases where what seemed a moderate dose has proved too big, I recall an instance where a child less than five years old swallowed and retained two grains of the sulphate of morphine, mistaken for quinine, without the subsequence of one alarming symptom, although, of course, I was alarmed, and this when he was already debilitated by a severe attack of scarlet fever. This, with regard to the supposed natural, normal, and therefore necessary impressibility of the constitution of children under the power of this narcotic.

(4) Undoubtedly there are instances of idiosyncratic susceptibility to the influence of morphine as well in infants as adults, but these are neither so common nor so extreme that we customarily regard their possible occurrence, or need to, in prescribing for a stranger, leastways if the occasion be pressing. Not many would hesitate to give a child at four years of age in great suffering five grains of Dover's powder or its equivalent, or one grain to an infant at four months, notwithstanding that he may have read in books of toxicology that such doses have sometimes proved fatal. For though it is a good maxim in jurisprudence that a hundred guilty had better be let escape than one innocent man be made to suffer, the corresponding maxim does not quite hold in the treatment of sickness. Here we must sometimes take a chance. *Anceps remedium melior, nullo?—quam nullum?*

(5) But the reason why "my treatment by morphine is so bold," according to your entitlement, is because the conviction is long settled in my mind that infantile convulsions are joined in a deep identity with eclampsia and epilepsy, as indeed their very aspect preaches, and that they all betray an extraordinary tolerance of opium. That this is true with regard to the last two of these maladies I have already offered some evidence and argument in the pages of this JOURNAL (Vid. Nos. for July, 1880, and April, 1881) which I have nothing now to add to, but which, if I am rightly instructed, the widening experiment of the profession is fast confirming. That it is also true in the convulsions of children I have no special proofs—unless the cases given above may be looked on as such—though I hardly myself considered their treatment "bold," and certainly not rash, but only the firm ground of obvious analogy to argue from. If your readers will

once try this practice, with whatever cautiousness they please, I am confident that, gathering courage as they go on, they will soon find that they have got the substantial mastery of a heretofore very troublesome and perplexing class of cases.

A CASE OF TRISMUS NASCENTIUM (?).

BY

IRVING W. SMITH, M.D.,
Charles City, Iowa.

A MALE infant, first-born, at full term, of healthy German-American parents; weight about six or seven pounds; fairly well-developed. It was seen by the physician only on the third day. The lower limbs were flexed and the feet inverted to the extent of partial dislocation of the ankle-joints, so that the parents supposed the child to have club-feet, and on this account called the physician. The *humeri* were both dislocated forward, and the arms considerably shortened, while the fingers were variously distorted by the muscular contractions. Even the features were distorted, the nose being distinctly flattened and retracted. There was general, tonic rigidity of all the muscles, increased by any attempt to straighten the limbs. The deformity was noticed at birth, but was then less marked.

Strangely enough the child was able to suckle and to swallow from a spoon. When not asleep it often cried weakly. The symptoms were reported not to have changed materially, and death occurred on the twelfth day.

TRANSACTIONS OF THE WASHINGTON OBSTETRICAL AND GYNECOLOGICAL SOCIETY.

Stated Meeting, February 1st, 1884.

The President, DR. S. C. BUSEY, in the Chair.

The paper of the evening was read by DR. SAMUEL S. ADAMS, on a case of

SUDDEN DEATH OF A CHILD WITH DIPHTHERIA.

The doctor said he referred to this subject on account of the many reports of sudden deaths occurring during the decline of this

disease, and the backwardness of the reporters in venturing an opinion as to the cause of this sudden dissolution. He then gave in detail the history of a case recently seen by him. The patient was a bright boy of sixteen months, well developed, with a good constitution, twelve teeth, and a fontanelle normal at the age. He was taken with convulsions on the morning of the 24th of December, 1883. Indigestion was suspected, and the remedies proper for its relief were given. As he was leaving the house, however, the mother asked him to look at her throat, as it felt sore. A dirty patch about the size of a pea was discovered on the right tonsil. This unexpected discovery caused the doctor to return and examine the boy's throat. His tonsils were found to be slightly enlarged and intensely hyperemic, but there was not the slightest trace of a membrane. At eight o'clock in the evening of the same day, the right tonsil was nearly covered by a membrane, the pulse was 150, and the temperature 104.5°. Dr. Busey was called in consultation, and agreed with Dr. Adams in making an unfavorable prognosis. The history of the case from that time was accurately given.

January 3d, 1883, 8 P.M. the doctor says: The pulse was regular, full, and with very good tension—better than at any time during his illness. Respiration was easy, no evidence of any obstruction in either the upper or lower air-passages, but very rapid; nostrils not dilated; no râles of any kind; no cough; skin cool and moist. Between 10 and 11 o'clock, however, he began to toss about, would give the most heart-rending screams, point about the room as if he wanted something, but after trying to satisfy him in every way and failing, his parents sent for Dr. Adams. The doctor found the child tossing and screaming, pulseless, blue lips, countenance livid, respirations above 100 and superficial, semi-conscious, face pinched, nostrils dilated, lungs edematous and rapidly filling; nose, hands, and feet cold; surface bathed in a clammy sweat. In less than half an hour he died. No autopsy was held.

The doctor considered the points of interest to be the simultaneous development of the disease in both mother and child, the source of the poison, and the cause of the sudden death.

Was the sudden death due to loss of heart power? The doctor thought not, because the heart had been acting remarkably well for sixteen hours, and when he left the patient, at 8 P.M., the pulse was 120 with excellent tension.

Was it because the stimulants had been withheld and the heart wore itself out? If this were true, why did not the pulse show the necessity for re-stimulation? From the history, paralysis of the heart must be excluded as the cause.

Was it due to traumatic pneumonia? He hardly thought this tenable, because there was no paralysis of the muscles of deglutition and no history of a foreign body entering the larynx.

Was it due to paralysis of the lungs? During the whole course of the disease there was nothing unusual about the pulse-tempera-

ture-respiration ratio until the 3d of January, when the pulse fell and the respiration rapidly rose. This break in the ratio made him apprehensive. What caused these rapid, shallow respirations? If the respiratory centre was irritated by the poisoned blood, why did the circulatory centre escape? As the glandular enlargement increased, the respirations became quicker. In Dr. Adams' opinion, paralysis of the lungs caused the child's death, or, in other words, paralysis of the vagus. Whether the cause lay in the respiratory centre or along the tract where pressed upon by the infiltrated cervical glands, he was not prepared to say.

DR. G. B. HARRISON, in opening the discussion, said the case reported was interesting in many particulars. The exudation seems to have extended upwards, contrary to rule. There was not the usual relation of respiration and pulse. In diphtheria respirations are less frequent usually, in reference to the pulse, than in croup; and this is relied upon as an element of differential diagnosis by dualists. Dr. Adams did not mention whether albumen had been found in the urine; nor did he state that any constitutional antiseptic (except alcohol) had been used. It was to be regretted that he had not experimented with some such agent as mercuric chloride. The doctor spoke of the heart's action as good, yet mentions that pulmonary edema existed. It is a matter of regret that an autopsy was not made: in its absence the cause of death must be conjectural. This was an undoubted case of diphtheria. The speaker seldom meets with cases of true diphtheria. He often sees membranous sore-throats. In Dr. John Harley's contribution, on enteric fever, to Reynolds' System of Medicine, attention is called to the relationship existing between scarlatina, typhoid fever, and diphtheria, and the occasional coincidence, in the same patient, of symptoms characteristic of these three diseases. The speaker, some eighteen months ago, met with a case of this sort—death ending the scene after four weeks. He was at a loss in making the certificate, and finally gave "typhoid fever complicated by diphtheria." An outbreak of scarlatina soon after, in the same household, led him to question the accuracy of his statement.

To revert to antiseptics. It is quite curious how the compounds of the bromine series and those of their ally, sulphur, have sustained their reputation. Inasmuch as the use, by Dr. Wm. H. Thomson, of New York, of bromine internally, has been recently referred to, it may be well to state that the details of his method have been recently given in the *Medical Record*. That gentleman claims that bromine is the only agent which can be tolerated in the system in sufficient quantity to effect the object.

Ollivier, in a letter to the Paris Academy of Medicine, has recently called attention to the importance of isolating pregnant women in epidemics of diphtheria, those affected being specially liable to abortion.

Alcohol might have been used more boldly in Dr. Adams' case; and notwithstanding the infancy of the patient, hypodermic injections of beef-juice might have done good.

DR. FENWICK asked Dr. Adams if he stated that the membrane disappeared ten days before death, and was informed that it was four days prior to death. Continuing, Dr. Fenwick said he had two cases, several years ago, in the same house, wherein the

membrane disappeared, two weeks in one case, and three weeks in the other, before death.

DR. PRENTISS maintained that the case reported was not one of sudden death. The child had been growing worse for four days, and then improved only to grow worse again, as was evinced by glandular swelling, etc. The case can be explained on the theory of multiplication of germs. The patient died from relapse of the disease. In this class of cases Formad found micrococci in the throat. In constitutional cases these germs multiplied in the blood and tissues. A child may die in twenty-four hours from a fatal form of septicemia. In Dr. Fenwick's cases, and others of the same character, death occurs from heart paralysis, due to nervous debility. The heart does not respond to the increased demand made on it. He has had a child to die from progressive debility. He had never been able to trace the cause of the disease. Writers tell us that it is due to a sewer-gas poison, but the speaker could not agree with those who held that opinion; on the contrary, he believes that the disease is induced by a contagium. In some cases which occurred in his neighborhood, the disease appeared to originate from the water-closet and refuse matter on the premises. Two out of three cases died from heart failure, the larynx remaining free from deposit. The speaker did not believe that croup and diphtheria are separate diseases, but thinks all should be called by the latter name. He did not include tonsillitis with white patches in the same category. Those cases with the ashy-gray membrane should always be called diphtheria. Some of these get well, others die.

DR. HARRISON recently had a case of croupous pneumonia where the crisis came on the eleventh day. There was a membranous sore-throat, of which the exudation could not be distinguished from that of diphtheria. These cases are not diphtheria; they get get well easily, but should be isolated, as they may become the starting-point of diphtheria.

DR. MCARDLE always believed the deposit to be a local manifestation of a general disease. May not the sudden deaths be due sometimes to migration of micrococci into the fourth ventricle?

DR. HAGNER does not agree with Dr. Prentiss in designating so large a class of cases diphtheria. The term "diphtheritic sore-throat" should be expunged. Such cases must be either one or the other. It is unscientific to call all of them diphtheria. Follicular tonsillitis cannot be mistaken. We have a patch appear on the tonsil, attended with chill and fever. The patch will disappear in forty-eight hours. This cannot be distinguished histologically from diphtheritic membrane. It is pseudo-membranous sore-throat. In many of these cases it is best to refuse to give a diagnosis and prognosis at first.

Diphtheria cannot be diagnosed with certainty within forty-eight hours unless the membrane appears in other places than the throat.

DR. LOUIS MACKALL.—In all cases except those due to closure of the larynx by membrane death is due to heart failure. Death too often results from the violence of the storm, but often the shipwreck comes from following the charts laid down.

DR. W. H. TRIPLETT has lost several cases of diphtheria, and always attributed the death to paralysis of the heart.

DR. ADAMS replied, and the Society adjourned.

ABSTRACTS.

Prepared by J. FEWSMITH, JR., Newark, N. J.

1. N. Wassiljeff: The Dependence of Diphtheria on the Lower Vegetable Fungi (*Abst. in Jahrbch. from Russian Lit.*).—There are three premises upon which the friends of the "Pilz" or fungus theory base their argument—*independent of the constant presence of the micro-organisms in the diphtheritic product and the diseased membrane.* These are, 1st, the increase or decrease of the fungi with the increase or decrease of the local disease; 2d, the contemporaneous occurrence of colonies of micrococci in the blood, lymph, kidneys, and other organs; and 3d, the transmission of diphtheria by inoculation with these product of the micrococci. The author replies to these points as follows:

Ad 1. It has not been fully proved that the number of vegetable elements does increase with the intensity of the disease, but even if it is so. it is easily explained by the fact that the diseased membrane offers so favorable a soil for their growth.

Ad 2. Plugging of blood and lymph vessels by colonies of micrococci is an occurrence seen not only in diphtheria, but also in other infectious diseases (Recklinghausen). The author has proved by experiments on frogs that these collections of micrococci in the blood are not formed till about twenty-four hours after death, and only when the animal was septically infected or had some open wound. Yet the micrococci must have entered the blood during life, for when one thigh of a frog was ligated before the septic infection, there were, after death, none of them in it. The formation of the colonies or collections is prevented during life by the circulation of the blood, and if we cause stagnation by ligaturing vessels, we can often cause these collections in animals during life. From all this we can see that the formation of these collections of micrococci in diphtheria has not much to do with its etiology.

Ad 3d. The proof of the transmission of diphtheria through inoculation with its products speaks not for the organized nature of the contagium, but only for its *fixed* nature. Many attempts at inoculation have failed, and careful examination of many others shows that in carrying them out some septic matter has been inoculated with the micrococci, and that the result was not diphtheria, but, like the infection with septic poison, an inflammatory exudation at the point of inoculation, and septicemia. If these experiments are correct, we must regard diphtheria as at first a strictly local disease, which the author thinks will have a sad effect on our method of treatment. He does not mean by his article to disavow the possibility of the existence of a vegetable micro-organism in the formation of the contagion of diphtheria, but believes that it has not yet been discovered or its existence proved.

2. Mischchenko: The Course of Temperature in Diphtheria (*Abst. in Jahrbch. from Russ. Lit.*).—This is a point which has been too little studied and is too often neglected. The author has paid strict attention to it in 105 cases, and reached these conclusions: On admission

into hospital the temperature in mild ("speckled") cases was from 39.0° to 40.5° C. (102° to 105° F.) [was not this herpetic tonsillitis?—J. F.]; in the croupous form, 37.5° to 39.4° (99.5° to 103° F.), and in the septic form, 38.0° to 39.4° (100° to 103° F.). That is the height of the fever is not at all proportioned to the severity of the disease. The temperature during the disease often shows wavy curves, falling for a few days till it reaches normal, then rising again, accompanied by a new exudation in the throat or at least an intense congestion of the membrane, or a swelling of the cervical glands.

3. Sellden: Treatment of Diphtheria by Cyanide of Mercury (*Abst. in Jahrbch.*).—Dr. H. Sellden uses a solution of 1 centigram of hydrargyri cyanuretum in 100 grams of water, giving a teaspoonful every hour, day and night. He at first used a stronger solution, but was obliged to abandon it. He does not paint the throat at all, but lets older children gargle with the solution every half-hour or hour. In 1881, of 5 patients treated in this way only one died, and in that the remedy was not used until a short time before death. In the year 1882, out of 32 patients treated only 2 died, and of those who recovered very few had symptoms of paralysis. All these 37 cases were genuine diphtheritis, and in all of them the contagion was shown with great probability. Beside these cases of genuine diphtheria, S. has treated about 200 cases of angina tonsillaris et faucium with hydrarg. cyan. and accomplished very rapid cures. Since he is convinced that a simple angina may pass over into a genuine diphtheria, and since he has observed cases of an abortive form of diphtheria (general malaise, alternating chill and heat, loss of appetite, weakness, difficulty of swallowing, profuse redness with yellowish-white sharply limited spots in the throat), he now treats every angina as if it might possibly become diphtheria. Most of the patients stated that they remarked an almost immediate effect from the hydrarg. cyan. S. also uses it as prophylacticum, letting persons exposed to the contagion take several teaspoonfuls daily of the solution, and gargle frequently with it. He sometimes uses in addition ice, both internally and externally, and regards stimulants as important, especially as he lost one case suddenly through cardiac paralysis. In a case in which, on account of nausea, the cyanide had to be given up, he had an excellent result from enemata of turpentine and cognac.

4. E. Boman: Strychnine Suppositories in Diphtheritic Paralysis.—A case of light diphtheria was followed by paralysis of the soft palate, with weakness of respiration and paralysis of the legs. B. administered twice daily a suppository of cocoa butter, with 5 milligrams of nitrate of strychnine. On the very next day there was improvement, and further progress toward cure was very rapid. From this case B. recommends the administration of strychnine in suppositories when paralysis of the palate and muscles of deglutition prevent its administration by the mouth.

5. H. Vilandt: Inhalation of Turpentine Vapor.—Twenty to forty drops of a mixture of equal parts of turpentine and carbolic acid are boiled in water in a tea-kettle over a spirit-lamp in such a way that the vapor floats over the patient. V. has seen a great deal of benefit from this treatment, and believes that charging the air of the sick-room

with this vapor will prevent the further spread by contagion of scarlet fever, diphtheria, croup, etc.

6. J. Murray Gibbes: The Vapor of Eucalyptus Globulus in Diphtheria.—Dr. Gibbes believes that the antiseptic method is the only rational method of treatment, not only of wounds, but also of the acute infectious diseases. Confinement in disinfected air seems to him the only means of destroying the seeds of disease already present in the organism and preventing their further extension through the exhalations and emanations of the patient. The author recommends as a convenient and pleasant desinfectant the leaves of *Eucalyptus globulus*. When hot water is poured over these they develop a sufficiently disinfecting atmosphere. In an epidemic of severe diphtheria, Dr. G. used this method, standing the boiling solution of the leaves near the beds, or, where only one child was sick, making a tent over the bed to retain the vapor. He also, in most cases, touched the throat every eight hours with diluted liq. ferri. sesquichlor. and glycerin, and afterward dusted denuded spots with sulphur. Thirty-seven patients thus treated all recovered. The eucalyptus vapor rapidly diminishes the fever, as well as the pain and swelling. G. recommends this principle of treatment by disinfecting vapors for other infectious diseases, such as typhus, pertussis, scarlatina, laryngeal phthisis, and influenza, and suggests that in hospitals arrangements should be made to carry it out more thoroughly.

7. Percy Kidd: Congenital Syphilis of the Larynx (*Lancet*, 1883, No. XVII.).—Dr. Kidd reports two cases of the above:

1. A boy, fifteen years old, in whose family history the only point of interest was that the mother had frequent eruptions on the skin, had suffered for nine months from cough and shortness of breath. Three months before admission a piece of bone came away from the hard palate. For three days before admission the breathing was very short and the voice lost. *On admission*, found a poorly-developed boy, hoarse whispering voice, teeth clearly sickle-shaped, cornea of both eyes normal, no eruptions or scars on the skin, at lung apices slight dulness and bronchial respiration, on hard and soft palate whitish cicatrices with dark-red boundaries. Examination of the larynx showed: The membrane throughout of a dark-red color, vocal cords reddened and thickened, partially covered by the greatly-swollen ary-epiglottic folds and false cords, and not freely movable. The glottis chink was much narrowed, partly on account of the swelling of the membrane and partly from the fixation of the cords. There was rapid improvement under treatment by iodide of potash and inhalation of benzoic acid, and there remained only a slight chronic laryngitis.

2. This was a girl, eighteen years old, with no family history of syphilis. When thirteen years old she began to have pain in the throat and hoarseness. The latter has continued ever since. The outer incisor teeth are somewhat sickle-shaped. Palate, pharynx, and left tonsil show white cicatrices. In the larynx the epiglottis is thickened, its edges irregular, as if gnawed out, its membrane pale, not ulcerated. The vocal cords were united at their anterior extremity by a grayish-red tissue. In the left cord was a small, conical new growth. The posterior part of the right false cord was occupied by a roundish red swelling, which partially

covered the true cord. The true cords were freely movable. The eyes were myopic with slight strabismus divergens. There was some chorio-
dinitis of the right eye; the left was healthy.

8. W. Jones-Morris: Recurring Hematuria (*Brit. Med. Jour.*, No. 1,160).—The author reports the case of a boy, ten years old, in which hematuria occurred at intervals of four weeks, so that it might have been called "recurrent." At the end of September, after getting wet through while playing, the boy, a healthy lad, suddenly had retention (suppression?) of urine. The face was somewhat swollen under the eyes, but there were no other signs of dropsy and no pain over the kidneys. Urine dark and one-quarter volume albumen. About a week later the urine was in normal amount, but became suddenly dark brownish-black, the boy complained of pain in umbilical region, had no tenderness over kidneys, and three days later had an eruption of petechiæ on the lower extremities. About the knees these were almost scarlet in color, and confluent. After this outbreak the urine gradually became normal, the spots disappeared, and only the pain about the umbilicus remained. The mahogany-colored urine during the attack was alkaline, spec. grav. 1.022, and three-quarters volume albumen. Beside granular matter and oxalates, it contained a few granular casts, no blood-corpuscles. This all disappeared, except a trace of albumen.

At the 1st of November the urine again became dark-brown, a second eruption of petechiæ occurred: this was followed by a gradual decrease of the hematin in the urine, and in eight days it was normal. This time the microscope showed a few blood-corpuscles.

In December and January attacks occurred entirely identical with these. The February attack was much slighter, the color of the urine was not nearly so dark, and there was no eruption. The urine became normal in two or three days. This was the last attack, and later examinations showed the urine to be entirely normal.

The author remarks that though the first attack may have been caused by exposure to wet and cold, the following four certainly were not, for the boy was kept constantly in one room and a large part of the time in bed.

9. Hallin: Acute Alcohol Poisoning (*Abst. in Jahrbuch from Scandinavian literature*).—A boy twelve years old drank nearly four ounces of brandy, and was found, after an hour, lying unconscious. He slept quietly till towards morning of the next day, when he began to have convulsive attacks about every ten minutes, and lasting about a minute. The patient was comatose, body somewhat stiffened, extremities freely movable, eyes closed, pupils somewhat contracted, jaws spasmodically, firmly closed, small, regular pulse (160), rapid respiration (60), and a temperature of 39.5°. The attacks began with a crying sound at each expiration, then the patient rolled his eyes upward and to the left, and here they stayed, oscillating slightly, till the attack was over. The convulsions could be brought on by touching the patient. They diminished after an injection of morphine. The stiffness of all the muscles disappeared, except that the trismus continued till death, two days after the poison was taken.

At the autopsy the longitudinal sinus was found filled with partly

clotted, partly fluid blood, the pia mater colored bright red, and adherent to brain; not much increase of fluid in the ventricles; the brain soft and with some points of extravasation: membrane of larynx intensely red; lungs congested; right heart distended and filled with clotted and fluid blood; left heart firmly contracted; stomach empty, its membrane slate-colored and covered with gray mucus. The symptoms and the autopsy pointed to alcohol poisoning. Trismus, tetanus, and opisthotonos are rare from this cause, but cases have been reported of their occurrence in children.

10. Tordeus: Electrolepsis or Chorea Electrica (*Jour. de Médecine de Bruxelles*, 1883).—Chorea electrica, as described by Dubini and the Italian physicians, is found principally in Lombardy. It begins with pain in the head, neck, or lumbar region, followed by twitchings resembling electric shocks. These come first in one finger, one extremity, or one-half the face, and some days later attack the other side of the body. Hand in hand with these twitchings comes paralysis of one or more groups of muscles, then a condition of coma, and finally death. The chorea electrica of the French and German writers commences in the same way, and is characterized by the same shock-like twitchings, but the *sensorium remains free*. The disease lasts in a few days, and ends in cure.

Dr. Eduard Tordeus (Brussels) rejects the name *chorea* for both these forms of disease, and cites cases of his own and of others (Henoch and others) to show that the disease has nothing in common with chorea, and he proposes the name "electrolepsis." I give one of his cases as an example. It occurred in a girl seven years old, well-built, and of healthy parentage.

On February 5th, 1882, on returning from a walk, the child complained of a chill, and was seized with twitchings of all parts of the body. The parents, considering it only a cold, put the child in bed, and gave her hot drinks; but when the attacks were repeated on the following days, they called Dr. Tordeus, who first saw the patient on February 9th. After he had observed the child for some moments without noticing anything abnormal, the girl suddenly began to shake with a violent chill. The shoulders were involuntarily raised and depressed, the hands and forearms flexed, and the arms drawn to the body with sudden movements exactly like those caused by an electric shock. This was repeated every three or four minutes. The girl did not show the slightest sign of pain. The tongue was free and the speech unaffected, the heart's action regular, but with a slight murmur at the apex, the membranes somewhat pale, appetite and bowels regular. Ordered potass. bromid. one gm. p. die. On February 12th, the attacks became less frequent, and on the next day they ceased entirely. On February 16th, the parents took the child home as cured, but they returned again on August 27th. The child, under the use of quinine and iron, had been very well, and had no more attacks up to August 25th. On that day, she was frightened and screamed, and from that moment the attacks began again. They were more frequent and more violent than before; they disturbed the sleep of the child, occurring almost every minute, and involving especially the muscles of the neck, face, and eyelids. Bromide of potash was again given, with the same favorable result, and the

case has not been seen since. The word *electrolepsis*, instead of *chorea electrica*, would surely seem to fit this case.

11. Boeck: In Reply to Eisenschitz, Concerning the Nursing of Syphilitic Children (*Abst. in Jahrbch.*).—Dr. Eisenschitz (Vienna) some time ago published an article in which he claimed that under certain circumstances it was allowable to place a child affected with or suspected of hereditary syphilis at the breast of a healthy nurse. Dr. Cäsar Boeck (excellent authority) takes up his pen in reply to this. He recognizes the importance of the motives described by Eisenschitz (which may be easily understood), but expresses the very decided opinion that it is “never, and under no circumstances,” allowable or justifiable to place at the breast of a healthy nurse a child affected with hereditary syphilis or born of a syphilitic mother, for he has seen cases of infection of the nurse by this means. Of course, the danger of this is less when the child is under strict treatment, and especially its mouth carefully watched and cleansed, but still the danger is there. Moreover, there is not only the danger of infecting the nurse, but of spreading the disease still further through her. Even when a syphilitic or *suspected* child is already at the breast of a healthy nurse, it should be removed, and the exposure of a family secret should not deter us from our duty in such a case. In many other points, Boeck agrees with Eisenschitz. He believes there is no reason for regarding the milk of a syphilitic mother as harmful to the child or for not preferring it to artificial nourishment. He has often seen children do well under such circumstances, even when the mother’s syphilis was recently acquired. It is only when the mother has acquired her syphilis during the last two months of pregnancy, and there is therefore a chance of the child not having become infected by it, that he does not allow the mother to nurse her child, since the latter might thus be first infected by the nursing. A child in whom syphilis is suspected, but no symptoms are present, should therefore be nursed only by its own mother, for it could not be given to a syphilitic nurse. In fact, we shall find frequent cases in which artificial nourishment will offer the best chances, and in such cases B. recommends the condensed milk as that which has given him the best results (a point on which other observers might not agree with him).

In conclusion, Boeck reports four cases of infection of nurses through the nipples from the mouths of children of syphilitic parents.

12. Sandberg: Invagination with Separation of Invaginated Portion of the Intestine and Recovery (*Abst. in Jahrbch.*).—A boy eight years old was taken sick with violent vomiting, distention of the abdomen, and complete obstipation. After nine days a piece of intestine was passed, about twenty-one centimetres long, and with jagged ends. After this, there were copious stools, containing, for some days, blood and pus. Recovery followed the use of ice compresses, oil-emulsion, and morphine. Five months later, there was new obstruction, with very great distention and tenderness of the abdomen, vomiting, and hiccough. The patient was pale and weak, temp. 39°, pulse rapid and thready, percussion sound over abdomen tympanitic. Under the use of ice compresses and restriction of the diet to milk and eggs, and

the administration of morphine, these symptoms disappeared, and free evacuations followed. Since then, the boy has been perfectly well.

13. A. Eckert: Perspiration in Children (*Abst. from Russian literature*).—Dr. Eckert determined the amount of perspiration according to Weyrich's method. She first examined thirty healthy children, and found that the amount of perspiration at different periods was as follows:

Between	2 and	3 years,	4.38 mm.	} 4.05
"	3 "	4 "	4.00 "	
"	4 "	5 "	3.77 "	
"	5 "	6 "	3.39 "	} 3.40
"	6 "	7 "	3.77 "	
"	7 "	8 "	2.77 "	
"	8 "	9 "	3.39 "	
"	9 "	10 "	3.57 "	
"	10 "	11 "	3.88 "	} 3.56
"	11 "	12 "	5.36 "	
"	12 "	13 "	3.21 "	
"	13 "	14 "	3.81 "	

When we consider that Wehrich has determined the average perspiration in an adult to be 3.50, we see that in children it is much greater, and increases as the age and weight decrease.

According to the same method, the amount of perspiration was determined in 27 sick children, and here the results are interesting. There were 10 cases of typhoid, 6 of recurrens, 4 of scarlatina, 3 of measles, and 4 of purulent discharges from bone affections. The experiments led to the general conclusion that in fever the perspiration is increased. In the cases of typhoid, it was found that with very high fever, 39°, there was great increase of the perspiration; with falling or rising temperature, between 38° and 39°, the amount was a little below normal, and that in the first three weeks after the cessation of the fever it was decidedly below normal, the figures for these three periods being 4.18, 3.34, 2.91 (normal, 3.69).

In the recurrens, the perspiration during the attack was increased, reached its maximum with the crisis, and about twenty-four to thirty-six hours after the crisis sank below normal—3.85, 3.95, 2.93.

In scarlet fever, the perspiration was very greatly increased during the exanthematic period, and some increase continued even to the time of desquamation. These results in recurrens and scarlatina differ somewhat from those obtained by other observers, but the authoress seems to have exercised great care in her experiments, and explains where the others failed to obtain correct results.

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ORIGINAL COMMUNICATIONS.

THE SIGNIFICANCE OF METRORRHAGIA RECURRING ABOUT
AND AFTER THE MENOPAUSE.

BY

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METRORRHAGIA, recurring about the menopause, is as likely to be the result of disease in the uterus, or its appendages, as it is at any period previous to that time.

The popular belief that flooding at the change of life is physiological often results in harm, and should be discouraged. But many physicians also believe that profuse hemorrhages are often necessary at the period of the menopause; that the blood-loss is depuratory or critical, and that it protects the vital organs from injurious or even fatal congestion. This, I think, is erroneous; for if it were true, many more cases of metrorrhagia ought to be met with at this period, or more women should suffer and die from cerebral or other internal congestions, as a result of the absence of hemorrhage. But statistics show that the mortality for the five years from forty-five to fifty is no greater than the ordinary increase for each five years of advancing age, and that it is no higher in females than in males for the same period of life; and investigation

proves that in those women in whom the cessation is gradual and without more than the ordinary menstrual flow, better health is enjoyed then and afterwards than where the so-called critical floodings recur.

Where health exists, the cessation of menstruation will be attended by no more aberrations of function than are seen in its establishment. Temperament and idiosyncrasy are modifying factors, but where deviations from the normal standard are marked and persistent, they should be regarded as pathological, and the cause sought for.

Those women who suffer at puberty and at the catamenial periods are almost certain to suffer at the menopause, and the cause is usually found to exist in an imperfectly developed sexual system and a nervous susceptibility. Where puberty and menstruation have been normally established and performed, and where much suffering is experienced at the change of life, the cause will very generally be found in a pathological condition of the uterus or ovaries, the result, probably, of injury at parturition, which may or may not have given rise to symptoms previous to the period of life at which menstruation usually ceases.

An analysis of twenty-two hundred cases that consulted me in hospital and private practice during the last five years, for symptoms referable to the sexual organs, and of which I have notes, corroborates the propositions that I have advanced above, and seems to show that fewer women suffer at the menopausal age than before it, and that the percentage of cases of metrorrhagia is less in those from forty to fifty than in those from thirty to forty years of age.

The character of the symptoms and the cause of the hemorrhage, however, differ somewhat. I confine the analysis to the cases which presented themselves within these five years, because my experience and observation were more fully matured, and my notes more trustworthy in consequence than in the cases treated previous to that time, and especially since the number is sufficiently large.

Of the 2,200 cases,

145 were from 15 to 20 years of age.

393 " 20 " 25 "

443 " 25 " 30 "

364	were from 30 to 35 years of age.
333	“ 35 “ 40 “
223	“ 40 “ 45 “
139	“ 45 “ 50 “
96	“ 50 “ 55 “
33	“ 55 “ 60 “
31	were above 60 “

— — —
Total, 2,200

This table shows that nearly the same number of women sought advice during the five years in which the establishment and decline of menstruation usually occur, one hundred and forty-five for the former period, against one hundred and thirty-nine for the latter; and it further shows that the numbers rapidly increase as the period of greatest fecundity is reached, and decline after it is past. Thus fifteen hundred and thirty-three were from twenty to forty years of age, whilst there were only six hundred and sixty-seven for all other ages.

It is true that there are not so many women living after forty as before that age, but the difference is only slight, compared to the difference in my figures, for during the ten years from thirty to forty there were six hundred and ninety-seven cases, and only three hundred and sixty-two in the succeeding ten years, from forty to fifty, a decrease of nearly one-half. It is also true that this great disparity in the numbers for the two periods may be accounted for in a degree by the fact that suffering at the latter period of life is accepted by many women as unavoidable and proper, and they therefore do not seek advice until compelled to do so by the severity and persistence of the symptoms. However, that will apply to the majority of cases of all ages, for they all alike procrastinate, through feelings of delicacy or from carelessness, until a stage of disease is reached when a cure is difficult, and often impossible.

The percentage of cases of metrorrhagia is likewise shown to be smaller in the decennial period from forty to fifty, than in that from thirty to forty, for six hundred and ninety-seven of the twenty-two hundred belonged to the latter period, and of these one hundred and ninety-nine, or about twenty-eight per cent, suffered from meno- or metrorrhagia, more or

less severe; while of the three hundred and sixty-two cases which belonged to the former period, only seventy-three, or about twenty per cent, suffered from the same symptoms. The smaller percentage for the former period appears more remarkable when we remember that it includes not only the age of cessation, but also that which is correctly recognized as the "cancerous age," for a much larger percentage of uterine cancers occur in the ten years from forty to fifty than in any other decennial period, and these cases are necessarily attended with hemorrhage.

Forty-one of the twenty-two hundred women were affected with malignant disease of the uterus, and were aged as follows: Twenty-six of the forty-one cases occurred within the fifteen years from forty to fifty-five, only three below thirty-five, and five above fifty-five, and the average age for the whole number was about forty-six years. If we take an equal number for each period and reject those who suffered from malignant disease, the relative percentage of cases affected with metrorrhagia about the menopause is shown to be very much smaller than before it.

I believe that it ought to be an axiom in gynecology that flooding at the menopause is never physiological, but always the result of disease. The pathological factor may be difficult to find in some cases, but I think the instances in which it cannot be discovered are so rare that an acceptance of this principle would prevent much suffering and save life; for who can doubt that the constitution is less able to resist the advance of disease after having been subjected to repeated and exhausting hemorrhages, than where such loss has not been met?

I think that we are correct in believing that epithelioma of the cervix may result from injury of that organ, but we must also believe that the local lesion is not of itself sufficient; it merely prepares the ground or supplies the exciting cause; the predisposing cause has a deeper origin. For that we are compelled to go back to some peculiarity in the structure of the tissues of the individual (inherited), which renders them susceptible to an induced dyscrasia. If cancer of the uterus were simply the result of local injury—laceration of the cervix, for instance—there should not be such a vast difference between the number of cases of cancer and of laceration of the cervix.

Emmet, "Prin. and Prac. of Gynecology," p. 451, says, "that 32.80 per cent of all women under observation, who had been impregnated, and had suffered from some form of uterine disease, were found to have laceration of the cervix," and I do not think his estimate much too high if we include all forms and degrees of the injury. But the percentage of cases affected with epithelioma is, according to the same authority, only about two and a half per cent, and this agrees with the statistics of most observers, as well as with that given in this analysis. The same eminent author on p. 496 makes the statement that, "those who suffer from this form of cancer about the time of a change of life are, without exception, from a class who have enjoyed more than the average degree of health," and a little farther on are these words: "I believe that nearly all, if not all cases of epithelioma or cauliflower growth have their exciting cause or origin in a laceration of the cervix." Now, the fact that thirty-two per cent of the fertile women who consult a gynecologist should be found to have a laceration of the cervix and only about two and a half per cent of them should be affected with epithelioma, causes one to doubt, at first glance, the correctness of the prevalent belief concerning the causative relation which laceration sustains to cancer of the cervix. Then, it seems contradictory to say that the cases of epithelioma come, "without exception, from a class who have enjoyed more than the average degree of health," and to follow with the statement that "nearly all, if not all cases have their exciting cause or origin in a laceration of the cervix," for we all believe that where laceration exists the subject suffers, as a rule, in consequence, and therefore could not belong to a class in which, "without exception, more than the average degree of health is enjoyed." It is true that some cases of cancer have apparently possessed unusually good health previous to the development of the disease, but they do not enjoy this immunity *without exception*, nor in the majority of instances. Where these cases are closely questioned regarding symptoms of uterine disease, such symptoms will usually be found. Then, the fact that so many of the women who are found to be suffering with cancer have been for years the subjects of acquired sterility would indicate that they do not really enjoy the

freedom from disease which they are led to imagine themselves from the absence of marked symptoms. Thus, twenty-one of the forty-one cases of cancer in my analysis had not been impregnated within ten years, some of them not in fifteen years, and in several cases more than twenty years had elapsed since the birth of the last child. When a woman, who has been regularly bearing children, suddenly ceases to do so in the midst of the fertile period, there is often some local cause for it, and when this circumstance is so commonly found to precede the development of epithelioma of the cervix, there must be some causative relation between acquired sterility and cancer, as there almost certainly is between congenital sterility and fibroid degeneration of the uterus; and as epithelioma seldom or never develops in a uterus which has not undergone the changes of gestation, or been subjected to the local injury which often attends parturition, we are driven to the conviction that the latter process prepares the ground for the growth of cancer of the uterus. But we must still believe that there is an individual predisposition, either inherited or acquired, back of the local one, else more cancers ought to occur, in proportion to the number of lacerations. However, this cause cannot act unless the tissues have been previously prepared for it by parturition, and the strongest proof of that is found in the immunity which sterile women enjoy, for the same predisposition must exist in many of them.

Practically, therefore, it is safer to regard the disease as of local origin, for we will then endeavor to discover and remove all sources of irritation, and possibly prevent its development or arrest it in its incipency.

That it is sound practice to regard metrorrhagia about the menopause on the same basis as at any other period, viz., as the result of local disease, is shown by the following case, which is typical of its class:

CASE I.—Mrs. Q., residing in a neighboring State, entered one of my private rooms in October, 1883. She was forty-seven years of age, married and had three children, the youngest of which was aged twenty-two years. She had had a miscarriage two years after the birth of the last child, or twenty years ago, but since then she had not been pregnant. In the interval between the occurrence of the miscarriage and the beginning of the present trouble, she suffered occa-

sionally from leucorrhea and slight menorrhagia, with pain in the sacrum. But of this she gave little heed, and considered herself well. About three years ago she began to lose more than the usual amount of blood at her catamenial periods, and the quantity gradually increased with each recurrence, until it amounted at times to a severe flooding. The intervals between the hemorrhages, which were becoming shorter, would be characterized by a watery, fetid discharge. When she became anxious regarding the loss of blood, which she did as soon as she found that her strength was failing, her fears were set at rest by the "wise old ladies" of her circle, who ascribed it all to the "change of life," and advised her to let "nature take her course." And nature did take her course, for as the metrorrhagia and the fetid discharge continued, she became pale and began to lose flesh. Being now thoroughly alarmed, she consulted her physician, an able and conscientious gentleman, but who was unfortunately biased in favor of the theory that flooding at the menopause is physiological. He made an examination, which consisted in the vaginal touch, simply, and found that the cervix uteri was hypertrophied and lacerated, and that the body of the uterus was also somewhat enlarged. However, he informed her that this was not enough to produce the hemorrhage, that it must be the result of the approaching cessation, and advised her not to worry about it. This was nearly a year previous to the date at which she consulted me. She impressed me, when I first saw her, as one in the last stage of malignant disease, and I imagined that I could detect the peculiar odor of cancer, so great were the anemia and cachexia. She was jaundiced and suffered from vertigo and tinnitus aurium, had lost all desire for food, was emaciating and had become so weak that she could scarcely maintain the erect posture without fainting. She was in such constant dread of sudden death that she had become painfully hysterical.

I found, on physical examination, that the cervix uteri was considerably hypertrophied, soft, and lacerated: but the laceration was not a deep one. The os was patulous and dilatable, and the mucous membrane of the cervical canal was congested and abraded. There was nothing about the cervix to indicate epithelioma. The body of the uterus, as outlined by conjoined manipulation, was found to be as large as at the third month of gestation; it was also symmetrical, smooth, and softer than normal. I next attempted to pass my finger into the uterine cavity, but it was arrested at the internal os by a mass of tissue which was of the same consistence as that of the uterus, probably not so firm as the latter, but it was not friable. The sound, when passed to the left of this mass, could be made to enter to a depth of four inches, but when passed to its right it was arrested at a depth of three inches. The manipulation so increased the hemorrhage that it was necessary to tampon the vagina to control it. Of course, I diagnosticated a fibrous polypus, but feared that it might possibly prove to be a malignant growth from the mucous membrane.

On the next day the patient was anesthetized, and I adjusted the noose of a wire *écraseur* around the attachment of the tumor to the uterine wall, and severed a thick, firm pedicle, and then delivered the growth through the os uteri. These manipulations were rendered more difficult than usual because the os was not previously dilated with tents; but as the tissues of the cervix were soft and dilatable, I chose rather the more difficult manipulation than the danger of septicemia, to which a patient in this condition is always more liable from tents. The tumor proved to be a fibrous polypus as large as a hen's egg, and it was a benign growth. Exploration of the uterine cavity with the finger showed it to be free from other disease. Two weeks after the removal of the polypus, the general condition of the patient was so much improved, and the cavity of the uterus so well contracted and free from discharge that I operated for the lacerated cervix, and secured primary union. Her recovery was uninterrupted, and no blood has been lost since the removal of the tumor, except that resulting from the operation for the restoration of the cervix; she has not even menstruated, and I believe that the menopause has been established.

It is very probable that this patient would have died from the hemorrhage produced by a benign disease had the polypus not been discovered and removed. There is another danger to which women suffering from metrorrhagia at this period of life are exposed from the fallacy of regarding the hemorrhage as physiological or critical, to which I wish here to refer. Cancer of the uterus is properly regarded by many excellent physicians as necessarily fatal, and they therefore look upon operative interference as futile, and unwarrantably subjecting the patient to the pain and danger of an effort to eradicate or even palliate the disease. Suppose, now, that this lady had consulted some one who held such views, and that he had concluded from the history, symptoms, and general appearance of the patient that it was a case of cancer—which it resembled very closely—and then, on superficial examination, the soft mass in the cavity of the uterus had been pronounced malignant and non-interference advised. The patient would have been allowed to die from the hemorrhage caused by a benign and easily removable tumor.

CASE II.—On July 9th, 1883, my friend, Dr. D. P. Pancoast, of Camden, requested me to see with him the patient whose history follows: Mrs. S. was forty-two years of age, and a widow. She had had five children, the last one six years ago. Her labors had been unusually difficult. During the last three years, she had suffered from a peculiar sensation in the left iliac region, as

though something were contracted or too short, for the effort of reaching across the table or raising her arms, for instance, would produce a pain or soreness at that point.

About two years ago, she found that she was loosing more blood at the catamenial epochs than usual, and that her strength was failing as a result. No cause could be found for the hemorrhage, and it was attributed to the change of life. The patient was advised to submit to the loss until such change should have been reached. But the hemorrhage rapidly increased until she was rarely free from it, and she was extremely emaciated when Dr. Pancoast was consulted. The doctor found the os uteri widely dilated, and a mass of tissue, polypoid in form, projecting from it; this he suspected to be malignant. My examination confirmed his suspicions, for the growth was of a very friable, vascular character, and in passing my finger into the uterine cavity I found that it originated from many points on the surface of the mucous membrane. The cervix was lacerated, but was not involved in the disease; the body of the uterus was mobile.

I considered the advisability of hysterectomy, as the disease seemed to be confined to the uterus, but concluded that if the procedure is ever justifiable, which is doubtful, it was not so here, for our patient was in such a low condition that she would almost certainly have succumbed to the operation. To check the hemorrhage for a time and rid the patient of the degenerated and decomposing tissue which was rendering her life a burden, we advised an operation for the removal of all that could be scraped away, to be followed by cauterization of the surface, and this was done.

The improvement of the patient was so rapid that within a month she was able to visit friends who lived at a distance. The hemorrhage and other discharges had ceased and her color and weight had been restored to a remarkable degree. But the respite was only temporary, as we had anticipated, for a few months afterwards she died of acute peritonitis, which, I think, resulted from hemorrhage into the peritoneal cavity.

CASE III.—R. X. consulted me in March, 1880. She was then *æt.* forty-two years, married, had two children, and the youngest was ten years of age. Since the birth of the last child her menses had been rather profuse, and she had had some leucorrhœal discharge, with slight inconvenience in the pelvis and pain across the sacrum. Six months previous to the date at which I first saw her, she had an attack of metrorrhagia which lasted two weeks, and this had been repeated frequently within that time; she had not been able to go out of the house, and rarely to leave her room for three months. She had lost more than twenty pounds in weight, had become pale, and suffered from great nervous prostration.

Examination showed the uterus to be slightly retroverted, considerably enlarged, and not freely movable. The cervix was

somewhat hypertrophied, but otherwise was normal. The sound was passed through the internal os with difficulty, on account of some obstruction met with at that point, and indicated the uterine cavity to be large, soft, and rugous; and its withdrawal was followed by a very fetid sero-purulent discharge. I diagnosed fungous hypertrophy of the endometrium, but feared, from the degenerated condition of the tissues, that it might be malignant. I at once decided to dilate the cervical canal and remove the disease as far as possible; and for that purpose four tents were inserted.

When they were removed, twenty-four hours later, the os was so patulous that the index finger could be readily introduced into the uterine cavity, which was found to be festooned with ridges of hypertrophied tissue. This was soft and ulcerating on the surface, but it was firm at its attachment to the uterine wall, not friable. I removed, by means of the polypus forceps and the sharp curette, all of the redundant growth, aggregating enough to fill the palm of my hand, and then thoroughly cauterized the surface with nitric acid.

As soon as the patient had recovered from the immediate effects of the operation, I placed her upon the enforced discipline and diet of the "rest treatment," together with tonic and alterative medicines appropriate to her condition. Perfect cleanliness and an occasional application of equal parts of Churchill's solution of iodine and pure carbolic acid to the uterine cavity, completed the local treatment. Three months after the operation, she had gained fifteen pounds in weight, and was otherwise so much improved that she felt that her health had been entirely restored. There had been no return of the metrorrhagia, and the cavity of the uterus appeared to be free from disease. She went now to spend the summer in the mountains of Pennsylvania, and when she returned in the fall she looked the picture of health, and assured me that she was as well as she had ever been. Careful examination was made at this time, and I failed to detect any signs of a return of the malady.

Four years have now elapsed since the operation, and this lady enjoys good health; there has not been the slightest evidence of a return of the disease, and examination of the uterus confirms the outward appearances, for it seems to be in a normal condition. But the menopause has not yet been established.

According to the opinion of my friend, Dr. H. F. Formad, who kindly examined the specimens removed from Cases II. and III., the microscope showed them to be of like malignancy ("endothelial cancer"), differing only in the stage of the disease.

These cases present the two extremes of the disease, and very forcibly illustrate and strengthen the position which I have taken as to the cause and treatment of metrorrhagia at

this period of life; and they show the value of seeking for and removing the source of the hemorrhage without delay. For if the same decisive plan of treatment had been followed in Case II. as in Case III., when the first signs of the pathological change were manifested, the life of the patient would very probably have been prolonged, and possibly saved, *i. e.*, the disease eradicated; for there is a possibility that this form of disease in this locality (uterine cavity), is not essentially malignant in its incipency, but only becomes so after the health has been undermined by a prolonged drain upon the system, thus destroying the inherent resisting power, or plastic force, of the tissues, and allowing an activity of a lower type to take its place.

This principle will also apply to epithelioma of the cervix, though in a minor degree, for, while it is almost certain that this form of cancer is a necessarily fatal disease from its beginning, it is just as certain that if it could be discovered in its earliest stage and were removed as thoroughly as possible, an untold amount of suffering would be saved, and many years of life added to this most unfortunate class of cases.

But how discover the disease in its incipency? An important question, the solution of which would be potent for good. It can never be hoped for until we come to regard all irregular discharges from the uterus as the result of local disease, requiring immediate intelligent investigation, and to teach women to so regard them. It is true that, in many cases, the disease is so far advanced before the stage of ulceration is reached, upon which the discharges of cancer usually depend, that little, except to palliate the symptoms, can be hoped for; but there are also many exceptions; some in which the disease begins as a superficial ulcer; others in which the growth partakes of the nature of a papilloma in its early stage, and in these cases very much towards prolonging life and alleviating suffering may be accomplished. It is unnecessary to occupy your time in citing cases of early and late operative interference to illustrate and prove this, for every gentleman of experience present will substantiate it. Then, by healing all sources from which these discharges originate, of whatever pathological character, it is possible that the soil, fertile for the development of the affection, may be destroyed, and its growth prevented.

"The change of life is a time of turbulent activity for the reproductive organs," says Tilt; but I believe that this is true only so far as regards tendency or predisposition; that an injury, which may have given rise to only slight inconvenience before, when the organs were actively engaged in the performance of their proper functions, may now be instrumental in developing a lower form of tissue; but this is pathological; it is not an inherent condition.

Why should there be a necessity for the traditional flooding at the menopause? It has no analogy in comparative physiology. It does not relieve symptoms, for it is in those very women who suffer most from irregular hemorrhages at this period that the so-called signs of the approaching change of life are most marked. Apoplexy is very uncommon in the female at this period, and when it does occur, it probably results from degenerative changes in the tissues of the blood-vessels, induced by some form of dyscrasia, and not from plethora. I believe that women suffer less about the menopause since venesection has been abandoned than when it was practised regularly.

CASE IV.—Mrs. W. first consulted me in January, 1878. She was then forty-three years of age, had six children, the youngest being æt. twelve years. She had suffered from menorrhagia since the birth of the last child, and recently from metrorrhagia, which would amount to enough at times to be designated a "flooding." She had also occasional attacks of rectal hemorrhage during the last few years. In addition to the loss of blood, or as a result of it, she suffered intensely from the burnings and flushes, vertigo, palpitation, dyspepsia, and other nervous disturbances to which women at this age are especially liable when the functions of the generative system are not performed properly.

The uterus was found to be large, soft from engorgement, and sharply retroflexed. The cervix was involved in the general congestion and hypertrophy of the uterus, but was not otherwise diseased. The uterus was mobile and not tender on pressure. The sound, when passed to the fundus, indicated a uterine depth of three and one-half inches, and a soft, hypertrophied endometrium. Except in partaking of the general congestion, the pelvic tissues and organs around the uterus appeared to be in a normal state. Examination of the rectum, however, showed the hemorrhoidal vessels to be in a varicose condition.

The plan of treatment followed in this case was one, of course, designed to restore tone to the uterine and pelvic vessels and tissues, to thereby reduce the amount of blood circulating in that locality; and it consisted in the reduction of the retroflexion,

the application of the appropriate remedies to the uterine cavity, and the occasional use of the curette, together with the general medication indicated. It is sufficient for my purpose to state that, as the metrorrhagia diminished and the catamenia became more regular, the nervous and other symptoms subsided; but when the hemorrhage would return, which occurred a number of times during the next two years, at the end of which time the menopause was fully established, these symptoms would likewise return. After the cessation of the catamenia, the patient remained well, until she began again to lose blood from the rectum, when the nervous symptoms returned with such severity as to suggest that the hemorrhage was probably vicarious, or supplementary to the menstrual flow. The vertigo was especially marked, so much so that the patient was in dread of apoplexy, and thought that the hemorrhage was conservative. I, however, regarded the vertigo and many of the other symptoms as the result of anemia, and advised the removal of the hemorrhoids, which were now large. So convinced was the patient that she was in danger of death from apoplexy that she prevailed upon her physician to bleed her from the arm, on several occasions. This had no other effect than to quiet, for a time, her disordered mental condition, and she finally consented to submit to the operation; the hemorrhoids were removed by ligation, about eighteen months ago. She has lost no blood since, and she informed me recently that she had been in better health during the last year than at any time during the preceding ten years. The uterus is in normal condition, undergoing senile atrophy, and the reflex symptoms have disappeared.

I do not think it can be said that the metrorrhagia in this case was at any time physiological, or that the hemorrhage from the rectum was conservative, in protecting the patient from apoplexy or other grave disease.

My experience would teach me that where the menopause is retarded beyond the usual period, the cause can often be found in some disease connected with the sexual system, which interferes with the pelvic circulation; and, as a rule, it is an old standing trouble; sometimes only the remains of a pathological process which was thought to have long since passed away, but which had only been lying dormant, ready to oppose, under a new influence, the natural law which would deprive it of power to act. I refer again to injuries received at parturition which were only partially healed, but more especially to those which had been followed by pelvic inflammation, resulting in uterine and ovarian adhesions by false membranes, and in hypertrophy and contraction of the broad and utero-sacral

ligaments. Such a condition as that almost always retards ovarian and uterine involution, by causing an abnormal amount of blood to flow to those organs in response to the irritant, and by retarding its return through pressure on the veins.

This results in uterine plethora, which is often followed by a development of new-growths from the mucous membrane or in the walls of the uterus, and in irregular discharges of blood from the uterine cavity. The ovaries may at this time, and under the same pathological influence, begin to undergo cystic degeneration. Under the latter circumstances, the metrorrhagia usually ceases, but not necessarily.

In single and sterile women, the menopause is sometimes retarded by the presence of a fibroid tumor in the uterus; but if the tumor be of the hard subperitoneal variety, it may have the opposite effect, and cause the cessation to occur prematurely.

When metrorrhagia recurs after the menopause has been fully established, it is almost invariably the result of a pathological change in the tissues of the uterus. This is sometimes secondary to disease in the ovaries, but, as a rule, the disease begins in the uterus itself. I have never met with an instance of the return of the catamenia after the menopause; yet I am not fully prepared to accept the statement of Dr. T. G. Thomas that "there is absolutely no such thing as a return of the menses when a woman has once reached the normal menopause," for there are some cases recorded by Tilt and others which seem to controvert such a sweeping assertion. However, the authenticated cases are so rare that they may practically be excluded from consideration.

In the majority of cases, the hemorrhage results from carcinoma, but the exceptions are sufficiently numerous to make the following statement from the same author appear likewise too strong: "If these cases could be followed out, it would be found, with scarcely a single exception, that the uterine flow was merely the indication of the presence of malignant disease" (*New York Med. Jour.*). Where the menopause has not been delayed, but has occurred normally, the uterus and its appendages, as well as the pelvic vessels and tissues connected with them, undergo senile involution or atrophy, in which event it is doubtful whether metrorrhagia ever returns.

But this natural process is sometimes retarded by obstruction to the pelvic circulation or by the presence of some irritant which determines an unusual amount of blood to the parts, as referred to above, and in these cases, although menstruation had long since ceased, the uterus does not undergo this senile atrophy, but remains large, and the blood-vessels, being weakened by age, more readily break down and bleed, simply from passive congestion, maintaining a sort of periodicity from the old habit.

This abnormal blood supply is apt to result in hypertrophy of the mucous membrane and the development of polypi, as in the following case, with the relation of which I will conclude.

CASE V.—Mrs. K. consulted me in January, 1880. She was then seventy-two years of age, and had been a widow thirty-seven years. She had had six children, the last one thirty-eight years previously. After more than the usual irregularity in the menstrual flow, the menopause finally occurred at the age of fifty-two. During the next five years there was no discharge whatever from the uterus, but at the end of this time, or when she was about fifty-seven, she experienced great grief in the departure of a favorite son for the West, and her menses, as she thought, returned in consequence. From this time she continued to lose blood at intervals which were rather regular, and she accepted the event as an evidence of rejuvenescence. The bleeding was at no time profuse, but it ultimately began to affect her health, and she sought advice fifteen years after the first appearance of the hemorrhage.

On touching the vagina, I found presenting at its orifice a soft mass of tissue, which extended up to the os uteri; it was large enough to entirely fill the vagina; it looked and felt much like a bunch of grapes, and resulted from a follicular degeneration and hypertrophy of the mucous membrane lining the cervical canal. It was probably four inches in length, and half as broad as my hand. After the polypus had been removed, the cervix was found to be large, soft, and the seat of an old laceration. The veins on the surface of the mucous membrane were dilated, and the uterine canal was patulous throughout, and measured three and a half inches in depth. Under the influence of local and general medication, the uterus contracted, and within a few months after the removal of the growth, it was much reduced in size, and seemed to be free from disease. The patient is still living and the metrorrhagia has not returned.

The fact that the hemorrhage began in this case simultaneously with the grief caused by the departure of her son on a

long journey is of interest from a psychological stand-point, but I think it was a coincidence. Possibly it may have had an influence on the generative organs, causing a hyperemia which, being kept up, may have resulted in the development of the polypus from the position of the laceration, but I prefer to take the more practical view, and ascribe it to a local cause.

2004 CHESTNUT STREET.

IGNIPUNCTURE OF THE CERVIX UTERI.

BY

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LESS than a decade has elapsed since the hot iron and galvano-cautery played a prominent part in gynecology, and especially, in surgical parlance, in "minor gynecology;" to-day these instruments are in danger of being consigned to the scrap heap.

Wherever their employment was possible and seemed likely to promise success, the knife, the scissors, and the suture have rightly taken the place of the incandescent instruments as the most suitable wound-producers and wound-closers. And who, from a surgical standpoint, would deny that the deeper chronic forms of cervical catarrh, ulcerating surfaces with and without cicatricial ectropion, and hypertrophies of the vaginal portion can be better treated and more smoothly healed with knife and scissors than by the application of incandescent instruments?

Nor would it avail to object that neither Emmet's operation nor Schroeder's excision of the mucosa has proven free from relapses; for the same is true of the cautery, and the method followed by prompter recovery would still be preferable.

However, the subject appears more questionable in reference to the wedge-shaped excision and amputation of the vaginal portion. What gynecologist who criticises himself, and who is able to watch his results for one or two years, has not experienced numerous failures in this direction; who has not asked himself, in the case of low descensus of the cervical stump, of

retroversions of the uterus deprived of its vaginal leverage, of dysmenorrhea from cicatricial contraction, etc., the question whether it would not be possible to attain the end in view—reduction of the swollen hypertrophied cervical portion of the uterus with the best possible preservation of its form—by less energetic local procedures, if more slowly, yet more certainly?

Moreover, neither the gravity of the affection nor especially the means of the patients and the time they are able to devote to their recovery bear a correct relation to the demands made upon them by the wedge-shaped excision and the amputation of the cervix.

About two and a half years ago, I conceived the idea of instituting the following mode of dispensary treatment in women whose social position or means did not permit prolonged confinement to bed or stay in the hospital, but who were suffering severely from the consequences of cervical hypertrophy.

At intervals of from ten to fourteen days, that is, two or three times between every two periods, I made generally four ignipunctures, two into the anterior, two into the posterior part of the vaginal portion with a pointed galvano-cautery or Paquelin; the former being more appropriate to lesser, the latter to greater depth of the puncture required. The direction of the punctures is centrifugal, extending from the investing vaginal mucosa through the stroma of the cervix to its mucosa; or the reverse, centripetal, according to the individuality of the case; the former, in general, more for indurated hypertrophy without profuse catarrhs or ulcerations; the latter for the opposite conditions.

Before and after the puncture, disinfectant irrigation; the tampon remaining till evening; one-half hour's rest; then the patient is allowed to attend to her ordinary occupation.

The depth of the punctures is from one to one and a half or two centimetres; the total number of the several operations no more than five or six; hence altogether about twenty punctures in the course of three months. The healing of the several granulating depressions consumes between two and three weeks. In this connection, it is of the greatest physiological interest that these granulations, according to several hours' observation in three patients, participate in the menstrual hemorrhage.

The result of this procedure, which succeeded far beyond my anticipations, has induced me to abstain from the wedge-shaped excision and amputation also in the better class of patients wherever possible, but especially in cases of hypertrophy confined to the vaginal portion proper.

We are forced to conclude that by this method, just as in the case of angiomata and nevi, blood and lymph channels are destroyed in the depth, as well as gland tubes and follicles, to be followed by atrophy of connective tissue and muscle.

The favorable after-effect is not to be expected immediately ; on the contrary, under the acute irritation of the puncture, the vaginal portion often swells still more, but only temporarily, for a few days ; however, after the conclusion of the process we can certainly anticipate a very permanent reduction which continues measurably for several months.

What induced me mainly to extend the method even beyond the polyclinic was the certainty of having no relapses ; of forty patients I have not had a single one ; in fully one-half of the cases, more than a year has elapsed since the conclusion of the treatment. No disadvantages came under observation ; the menses at the time of the puncture were occasionally rather profuse and prolonged, but always regular afterwards ; endometritis was treated incidentally, either generally or locally, according to its nature.

The instrumental outfit is so simple that it requires no special illustration ; a couple of not too thin wire points suitable for the galvano-cautery, a couple of variously curved, not too short, pointed attachments for the Pacquelin—that is all.

I particularly recommend this procedure for the polyclinical treatment of women who have neither time nor money for operations, and as a minor attempt at a more conservative gynecology.

FIFTEEN CASES OF SIMULTANEOUS CLOSURE OF THE
LACERATED CERVIX AND PERINEUM.

BY

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THE following cases of simultaneous closure of the cervix and perineum in the same patient are reported, simply on account of the uniform success attending them, and the belief that the performing of both operations at one time is a great saving of time and suffering for the patient, in all cases demanding the two operations.

CASE I.—Mrs. A. M. T., widow, the mother of two children, had suffered, for several years, from the usual effects of lacerated cervix and perineum, chronic hyperplasia of the uterus, erosion and eversion of the cervix, and consequent menorrhagia and considerable prolapsus. She was under my care for several weeks, prior to her admission to the Woman's Hospital. Was admitted in February, 1876, and the operations were performed on March 1st, 1876. The closure of the cervix having been first accomplished by the method originally recommended by Dr. T. A. Emmet, the operation upon the perineum was immediately done. Three sutures were used on one side, and four on the other side of the os, in closing the cervix.

Seven sutures were used in closing the perineum. The necessity for so large a number of sutures in the cervix was accounted for by its extensive laceration.

Previous to my seeing this case, it had been pronounced epithelioma by several physicians, and the propriety of amputating the cervix had been discussed.

The sutures were removed from the perineum on the seventh day after the operation; those from the cervix, on the twenty-first day. Both operations were perfectly successful, and the patient was, soon after, discharged cured, to resume her vocation—that of a nurse.

CASE II.—Mrs. R. B. H. (city). Operations performed in November, 1878. This case was complicated by a long-continued retroflexion of the uterus. Previous local treatment, and the application of a proper pessary to keep the uterus in normal position, had been made use of for several weeks prior to the operation. The results in both operations were successful. The sutures were removed on the days corresponding to those mentioned in case I., and she has continued perfectly well since.

CASE III.—Mrs. R. L. M., Brooklyn. Operations performed April, 1879. This case had also suffered, for a long time, from

retroflexion and prolapsus. The sutures were applied as previously described, and removed on corresponding dates, and at the present date she is perfectly well.

CASE IV.—Mrs. J. W. H. (Florida). In this case, a previous operation had been performed upon the perineum. It, however, had been unsuccessful. The laceration of the cervix had been overlooked. I performed the two operations in October, 1879. The success was perfect, and at the end of five weeks from date of operations, she returned to her home.

CASE V.—Mrs. G. A. A. (city). In this instance, a previous operation had been performed upon the perineum by another surgeon. A subsequent confinement had produced a second laceration of that part. I performed the two operations in March, 1881. Considerable preliminary treatment, to reduce the enlarged uterine body and cure the existing endometritis, was employed. The sutures were introduced in same manner and removed on corresponding days, and the result, in both cervix and perineum, was perfect.

CASE VI.—Mrs. L. H. J. (city). A long-continued hyperplasia of the body, with chronic endometritis and retroversion, had existed. The usual treatment for the relief of these conditions, such as the hot-water douches, iodine, glycerin, and the wearing of a proper pessary, had been carried on for some time, and, in March, 1881, I operated upon the cervix and perineum.

The sutures were removed, as in all the preceding cases, from the perineum on the seventh, and from the cervix on the twenty-first day, and the results were perfectly satisfactory.

CASE VII.—Mrs. H. W. (city). Similar, in all respects, to the preceding. The two operations were performed May 5th, 1881, and the results, when sutures were removed, were perfect.

CASE VIII.—Mrs. J. B. P. (city). Similar, in all respects, to the preceding. Operations performed in June, 1881, and the results, when sutures were removed, were perfectly satisfactory.

CASE IX.—Mrs. H. De F. Y. (city). In this patient, the laceration of the cervix was unusually extensive, and the cicatricial tissue removed was much above the average. The two operations were performed in November, 1881, and the result in both operations excellent.

CASE X.—Mrs. J. B. B. (city). The two operations were performed May 17th, 1882. On the 24th, I administered ether and, after removing the sutures from the perineum, extirpated a sarcoma, of about the size of a hen's egg, from the left breast, thus performing three quite serious operations, within a space of seven days, upon the same subject. The result in all of these operations was perfectly satisfactory, and the patient has been in excellent health ever since.

CASE XI.—Mrs. M. M. (city). The usual conditions indicating the necessity for the two operations existed in a marked degree. Both operations were performed October 11th, 1882, the sutures removed from perineum on the 18th, and those from the cervix two weeks later, as in the other cases. Results perfect.

CASE XII.—Mrs. B. L. (Pennsylvania). A complete laceration of the cervix, and an extremely severe and irregular laceration of the perineum, the tear extending back to a point a half-inch beyond the rectum, going around the rectum upon the left side. A large amount of the tissues had sloughed from the injuries incurred at confinement, and a rough and gaping condition of the parts existed.

After some preliminary treatment, to restore the uterus to its normal size, and to prepare the parts as thoroughly as possible for the operations, both the cervix and the perineum were closed, on the 30th of October, 1883. Some little difficulty was experienced in properly adapting the perineal surfaces, owing to the loss of tissue; still, it was overcome by passing the two posterior stitches somewhat obliquely, and *lifting* the left perineal surface slightly forward and drawing it up to the other surface.

This operation was performed at St. Elizabeth's Hospital. The result was eminently satisfactory, and at the end of the three weeks, when the sutures were removed from the cervix, all of the parts were in a perfectly normal condition. This patient writes me that, since her return home, she is in perfect health and enjoying life, to the same extent that she did prior to the labor which caused her such suffering and so long continued a state of invalidism.

CASE XIII.—Mrs. S. H. S. (city) came under my care early in November, 1883. She had suffered for several years from lacerated cervix and perineum, retroflexion of the uterus, prolapsus of the left ovary, and all the symptoms usually accompanying such a condition. Was very anemic and debilitated. November 14th, I replaced the uterus and ovary—patient in the knee-chest position—and applied one of my rubber-covered copper ring pessaries, moulded in such shape as to keep the uterus and ovary in place. The patient was then placed in bed, and the usual treatment for reducing the enlargement of the uterus and preparing her for the operations was begun, and faithfully carried out by her attending physician, Dr. Thomas H. Skinner, until December 22d, when I operated upon both the cervix and perineum.

In the cervix, six sutures were required; in the perineum, four. On the seventh day after, I removed the sutures from the perineum, and on the twentieth those from the cervix. The result in both operations was perfect. On examining for the ovary, I found that I could not reach it with my finger, and I believe it will remain in its proper position.

CASE XIV.—Mrs. W. G. Z. (city), forty-two years of age, has had menorrhagia, excessive at times, for some ten years. Laceration of cervix and perineum occurred about twenty years since, and during the greater part of the time since then she has been under the care of physicians for the retroflexion of the uterus, the granular erosion of the cervix, and the menorrhagia.

During November and December, 1883, she was regularly under my care, and after the usual preliminary treatment I operated upon both cervix and perineum on December 29th.

The patient's general condition was far from good, she being anemic and greatly emaciated. In paring the cervix I found the tissues so thoroughly hardened and hypertrophied that it became necessary to cut away the cervix to such an extent, in order to reach healthy tissue, as to remove at least a half-inch in thickness all around the os, and even to the limit of the vaginal juncture on either side. In closing the surfaces it was necessary to insert a small tent of carbolized cloth through the point where the external os should be, and up through the internal os, in order to prevent union of the entire surface. This being done, a silk thread was attached to the lower end of the tent, which projected through the vulva and enabled me to remove it on the third day after the operation without disturbing the sutures.

The perineum was then closed. The sutures were removed from the perineum on the seventh day, and those from the cervix on the twentieth. Union of both was perfectly satisfactory, the cervical canal being perfectly free.

CASE XV.—Mrs. T. H. S. (city), laceration of cervix and perineum, the result of instrumental delivery some six years since.

Shortly after delivery—about three months—the cervix was operated upon by a distinguished gynecological surgeon of this city. The result was not satisfactory, only a partial union having taken place. The failure was attributed to excessive vomiting caused by irritation of the sutures.

About the middle of November, 1883, the patient came under my care.

I found her suffering from the usual symptoms attending such cases, retroversion, procidentia, chronic hyperplasia of the uterus, and endometritis. She assured me, as did also her husband, who is a physician, that it had been impossible to keep the uterus up by any pessary which had been tried, they invariably coming away within a few hours after their introduction; and that the attempt had been given up by the gynecologist under whose care she had been.

I moulded one of my rings, described in Case XIII., so as to resemble very closely the Albert Smith retroversion pessary, simply making it considerably broader; and after the second attempt succeeded in keeping the uterus in good position, and with perfect comfort to the patient. Treatment appropriate to the reduction of the size of the uterus, and the cure of the endometritis previously mentioned, was begun and kept up with great regularity by her husband, and on the 31st December, 1883, I operated upon both cervix and perineum.

From the excessive induration and hypertrophy of the cervix I found it necessary to follow the same steps in operating upon it as I have detailed in Case No. XIV., inserting a tent as in that case.

Tent removed on third day. Sutures from perineum on the seventh. Considerable nausea and vomiting occurred during the four days following the operations.

On the *eighteenth* day subsequent to the operations I removed the sutures from the cervix. The union in both the cervix and perineum was perfect, the cervical canal being normal in size and perfectly free.

In all these fifteen cases the sutures, as has been stated, were removed from the perineum on the seventh day, and from the cervix on the twenty-first, twentieth, and eighteenth days after the operations. The bowels in each had been kept quiet until the sixth day after the operations, when they were moved by a cathartic assisted by an enema, thus giving the patient a rest of twenty-four hours before the perineal sutures were removed.

Warm carbolyzed vaginal douches were used morning and evening, beginning the day after the operations, and continuing until the sutures were removed from the cervix.

In some of the cases it was necessary to administer opiates, either hypodermically or by suppositories in the rectum, during the week succeeding the operations, the majority requiring this relief. In several no opiate was given. The first and the last four of this series of cases were operated upon in hospitals, the remaining ten at their residences. The result in every case, both as to closure of the cervix and perineum, was perfectly satisfactory.

In no case was there any dangerous symptom during convalescence. Neither was there any inconvenience or suffering on the part of the patient from the presence of the sutures in the cervix during the three weeks which elapsed before they were removed. In all the cases I found the perineum sufficiently firm, even on the eighteenth day in the last case, to allow the necessary manipulation for the removal of the sutures from the cervix.

Of the fifteen cases, three have borne children since the operation (Cases No. II., VI., and IX.), and in two of these (Nos. II. and IX.), no laceration either of cervix or perineum has occurred. In the other (No. VI.), a partial laceration of the cervix took place. It was very slight, and with little treatment contracted so well as not to demand closure by sutures.

My experience having been so satisfactory in these cases, and never yet having met with a case in my own practice in which the result has not been satisfactory, both as regards the cervix and perineum, I am a strong advocate of the double

operation (when both are required) in all such cases, unless some peculiar conditions obtain which might, in some way, render the performing of the two operations at one time objectionable.

I have rarely met with such a condition as to preclude the double operation.

As a rule, an hour or an hour and a half suffices for both operations, and etherization, as we all know, is easily borne during that time.

OCCLUSION OF THE OS UTERI DURING PREGNANCY.

BY
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CASE I.—Mrs. —, twenty-four years of age, was taken with pains recurring at regular intervals, on May 21st, 1883. This was her first pregnancy, and when I saw her, about ten o'clock in the morning, she had been in pain several hours. On making an examination, I found it difficult to locate the cervix uteri, and it was only after a protracted search that a small *pimple* was found, which I felt justified in assuming to be the cervix. I could discover no os uteri. The lower segment of the uterus was thin, and the head of the fetus could be readily felt through the uterine tissue. When a pain came on there was no opening apparent in the part which I have described as the probable cervix. Throughout the day the same condition of affairs remained, but on paying a visit after night I discovered the smallest possible depression in the "pimple." Hoping that nature would be able to point in the direction where interference should be had, I determined to wait until the pain had become more expulsive in character, and left, with directions to be sent for when the nurse deemed it necessary for me to attend. On the morning of the 22d, I learned that during the night the water had commenced to dribble away, and that the pains had continued as before. On examination, there was no appreciable change in the cervix, but the vagina was moist from the flow of water. A small probe could not have been passed through the os uteri without the exercise of force. During the day the pains increased in strength, and by four o'clock in the afternoon I was enabled to pass my finger into the cervix and make out the presentation. Dilatation proceeded very slowly, notwithstanding the actively expulsive contractions. At nine o'clock at night the os was about two and

a half inches in diameter, and had been so for some time. There did not seem to be any great rigidity about the os, but the pains seemed to accomplish but little in advancing the head. Watching the case for a while, and the patient becoming more exhausted, it seemed proper to hasten delivery by instrumental means, and, after administering ether, I applied the forceps, the head being at the superior strait, and succeeded in delivering the woman of a male child of ordinary size, after about fifteen minutes' work.

Owing to inefficient nursing, and general disregard of instructions, this woman had a protracted convalescence. A form of intermittent fever came on, without chill, and on alternate days the temperature would range from 106° in the morning to 103.5° in the evening; the pulse from 160 in the morning to 120 in the evening; while the respirations were as many as sixty per minute. On other days the temperature, pulse, and respirations were normal. Some pain and considerable tympanites were found to be present on palpating the abdomen, but local symptoms were altogether too slight to account for the range of symptoms on her *sick* days.

In this case, nature was able to overcome the obstruction to the extent of opening the os uteri, but the uterus seemed to be too much exhausted by the effort to complete its work of extruding the child. There was no disparity between the size of the fetal head and the pelvic diameters, and but for the occlusion of the os, labor would, doubtless, have terminated in the usual manner, without instrumental aid.

No cause could be assigned for the obstruction. The patient had been married less than a year, and had never been treated for uterine disease, nor had such been suspected to exist before the onset of labor.

CASE II.—June 7th, 1883, Mrs. S., age twenty, primipara, was taken in labor early in the morning. I saw her about eight o'clock A.M. She was then having pains which recurred at intervals of ten minutes. On proceeding to make an examination, I found the vagina moist, and the entire pelvic cavity easily accessible to the finger, but nowhere was there the slightest evidence of a cervix or os uteri. I cannot recall a case in which the cavity of the pelvis was so easily reached. At first I thought the case to be one in which the os uteri was high up posteriorly, but this expectation was soon disappointed. The examination was made very leisurely, and fully fifteen minutes occupied in the endeavor to learn the exact state of the parts. The lower segment of the uterus was so thin that the exact position of the presenting part (the head) was determined. The dimensions of the pelvis were ample.

As the symptoms were not urgent, I concluded to take time to think about the matter, and the patient was not seen for two hours. At this time a ring of hard tissue was found, having a diameter of about one inch and a half, which was filled in by a tissue through which the fetal head was readily felt; but no sign of an opening was to be discovered. I tried to break through this with my finger nail, as it was now manifest that the os uteri was occluded, and that the ring of tissue referred to was the opening cervix. In this attempt I was balked by the strength of the membrane. I next attempted to force a pocket-case probe through, but again my efforts were frustrated. Leaving the patient long enough to go to my office and secure instruments, I returned to find that pains were becoming more severe and recurring at shorter intervals, but no change had occurred in the condition of the soft parts. A bivalve speculum was then introduced and opened to its widest extent, but no os uteri could be seen. Withdrawing the speculum, I took my smallest probe and passed it along the finger until the membrane covering the *ring* was reached. Many efforts were made to pierce the membrane, and at last I succeeded, by pressing the probe in an oblique direction, in penetrating the obstruction. This was known to have occurred by feeling the probe moving freely within the uterine cavity. It will be remembered that the head was presenting and stretching the occluding tissue, and this made it necessary for me to be careful in using force, as there was danger of penetrating the child's head with the probe if too great an effort should suddenly overcome the resisting membrane. By exerting a little force the opening made by the probe was somewhat enlarged; a Sims' sound was then made to take the place of the probe. Again stretching the part with the sound, an œsophageal forceps was substituted, and by forcibly opening the blades the obstructing tissue was felt to tear. Withdrawing the forceps, the end of the finger was passed to the opening and, after some effort, made to enter. Using the finger as a dilator, I soon found that the uterine contractions were forcing the head down, and therefore concluded to leave the work to the natural forces. Visiting the patient at short intervals, the dilatation was found to be progressing slowly but satisfactorily. Several hours passed, the pain becoming violently expulsive, the os dilating to about two and a half inches, and then further progress ceased. When the contractions came on, the patient seemed to be unable to utilize them, and her agony was greater than I have ever witnessed in labor. She would spring up in bed and declare that she was bursting. The uterus assumed a position at a right angle to the axis of the body when pain came on. Never have I seen a patient whose symptoms so nearly indicated the approach of rupture of the uterus as this young woman's did. I endeavored to mitigate her suffering by administering ether, but the article was so poor that no relief was afforded. A better article having been secured, Dr. Caldwell came in, in response to my request, and administered the anæsthetic, after which I applied the forceps, the head being at the

superior strait and the os dilatable, and in a few minutes was able to terminate the labor. No injury was done to the soft parts, and the patient made an excellent recovery.

As in the former case, the removal of the obstruction did not seem to afford the relief which was to have been expected. The uterine efforts were frustrated, and, in my opinion, rupture of the uterus would have occurred if relief had not been afforded by forceps.

This patient had always suffered from dysmenorrhea before marriage, but beyond this had never required treatment for disease of the uterus.

These two cases are all of the kind that have occurred in my practice. The symptoms have been given with too much detail, perhaps, but as others may meet with similar cases, the recital of symptoms and the treatment pursued may be of advantage.

It is true that the obstruction in both of these cases was of a character that was, fortunately, easy to overcome; and yet, in both it was sufficient to render the efforts of the uterus futile, and call for the interposition of the physician in the application of the forceps.

Before discussing the subject of occlusion of the os uteri and its treatment, it is well to call attention to a few facts relating to the patients whose cases have been reported above.

Both patients were primiparæ. Neither had shown symptoms, before labor commenced, which called attention to the uterus as an organ requiring treatment. Both were strong, healthy white women. The cases also proved the advantages of good nursing. In my first case, the attendants were ignorant, opinionated, and beyond control, neglecting to carry out orders and admitting to the sick-room sometimes as many as a dozen people at once. In the other case, the nurse understood her business, co-operated with the doctor in the management of the case, and carried out his instructions. The patient was benefited, and the medical attendant relieved of much anxiety.

In this paper, it is my purpose to confine my remarks to the subject of occlusion of the os uteri during pregnancy, and omit, as far as possible, all reference to the subject of atresia of the genital canal which may be found under other circumstances.

Inasmuch as the subject discussed in this paper is generally

referred to briefly by authorities, I have only quoted those whose remarks would best elucidate the points considered to be worthy of attention.

Varieties of Occlusion of the Os Uteri.—Occlusion of the os uteri presents itself in three principal forms: 1st, agglutination or sticking together of the margins of the os; 2d, the presence of an adventitious membrane; 3d, closure of the os by cicatricial tissue. The first and second forms are more frequent than the third.

The obstruction may be confined to the external os, or the entire cervical canal may be affected; but the external os seems to be the seat of the occlusion in a number of cases out of all proportion to those located in other parts of the cervix. This statement is borne out by the almost unanimous opinion of observers, and, hence, it is hard to understand upon what ground Klob (*Path. Anat. of the Female Sexual Organs*, 1868, p. 110) can justify his statement that "partial contractions and occlusions of the uterine cavity take place at its orifices, and, as regards frequency, those at the internal orifice are more frequently observed than those of the external." And, again, he says (p. 111): "A purely mechanical stricture rarely affects the external orifice." These statements are cited merely that they may be discredited. In speaking of cicatricial atresia, Lusk says (*Midwifery*, p. 505): "Cicatricial atresia of the os externum is rarer than the adhesive stenosis just described. It is usually confined to the lips of the external os, but may involve the cervical canal for a varying distance."

Etiology.—Closure of the os uteri due to atrophy following cessation of the menses has been observed by some authorities. Tilt says: "Virchow has noted the closing of the internal os uteri in old age, and I have found an obliteration of the os uteri in nine women whom I had occasion to examine, from ten to fifteen years after cessation." While this quotation is not exactly pertinent to the subject intended to be discussed in this paper, it is worth remembering when examining into the causes which produce occlusion generally.

Many authorities look upon inflammation as the only cause of occlusion; and, while this may be true, it is difficult to explain why so serious a result should follow a cause so slight, in many instances, as to present no symptoms to attract attention

to the part diseased. Tyler Smith wrote: "Entire occlusion of the os uteri is difficult of comprehension. The causes of this extraordinary state of things are, it must be confessed, involved in obscurity." And again: "In the most perfect cases of occluded os, it is found, moreover, that no signs of inflammation have existed during pregnancy." Klob says (p. 115): "Kiwisch thought that atresia of the gravid uterus might possibly occur in consequence of the formation of a kind of decidua in the cervical portion, representing an agglutinating intermediate layer, which afterwards, when the canal is distended, is increased into a membrane of varying density." I do not recollect to have met with this explanation from any other writer.

Concerning agglutination of the os, a few quotations will show the drift of opinion in relation thereto. Lusk writes (p. 504): "Adhesion of the lips of the os externum is occasioned by the superficial union of the opposing mucous surfaces through the medium of inspissated epithelium, or of new connective tissue resulting from adhesive inflammation produced by vaginitis or cervical endometritis." In Chailly's *Midwifery*, translated by Bedford, we find the opinion expressed, in relation to agglutination of the os, that it "seems to be caused by the inflammation of the surfaces in contact, which are frequently united by a more or less resisting pseudo-membrane or fibrous tissue." Cazeaux believes the agglutination to be the result of inflammation of the cervix and upper part of the vagina, and the agglutinating material analogous to the bands formed in pleuritis. Meadows (*Manual of Midwifery*, p. 310), among other causes of unnatural labor from abnormal condition of the passages, gives "agglutination of the edges of the os, apparently from previous inflammation." Glisan says (*Midwifery*, p. 365): "This condition is generally the result of inflammation of the cervix during the early months of gestation."

With regard to cicatricial atresia, all that it is necessary to say concerning it is summed up by Lusk, who declares (p. 505): "Its most frequent causes are *post-partum* ulceration, inflammation, cauterization of the cervix, and mechanical irritation applied for the purpose of producing abortion. The diminution of the uterine discharges during pregnancy affords a

favorable opportunity for the development of the stenosis under consideration."

Frequency of the Affection.—While many writers have described the affection under consideration, all admit the infrequency of its occurrence in a form calling for operative measures to overcome it. Denman practically denies that occlusion ever exists during pregnancy, believing that, in cases where incisions were made to remove the obstruction, the apparent absence of the os uteri was due to unrecognized obliquity of the uterus. On the contrary, many express the opinion that occlusion by agglutination of the lips of the os is more frequent than generally admitted, and claim that the obstruction is overcome by natural efforts before the physician is summoned. Thus, Chailly says, it "is much more frequent than is generally supposed. If it be often unnoticed, it is because nature most usually overcomes it." Cazeaux makes the same statement. But the fact remains that recognized cases of occlusion of the os uteri during gestation are rarely met with.

It is worth while to state here that, according to Lusk, atresias of the uterus "occur less frequently than those of any other portion of the genital passages."

Strength of the Obstruction.—When we remember how often labor is protracted and uterine contractions rendered nugatory, because the membranes fail to yield when the os uteri has dilated to its full capacity, and how easy it is to terminate the patient's sufferings by rupturing the "bag of waters," it is not to be wondered at that labor should be in vain when the uterus is closed by an obtruding tissue. Nor is it necessary that such tissue should be very thick or strong; for familiar examples in physics might be cited to prove the disadvantage at which the uterus is placed in its efforts to relieve itself of its contents. It is not to be expected that nature will be able to overcome any but the mildest forms of occlusion. Tyler Smith has written of occlusion: "This may exist to such an extent that labor becomes impossible, unless rupture of the cervix takes place, or artificial assistance can be rendered. In cases of this kind it has been known for the whole of the lower segment of the uterus to be separated in the form of a muscular ring, and detached from the rest of the uterus by the strength of the pains acting against an occluded os. Other patients have died

undelivered where rupture has not occurred and where no operation has been performed." And Cazeaux records the following: "In a case where a woman died during labor, the adhesion of the neck was found, at the *post-mortem* examination, to be so resistant that it could neither be lacerated nor broken by any moderate force, and the membrane that blocked it up was of an aponeurotic character."

Recurrence.—In some women, the parts seem to acquire this abnormal condition with succeeding pregnancies. Playfair has known it to recur in the same woman in two successive pregnancies, and Tyler Smith says: "Dr. Ashwell relates a case in which incision was required in four successive labors, in the same woman, attended by different practitioners."

Symptoms.—Little need be said in relation to the symptoms. Still it may be proper to call attention to a few points under this heading, and reserve further consideration until the question of diagnosis is discussed.

Before the obstruction is removed, there is no "show" in these cases, and the vagina is apt to be comparatively dry. The presenting part may descend low in the pelvis. The character of the pains is such as to cause the patient to suspect some impending evil. She seems to realize that the pains are wasted, and friends too often will remark that labor is pursuing an irregular or unusual course. After overcoming the occluding tissue, the pain seems still to be ineffectual. The uterus, in some cases, acts as if it were working at a disadvantage, and the pain is of a severe and agonizing character altogether out of proportion to the work accomplished. In other cases, when the cervix is opened, the uterus appears to be exhausted. The latter condition seemed to be the case with my first patient. Reference has already been made to the character of the pain in my second patient.

Diagnosis.—It would seem to be a very simple matter to diagnose correctly the affection under consideration, but the warnings laid down by authorities would lead us to believe that such is not always the case. We are warned not to mistake this condition, when the uterine walls are thin, for a fully dilated cervix with the membranes closely covering the head of the child, lest the error be committed of forcing the finger through the uterine tissue, and thus make the situation favor-

able for extensive rupture of the uterus. Chailly says: "This distended part frequently becomes so thin that we imagine that only the membranes of the ovum are interposed between the finger and the head." Another point upon which the greatest emphasis is laid, is not to overlook the probable obliquity of the uterus, or overlapping of the lips of the cervix. A few brief quotations will place this matter in a clear light. Cazeaux, after admitting that occlusion does occur, continues: "But it is an exceedingly rare occurrence, and the accoucheur must not permit himself to be deceived by a well-marked obliquity of the cervix, which would render the orifice of difficult access, nor by an agglutination of the lips of the os tinæ, since it is possible for an overlapping of the two latter to be mistaken for an absolute obliteration of the orifice. 'Several times,' says Dugès, 'we have found the anterior lip covered and embraced by the posterior one, which thus masked the opening, so that the finger could only penetrate it in a very oblique direction, though when effected, this introduction furnished a means of rectifying the error promptly, and of reducing the parts to a more favorable state.'" In order to make a clear diagnosis, Glisan says it may be necessary to give ether and introduce the hand into the vagina to search for the os, which may be high up behind. He adds that the use of the speculum is generally unavailable, and this statement is sustained by my experience in the second patient. Others, however, recommend the use of the instrument for diagnostic and operative aid. Concerning the diagnosis in cases of occlusion, Meadows says it is not, as a rule, very difficult, and then uses the following equivocal language: "In the case of obliteration of the os from agglutination of its edges, the main point of difficulty is, of course, the inability to detect any orifice, even after many hours of labor, and though the lower segment of the uterus is driven down into the vagina; sometimes we are able through the thin-stretched wall of the uterus to feel the presenting part, but no indication anywhere of an opening into the uterus." Cazeaux says the existence of occlusion may be suspected when the lower uterine segment descends low down into the pelvis and offers no trace of an orifice; or the orifice may present as a slight depression occupied by a filamentous web, etc.; or the lips may be held together by consistent mucus, and

further, that when pain comes on, the os is tightly closed and seems to ascend and be carried to one side. Glisan writes of occlusion: "It is not generally associated with any rigidity or induration. The cervix will be found during labor to be thin, and to form a smooth, membrane-like covering to the presenting part, but the os may exist as a mere dimple, and be exceedingly hard to find."

Treatment.—When occlusion of the uterus is discovered at the onset of labor, there should be no doubt as to the course to be pursued by the accoucheur. When he is certain that labor has actually commenced, nothing will be gained by waiting. On the contrary, the uterus is soon exhausted by its futile efforts to overcome the pathological barrier placed at its portal, and, unless aid is rendered, rupture may take place or the lower part of the uterus be torn or separated by the powers of the uterus, as already mentioned above. Nothing in the text-books justifies prolonged waiting, although Meadows would give nature a chance. In all cases, except those due to cancer, he says, "it will be right, for a time at least, to give a fair trial to nature's efforts; for even in the case of agglutination of the os, uterine action has been known to effect an opening and subsequent dilatation, so that the labor was completed by the natural process. We ought not, however, to trust to this too long, especially when the contractions of the uterus are violent and frequent, without any corresponding advance, for it is just in these cases that collapse from exhaustion, rupture of the uterus, or laceration of the cervix, has taken place." While yielding proper respect for the opinion quoted above, the reasons given by others for an early resort to means for relief, are, in my opinion, based upon a more correct appreciation of the situation. Chailly says: "In fact, if nature generally conquer this obstacle, and the orifice of the uterus finally open, it is not till after violent and long-continued pains, by which the lower part of the organ is often forced low down into the cavity of the pelvis. . . . The distention is so great that the uterus would finally be ruptured if the agglutination of the neck were not destroyed by nature or in some other way. Hence, this state of things should be remedied at the beginning of labor, or as soon as recognized, in order to spare the female those severe pains which might, of themselves, possibly remove the difficulty."

And, according to Cazeaux, the orifice may open under pressure of strong contractions, but if it does not do so, and the occlusion is not early recognized, rupture or paralysis of the uterus may ensue. Leishman, also, says when this condition is recognized, and an operation deemed necessary, "no advantage, but the contrary, will ensue from delay." The opinions quoted justify me, then, in stating that when occlusion is recognized, the physician will benefit his patient most by at once proceeding to remove the obstacle. How shall be this done?

Every case must be treated according to surgical principles applicable to it alone. If the case be one of agglutination, that is simply sticking together of the lips due to inspissated mucus; or the presence of an adventitious membrane, and the location of the os can be determined, a probe may be forced through the obstruction, followed by dilating instruments, as in my second case; or the membrane may be scratched through by the finger nail; or a cutting instrument may be used for the purpose. In fact, any means which remove the barrier may be resorted to, provided violence is not done by misdirected force. Extensive incisions are not deemed necessary by any authorities. Meadows says (Manual, p. 311): "If it be a case of *agglutination*, probably a small nick on either side will suffice; the contractions will complete the dilatation, and so terminate the labor. It is a good rule always at first to try the effect of a very small incision, which, if nature afterwards proves unequal to the task of completion, can always be enlarged." And Cazeaux contends that the agglutination of the orifice has been remedied in most cases without much difficulty, the membrane having been easily ruptured by the finger or some blunt instrument. He adds: "It is really difficult to understand how this agglutination, which almost always yields to the pressure of the finger, can resist the impetus of the strong contractions of the womb." This matter has been, however, already referred to above.

In cases where no os or cervix can be discovered and it becomes necessary to make an incision, Meadows directs that "a slight puncture must be made first, and this should be done in the interval between the pains, taking care not to injure the child; it should be made a little posteriorly, and, as nearly as possible, in the natural position of the os." Chailly, however,

says the incision should be made "in the anterior part of the lower segment of the uterus." It is generally directed that the incision should be from before backwards, care being taken to avoid wounding the bladder or the rectum, while the uterine arteries are also likely to escape injury if the incision is thus made.

It is, of course, to be expected that, where occlusion is due to cicatricial tissue, the incision will have to be more extensive, and the danger to the patient correspondingly increased; and, as I have no personal experience to relate of this form of obstruction, the paper will be made more complete by reciting two cases from different sources, descriptive of the treatment and results.

In Gooch's *Midwifery*, p. 181, a case is related where, after miscarriage, extensive sloughing took place, involving vagina and cervix uteri. In place of the latter was a hard circle of cicatricial tissue. Three surgeons in attendance agreed in the fact that the os was lost, and that the pains were insufficient to force the head through. The head was low down in the passage, and the patient almost exhausted, when it was determined to cut an os uteri. By dilating the external parts, the cervical portion of the uterus was brought in view, and an incision made, after which the head was forced through, "rending it right and left." The woman recovered.

The other case is from Smellie's collection, and is recorded in his third volume, p. 64, which was published in 1764. The case is interesting from the ingenuity displayed in overcoming obstacles, and I take the liberty of quoting it *in extenso*. The case is introduced as follows:

"An account of the sides of the os uteri grown together in a woman with child, by Thomas Simson, M.D., Professor of Medicine in the University of St. Andrews.

A woman, forty years of age, observably narrow between the ossa pubis and os sacrum, had been four days in severe labor of her first child when I was called to assist her. The child appearing to have been dead for some time, I opened its head and extracted it, but with great difficulty, its shoulders and haunches being too large to pass in the straitened passage between the bones. During some days after her delivery, she passed a great many small rugged stones by the urethra, and at length, after her urine had been stopped some time, her husband drew out of the urethra a large piece of thick membranous substance, three inches in

length, and, in some parts, two inches broad ; one side of it was covered with a crust of small, sharp stones, the other side was inflamed and bloody, which made me judge it to be part of the coats of the bladder separated, and I was confirmed in this opinion by introducing a catheter into the bladder ; for, whenever it touched certain parts of the sides of the bladder, blood came with the urine. The patient continued a long time with a plentiful supuration about the pudenda ; but we did not suspect that the pus came from the internal parts, but only from the exterior, which had been somewhat lacerated. About three months after delivery, she fell again with child, and took her pains after the ordinary period. She continued two days in hard labor before I saw her. The midwife then informed me that the inner orifice had yielded nothing. I left her half a day, and, things remaining in the same way at my return, I examined her condition and found that the os tinæ had not only not yielded, but that the sides of it were grown together, without any vestige of a passage, whereupon I asked the assistance of another physician, and Dr. Haddow being called, was, as well as the midwife, sensible of the case being such as I judged it to be, wherefore we agreed to make an incision into the os uteri ; but we were first obliged to dilate the vagina sufficiently that we might operate more securely. We had no speculum matricis, and therefore behooved to supply it by some other instrument. We tried to make the dilatation with a pair of long-bladed forceps, but they neither had strength to dilate sufficiently, nor did they keep the vagina equally open. After this, we caused two pieces of wood, each three inches long and two and a half broad, to be made concave on one side and convex on the other, and of no more thickness than we thought would be sufficient to bear a strong enough pressure by the necessary dilatation. When these were finely polished and besmeared with grease, I introduced them into the vagina, with the concave faces to each other, then sliding in the legs of a speculum oris between them, and turning its screw, I separated the pieces of wood so far as we could see distinctly the cicatrix of the grown-together parts, and could have easy access to divide them, which I did, by an incision at least half an inch deep, before I pierced through the substance of this part of the womb ; then, immediately introducing my finger at this wound, I touched the head of the child, and felt the whole circumference of the passage hard, like a cartilage, which yielded nothing to several throes she had after the incision, so that I was obliged to guide a narrow-bladed scalpel with my finger, to make several incisions into this cartilaginous ring ; in doing this, there was not the least appearance of blood, and the patient had no trouble, except what the dilatation of the vagina gave her. The labor continuing, the passage dilated a little, but not so much as to give any hopes of its allowing the child's head to pass, notwithstanding the bones of the cranium were overlapped ; and therefore I was obliged to bring away the child as I had done the former. In this birth, there was no liquid with the child, nor did any blood follow it ; it was quite

supple and had a white, chalky crust over its whole body, so that we were convinced it had been dead some time. The want of water was some surprise, till I recollected that, in the time of labor, she told us they were passing, at which time I had the curiosity to make strict observation, and found what she called the waters passed by the urethra, which opened externally by three different orifices; this, with her having lost such a portion of the bladder formerly, and her being subject to gravel, gave me ground to think there was some communication between these passages and the cavity of the womb above the os tinæ, which had allowed the waters to be evacuated. I was the more inclined to entertain this supposition, because frequent instances have been observed of stones making their way through the neighboring parts, as happened to a boy in this neighborhood, who passed a very large stone which had lodged long in the bladder, by the anus, by which the urine had its course for some time after.

My patient, immediately after being put to bed, was seized with a pleuritic pain, very high fever, and difficult breathing, which, coming on so soon after her being fatigued several days with hard labor, during which she slept none, but drank much of everything in her way, appeared to me rather the cause of her death, in twenty-four hours after, than any consequence of the incision I had made; for she never complained of uneasiness in those parts, nor had any hemorrhage. Notwithstanding all the solicitations I could use with her relations, I could not prevail with them to allow me to open her body."

To the cutting operation just described, the name of "vaginal Cesarean operation" has been applied.

In several cases, the Cesarean operation has been performed for the relief of patients who were suffering from occlusion, and some interesting historical points are involved in relation thereto. The cases are not as clearly related with reference to the subject as could be desired, but their recital will be interesting.

The first case is reported by Dr. J. L. Richmond, in the *Western Journal of the Medical and Physical Sciences*, 1830, vol. iii., p. 45, and was performed under the most unfavorable circumstances. The patient lived in a log hut, built of green timber, which had not been built long enough to allow the interstices between the logs to be closed up. The operation was done at one o'clock in the morning, and attendants had to hang blankets so as to keep the wind from blowing out the candle. No attempt was made to open the cervix, and the lochial discharge was permitted to drain away through the lower part of the abdominal wound. Notwithstanding this state of affairs, the abdominal cavity filled up after closing of the wound, and it became necessary to open it again, to give vent to the discharge, and permit the washing out, by warm water and soap, of the peritoneal inclosure, the

woman laughing while the latter procedure was being carried out, because it felt so queer. She recovered.

Dr. Harris says this is the first published case of Cesarean section by an American operator.

The second case is recorded in the *N. Orl. Med. and Surg. Journ.*, vol. viii., p. 194, by Dr. D. B. Gorham, and is interesting from the fact that, as in the former case, an opening was left in the line of the abdominal incision for the escape of the lochia, Dr. G. having determined to make a subsequent operation for opening the cervix. Strange to relate, the object of the surgeon was accomplished for some hours; but, finally, the uterine incision healed, and the retained discharge distended the uterus to such an extent that it became necessary to incise the cervix, to give vent to the accumulated fluid, and the woman recovered.

The third case occurred in the practice of Dr. D'Aquin, and is reported by Dr. Brickell. The woman was ten days in labor when seen by Dr. Brickell in consultation. The occlusion had been overcome by Dr. D'Aquin, but the cervix became tumefied and assumed a sloughing condition. There was also an obstructing band in the vagina. This woman also recovered, and the case is worthy of mention, from the fact stated by Harris that it was the first case recorded in the United States in which silver-wire sutures were used to close the uterine incision, and the third in which sutures were used for the same purpose.

The subject of atresia of the genital passages is an interesting one for consideration, but is so extensive that I have been compelled to limit my remarks to only one branch. Those who desire to investigate the subject further will find material for reflection in the contributions of I. E. Taylor and Lusk.

THE REMOVAL OF THE AFTER-BIRTH.

BY

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It is now thirty years since Credé first published his views about the best way of treating the third stage of labor, and, since then, he has repeatedly returned to the same subject.¹

¹Klinische Vorträge über Geburtshülfe. 1853, pp. 518-602.—Bericht über die Versammlung deutscher Naturforscher und Aerzte in Königsberg im Jahre 1860, in the Monatsschrift für Geburtskunde, vol. xvi., pp. 337-342, and 345.—*De optima in partu naturali placentam amovendi*

The chief point in this method is that the placenta is not removed by traction from below, but by pressure from above. After the expulsion of the child, the accoucheur lays the whole hand gently on the region of the womb, and makes slight friction on as large a surface of the uterus as possible. When this organ is felt to contract, he seizes it with one or, preferably, both hands, spreading out the fingers over the posterior surface, and placing the thumbs in front, and when the contraction reaches its maximum, he presses the whole organ in the direction of the concavity of the sacrum. To press on the uterus when it is not contracted is a fault and has not the desired effect. An attempt is made to utilize the first after-pain for the expression; but, in most cases, the placenta does not come away before the third, or even the fourth. Each time, the same pressure is exercised on the uterus during the contraction, and, in the interval, the hand is kept on the womb, and if the contractions do not follow quick enough, it is used for rubbing that organ gently.

Some German authors¹ have of late years pretended that this method gives rise to several undesirable conditions, especially the retention of the membranes, or part of the placenta, hemorrhage, nay even puerperal fever.

Having been taught in younger years the old way of pulling on the cord in order to remove the secundines, I can testify to the great advantages presented by the modern method of squeezing them out by pressure on the uterus. I have practised this method during the last eight years; I introduced it in the Maternity Hospital as soon as I entered on duty there, and it has given perfect satisfaction. During my last two terms of service as visiting surgeon, the house surgeons (Drs. S. Pierson, F. K. Priest, R. Waldo, H. A. Leipziger, D. Pease, and A. Y. P. Garnett) have, at my request, kept a very exact record of everything bearing on this question. Four hundred

ratione. Programma. Lipsiæ, 1860.—Ueber die zweckmässigste Methode der Entfernung der Nachgeburt; Monatsschrift für Geburtskunde, 1861, vol. xvii., p. 274, and Archiv für Gynäkologie, 1881, vol. xvii., pp. 260-280.

¹ Dohrn, in Deutsche Med. Wochenschr., 1880, No. 41; ibidem, 1883, No. 39; Runge, ibid., No. 44; Schultze, ibid., Nos. 51-52; Kabierske, Centralbl. f. Gyn., 1881, vol. v., p. 145; Ahlfeld: Berichte und Arbeiten aus der geburtshülflich-gynäkologischen Klinik zu Giessen, 1881-82.

and eight women were delivered. Two of them aborted respectively in the fifth and sixth month of utero-gestation, and the adherent placenta was in both cases removed piece-meal by myself, by means of the large dull wire curette and forceps. Among the remaining four hundred and six women delivered more or less at full term, there were six cases of retained placenta. In other words, in four hundred cases the placenta was expressed by Credé's method, and in six only—that is, one and one-half per cent of all—the method failed.

CASE I.—*Adherent Placenta* (accoucheur: Dr. Samuel Pier-son). Ellen C., æt. thirty-nine, married, delivered October 19th, 1882. It was a premature delivery, the utero-gestation having lasted about eight months. The labor was normal up to the third stage. Credé's method was used to no avail. At the end of one hour and three-quarters, the placenta was removed by means of the hand in the uterus. A part of the placenta was protruding through the os, and, on following the cord up, the remaining part was found attached at the fundus, whence it was with great difficulty dissected off. After removal, the placenta was found to be small, very thin, flabby, and made up of two distinct parts joined by an intervening part of the membranes. Recovered.

CASE II.—*Adherent Placenta. Ante-partum hemorrhage* (accoucheur: Dr. S. Pierson). Nellie S., æt. twenty-four, single, second pregnancy. She gave a history of three or four slight hemorrhages during her pregnancy. On October 22d, 1882, she had a severe hemorrhage by which she lost fully sixteen ounces of blood, and which was only checked by tamponade. In the afternoon of the same day, she was delivered of a still-born male child weighing eight pounds and six ounces. The uterus contracted very poorly. There were no after-pains. Pulse 150; respiration 50. The placenta having failed to come away, the doctor, at the end of one hour and a half, introduced his hand and dissected it off from the uterine wall, to which it was very intimately adherent. At the time of its removal it had a distinct odor of decomposition. She died of exhaustion on the same day.

CASE III.—*Adherent Placenta* (accoucheur: Dr. S. Pierson). Mary R., æt. thirty-five. Fifth pregnancy. Breech presentation. Very large child. The placenta failed to come off despite the use of Credé's method. At the end of two hours, Dr. P. introduced with great difficulty his hand into the uterus. The placenta was found intimately adherent to the right corner, whence it was dissected off with great difficulty, and at length it was delivered very much mutilated. The uterus was then washed out with two-per-cent hot carbolized water. Her condition was very bad, but she rallied quite well under the use of hot bottles, stimulants, and morphia. The next day when I saw her, her condition was com-

paratively good. As it was uncertain if all had been removed, I had her etherized and introduced the hand up to the fundus, but found the walls entirely smooth. She had a slight parametritis, and made an excellent recovery.

CASE IV.—*Retained Placenta. Post-partum hemorrhage* (accoucheur: Dr. H. A. Leipziger). Josephine S., primipara, delivered November 30th, 1883. Repeated efforts by Credé's method having failed, and the patient being anemic and losing considerable blood from oozing and the expulsion of clots, the hand was introduced into the uterus, and the edge of the placenta found at one and one-half inches above the external os. It was delivered one hour and ten minutes after the birth of the child, and a hot intrauterine douche of bichloride of mercury solution (1 : 2,000) was given. After that no trouble.

CASE V.—*Precipitate labor. Post-partum hemorrhage. Retained placenta* (accoucheur: Dr. Leipziger). Margaret G. Tenth pregnancy. Delivered November 5th, 1883. Child born precipitately while the patient was at stool. She lost considerable blood by expulsion of clots and a gush of fluid blood. Placenta not coming off by pressure, two fingers were introduced into the uterus and the placenta removed. Patient remained weak and pallid for some time, but otherwise the convalescence was normal.

CASE VI.—*Adherent Placenta* (accoucheur: Dr. D. Pease). Regina G. Third pregnancy. Delivered December 18th, 1883. Pains stopped. Easy low forceps delivery. It was not possible to excite uterine contraction by Credé's method, wherefore the hand was introduced into the uterus one hour and fifteen minutes after the delivery of the child, and the placenta, which adhered to the posterior wall, separated with considerable difficulty. Intrauterine injection of bichloride of mercury (1 : 2,000). Normal lying-in period.

In four of these cases, there existed an abnormally intimate connection between the placenta and the uterus, so that it may be taken for granted that no amount of pulling on the cord would have brought the after-birth away. These cases of true adhesion of the placenta are of rare occurrence, and the supposed frequency of retained placenta is probably in most cases due to improper manoeuvres or undue haste on the part of the accoucheur. By the use of the method recommended in this paper, the frequency of such cases will dwindle down to a small fraction. In two cases only out of four hundred, or in one-half per cent, Credé's method failed to bring the placenta out, although it was not adherent, but merely retained.

In six cases, or one and a half per cent, more or less large

sbreds of membranes were retained. In one of them the whole hand was introduced into the uterus, in order to remove them ; in the other five cases the introduction of the index and middle fingers sufficed for this purpose, and no trouble was observed in consequence of this procedure.

Credé¹ says that he has not seen any bad consequences of leaving the membranes in the uterus, and that the introduction of the hand for their removal might cause infection. He advises, therefore, to leave them alone. Having myself seen very serious post-partum hemorrhage occur in such cases, which was checked as soon as the uterus was emptied, I have made it a rule for myself and my pupils to remove retained parts of the after-birth in all cases. By thoroughly disinfecting the hand, and following the removal of the membranes up by an intra-uterine antiseptic injection, I have never seen any bad results, and I take the danger of infection under these circumstances to be smaller than that of hemorrhage and infection by leaving parts of the secundines behind. Formerly we used carbolic acid, a five-per-cent solution for the hand and a two-per-cent for the uterus, but since October 1st, 1883, when I introduced the bichloride of mercury treatment in the Maternity Hospital,² we have exclusively used the latter drug in a solution of 1 to 2,000. In no case was any part of the placenta retained.

Credé's method recommends itself by being modeled on the natural course of labor. It helps Nature by employing her own means. All it does is to increase the strength of the normal contractions of the uterus by which the placenta is expelled. In some rare cases we see this expulsion follow instantaneously after that of the child, but in the majority of cases, after the strong contractions at the end of the second stage, there is some atony of the womb. More or less long time intervenes before the placenta is thrown off ; in some cases a more or less considerable amount of blood collects in the membranes, which are finally pushed out as a bag full of clots and fluid blood, and in not a few it does not come off at all. In the large majority of cases there is therefore a call for artificial help, and then Credé's method comes in as the one that so to say works in the spirit of Nature.

¹ Arch. f. Gynäk.. 1881. Vol. vii., p. 278.

² Garrigues : The Prevention of Puerperal Infection, New York Medical Record, 1883, xxiv., p. 703 et seq.

In the natural process the placenta is expelled by the contraction of the muscles of which the womb is composed. This organ forms a sac with the mouth turned downward and backward, and is only attached in its lower part. The bundles which enter in its composition may be divided into two groups, which may be called the proper and the common muscular bundles. The first form the bulk of the different layers of which the womb is composed. They accomplish only a concentric movement of the walls against one another and approximate the fundus to the mouth. The second comprise the bundles which find a fixed point of insertion on the bony parts of the pelvis. Above and in front we have the round ligaments, so-called, which, during pregnancy become almost as thick as the little finger, send a thick loop up over the fundus, and are attached below to the spine of the pubis. In front and below bundles pass from the cervix on both sides of the bladder to the pubic bone.¹ Behind, the so-called utero-sacral ligaments run from the level of the internal os to the anterior surface of the third and fourth sacral vertebra. On both sides we find the large muscular expansions which form a great part of the broad ligaments, and starting from the edge of the uterus, insert themselves on the side walls of the pelvis.

Through the contraction of this second group of muscular bundles the uterus is drawn downwards into the pelvic cavity, and at the same time tilted in such a way that the fundus moves forward, the cervix backward. This tilting is due to the co-operation of the round and the utero-sacral ligaments, while all the lower bundles probably contribute to the expansion of the internal os.

All these different movements are closely imitated by Credé's method. Through the pressure of the thumbs and the fingers, the walls of the uterus are from all sides brought nearer to one another, and thus the area covered by the placenta becomes diminished. On the other hand, the pressure on the well-contracted fundus pushes the whole organ backward and downward toward the hollow of the sacrum, and at the same time presses the detached placenta against the inner os, mechanically helping to open it.

Besides this purely mechanical effect, the compression of the

¹ Thévenot: JOURN. OBSTET., 1882. Supplement, p. 216.

muscular bundles works in a dynamic way, by increasing the power of the natural contraction, especially that of the fundus.

By shortening the duration of the third stage of labor, and by increasing muscular contraction, Credé's method counteracts hemorrhage, both immediately during the third stage and after the end of labor. During the period referred to, we had among four hundred and eight patients only two cases of post-partum hemorrhage besides the two mentioned above (IV. and V.), in which a moderate amount of blood was lost.

CASE VII.—(Accoucheur: Dr. F. K. Priest.) Susan G., *æt.* twenty-three. Third pregnancy. Delivered on December 11th, 1882. Breech presentation, with the sacrum turned backwards and to the right. When she had been in labor twenty-three hours, and was somewhat exhausted, a foot was brought down and a still-born male child weighing ten pounds and fifteen ounces extracted. The perineum was ruptured to the anus. The delivery of the child was followed by a profuse hemorrhage. The hand was again introduced into the uterus, and everything cleaned out. At the same time it was grasped and compressed through the abdominal wall. The uterus was washed out with hot two-per-cent carbolyzed water. Besides, ergotine, brandy, and digitalis were given hypodermically. The hemorrhage stopped, and the uterus contracted well. Later she became affected with diphtheritic inflammation of the genitals and died of septicemia.

CASE VIII.—(Accoucheur: Dr. Garrigues.) Kate R., *æt.* twenty-three, delivered February 14th, 1883. She suffered from nephritis with uremia and purpura hemorrhagica. Her urine contained large granular casts. She had severe headache, vomited continually, and suffered very much without having any real labor pains.

A slight erythema had been noticed in the groins on the previous day. Since then it spread and changed character. The whole abdomen up to the ensiform process was the seat of a uniform rose-colored erythema which disappeared on pressure. But interspersed in the erythema were found purple or blue spots from the size of a pin's head to a pea, which did not change on pressure. The eruption extended three fingers' breadth down the inner surface of the thighs. It was less marked on the thorax, but became again much more pronounced on the neck, whence it even extended over the posterior surface of the body. Petechiæ were likewise found in both axillæ, on the mammæ, and on the back. In both eyes were large ecchymoses.

She was delivered, by version, of a dead child. Considerable hemorrhage following immediately after the extraction, the hand was introduced into the uterus, and the placenta removed without any difficulty. A hot intrauterine injection was given, and

the hemorrhage stopped. She fainted, was revived, but died three hours later.

In both these cases, hemorrhage came on immediately after the birth of the child, before Credé's method could be practised; both patients were exhausted, and the latter had blood extravasations all over the body. We have not had a single post-partum hemorrhage occurring after the expression of the placenta.

When properly executed, Credé's method of expressing the placenta is the best prevention of post-partum hemorrhage.

Inversion of the womb, which is favored by pulling on the cord, is powerfully counteracted by the method recommended; but it cannot be repeated too often that the womb is seized from *all* sides and over *the largest possible space*, that the pressure is chiefly exercised so as to *press wall against wall*, and that the downward pressure on the fundus must never be practised except *when the womb is contracted*.

The fearful avulsion of the whole uterus by pulling on the cord, of which more than one case has been reported, becomes, of course, impossible, when the placenta is removed by pressure from above.

In some cases, the whole after-birth is expelled outside the genitals, but in most only the placenta itself with the larger part of the membranes are expelled. By turning the placenta, the membranes are made to form a kind of cord, and by gentle pulling on this, always seizing it as near up to the vulva as possible, it is easily withdrawn from the vagina without introducing the fingers into the canal. In this way the expression of the placenta becomes even a part of prophylactic antiseptic treatment.

Although I deliver the patient by the so-called London method, that is to say, in the left side position, which I think has several important advantages over the dorsal decubitus commonly used in this country, as well as on the continent of Europe, after the birth of the child, I turn her over on her back, in which position the *expression* is performed much better. The accoucheur stands in a much freer attitude, and can use both his hands on the womb, which by gravitation, sinks down in the most favorable direction for the expulsion of the placenta. Should hemorrhage supervene, the dorsal posture is likewise

preferable with regard to the compression of the aorta, and intrauterine injections.

In his last communication, Credé states that in two thousand deliveries, the average duration of the third stage was only four and a half minutes. In our experience it was considerably longer, but this is easily explained when we remember that in the European clinics the same assistant occupies his place for a long time, and thereby acquires the courage and skill necessary for the early removal of the after-birth, while in our Maternity Hospital a new house-surgeon goes on duty every two months. But in my opinion it is of subordinate importance whether the third stage is allowed to last some minutes more or less. In my own practice it occupies commonly fifteen or twenty minutes. The chief point is that the uterus all the time be prevented from undue relaxation, that very little blood be allowed to accumulate, and that the placenta be removed by pressure.

The assertion that the expression of the placenta should cause puerperal fever is effectually refuted by my experience, as well as by that of others, *e. g.*, Breisky's clinic. Since the introduction of the bichloride of mercury treatment, two hundred and sixteen women were delivered. There was only one case of puerperal fever, which was easily explained by the circumstance that the patient was delivered at the same time as another who brought forth a macerated child and a stinky placenta, and the accoucheur who had care of the latter case, had unfortunately been allowed to examine the former. Even other forms of inflammation and fever had been exceedingly rare. The details for the first three months are found in the above-mentioned article on "Prevention of Puerperal Infection," and during the next three months, a similar healthy condition prevailed in the hospital.

Credé's originality has been impugned, especially by some of his own countrymen, who assert that the same measures were recommended and practised before him by English (Scotch and Irish) accoucheurs. I have, therefore, examined all the older authors whose works are found in the New York Hospital Library, a full score in number. Some of them (Smellie, Charles White, "Edinburgh Practice," and Gooch) recommend exclusively to remove the placenta by pulling on the

cord, except when it becomes necessary to introduce the hand into the uterus, and they do not mention any kind of rubbing or pressure. A second and larger class (R. W. Johnson, David Spence, Hamilton, Merriman, Samuel Bard, of New York, Thomas Denman, Blundell, Charles D. Meigs, of Philadelphia, Robert Collins, Edward Rigby, Francis H. Ramsbotham, Fleetwood Churchill, and David H. Tucker, of Philadelphia) combine pulling on the cord with rubbing or pressure on the abdominal wall and kneading of the uterus, but with them all this latter procedure is only mentioned as a help; all of them remove the placenta finally by traction on the cord or the placenta itself.

A third class, finally, is formed by Johnston, Sinclair,¹ and Murphy,² all formerly assistant physicians in the Dublin Lying-in Hospital, who describe the mode of delivery used in that institution. They show how the uterus is "followed down" by pressure on the fundus during the delivery of the child, how the contraction is kept up after the birth of the child by never leaving the uterus out of the hand, how friction and pressure are used, and state that these measures are often sufficient to expel the placenta out of the vagina; but at the same time they speak of *putting the cord on the stretch, hooking the fingers into the rugosities formed by the umbilical vessels, and drawing on the funis*. Thus not even the so-called Dublin method comes up to Cr  de's, the distinctive features of the latter being, 1st, that it is used in all cases; 2d, that the uterus is grasped from all sides with both hands; 3d, that the placenta is squeezed out by a decided pressure during the uterine contraction; 4th, that the cord and placenta never are touched, except in those rare cases in which the placenta is really adherent, and has to be peeled off from the uterine wall; and 5th, that the fingers are not introduced into the genital canal after delivery, at which time the danger of infection is much greater than before the passage of the child.

The advantages of this method are, 1st, the certitude with

¹ Johnston and Sinclair: *Practical Midwifery*, comprising an account of 13,748 deliveries, which occurred during a period of seven years, commencing November, 1847. London, 1858.

² Edw. Wm. Murphy: *Lectures on Natural and Difficult Parturition*. New York, 1846.

which the aim is attained; 2d, the exact imitation of natural processes; 3d, the prevention of hemorrhage; 4th, the prevention of hour-glass contraction; 5th, the prevention of inversion; 6th, the prevention of avulsion of the cord or the uterus; and 7th, the prevention of infection.

The method of expression has several modifications, for the details of which the reader is referred to Dr. Mundé's article on the "Diagnosis and Treatment of Obstetric Cases by External Manipulation" (*JOURNAL OF OBSTETRICS*, 1880, Vol. XII., p. 361 et seq.).

I need not say anything about the old method of pulling on the cord, because to all the advantages claimed for Credé's method correspond disadvantages in the former.

The third method in use, if it can so be called, is the absolute abstention from any kind of interference as practised, *e. g.*, in Strassburg.¹

In normal labors, they do not even lay their hands on the abdomen, and do not remove the placenta even if it lies entirely detached in the vagina. This method offers all the advantages and limits of an experiment. It teaches us, what otherwise we might ignore, how the third stage of labor takes place in a civilized woman lying in bed in a hospital. In a hundred cases treated in this way, the placenta twenty-four times came off within half an hour, twenty times within the second half-hour, twenty-five times in the second hour, eleven times in the third hour, nine times in the fourth, five times in the fifth, three times in the sixth, twice in the eighth, and once in the twelfth hour. In every case, the fetal surface appeared first in the os and in the vulva. The membranes formed a bag filled with a rather large quantity of blood. The decidua formed a thick, succulent membrane, which commonly was pervaded by numerous blood-vessels. Twice hour-glass contraction was observed. In one case, a decided putrid odor was perceived.

Apart from the experimental interest, I fail to see a single advantage in this method. It does not even give a correct idea of what would take place under natural circumstances. By

¹ Kabierske: Beitrag zur Frage über die Behandlung der Nachgeburtsperiode, in *Centralblatt für Gynäkologie*, 1881, No. 7, vol. v., p. 145 et seq.

the recumbent position in bed, the placenta is artificially prevented from coming off as early as it otherwise would. If we imagine a woman giving birth to a child lying or squatting under a tree, in the open air, it is all but sure that she would not lie for many hours on her back. After a short rest, she would be likely to get up, and the placenta, which, by uterine contraction, had been cast off from the inside of the womb, and pushed more or less completely out into the vagina, would drop out by its own weight, and draw the membranes after it.

Dr. Engelmann,¹ of St. Louis, in summing up his comprehensive study of the obstetric practice of savage people, says: "The placenta usually follows the child, but unless this is the case *massage and expression are invariably resorted to.*"

Nay, even some animals seem to use similar procedures when circumstances permit. Dr. Sussdorff,² of this city, in a most interesting paper on the birth of an elephant, described how that sagacious animal, after the escape of the baby, placed her abdomen upon a short post in the ground, to which she was chained, standing almost upon her head, and grasping the post with her trunk, thus forcing the abdomen with great power against the post for about ten minutes. This is not exactly Credé's method, for the placenta was not expelled before an hour and a half after the birth of the little one; but, on the other hand, it is certainly a recourse to an artificial means of increasing uterine contraction.

In spite of the good results obtained in the small number of cases reported by Kabierske, I do not think they in any way are sufficient to remove the apprehension of dangerous consequences of non-interference which has prompted humanity to watch and abridge the third stage of labor, and to surround it with particular precautions. As it is, we see that hour-glass contraction occurred twice in one hundred cases, while in Maternity Hospital we had none in more than four times as many cases which I have used for this paper. The risk of hemorrhage is evident, since experience on a much greater scale has proved that it occurs much oftener in those cases in which compression of the womb is not used than in those in which it is resorted

¹ Geo. J. Engelmann: *Massage and Expression in Primitive Obstetric Practice.* AM. JOURN. OBST., 1882, vol. xv., p. 622.

² New York Med. Jour., 1882, vol. xxxvi., p. 22 et seq.

to, and since emptying and compressing the uterus when hemorrhage actually sets in proves such an excellent remedy, I need hardly point out that it is dangerous to leave the detached placenta inside the genitals until it stinks. Upon the whole, in a country like this, where confinement cases, fortunately, in a large measure are in the hands of physicians, I do not think it very likely that the method should win many adherents. The accoucheur is glad to get away from a place where he has been detained for hours. He will scarcely allow himself to be retained by some decidua cells, which experience has shown come off quite as innocently in the lochial discharge as if expelled together with the bulk of the membranes. All that is required is that he should abridge the third stage in such a way as not to expose the patient to any danger, and that is best done by Credé's method. Of the three systems now followed in different lying-in asylums, that is the one I think ought always first to be tried. In very exceptional cases, that of pulling on cord or placenta may be required, while total expectancy has many disadvantages and hardly any redeeming qualities.

A CASE OF VESICO-VAGINAL FISTULA AND RUPTURE OF
THE PERINEUM. OPERATION AND CURE.

BY

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THE vast amount of relief to suffering woman which has been brought about by the operation for the cure of vesico-vaginal fistula, the endeavor to do which operation fifty years ago was considered labor lost, has now placed this procedure on a basis of such certainty that even in the most unpromising cases some assurance can be given, although it may be after many attempts that permanent relief can be attained. J. Marion Sims, whose perseverance while a young man established this procedure on a rational and scientific basis, must stand out before the world as one American who has added

something to the world's knowledge and much to woman's comfort. His recent death, although deplorable, was not until he had reached the zenith of operative work in this direction, and left little for his successors to add. Yet, while it may be but a repetition of what has already been done, the record of any case has some individuality about *it* or the *operation*, which it is proper to place on record. The following is believed to be of this character.

Mrs. P. S. W., æt. 31, short, stout, and inclined to "embon-point," with brunette features, was, prior to her accouchement, January 14th, 1881, a healthy woman as far as the pelvic region was concerned, and had been married for seventeen months. For three days prior to the above date, she had experienced pain of a more or less violent character, and at this time was in the active throes of labor for seven hours, during which time she states that she was unable to urinate; at the end of this time, she was delivered instrumentally by a physician of our city, giving birth to a very large still-born male child. Without knowledge of any trouble, she had a slow recovery from the primary condition incident to parturition, soon discovered that the water "came the wrong way," and that she could not get up at the end of two or three weeks, as she had expected, but was compelled to remain abed for five months or more, and then was able to get around the house only with difficulty.

A year or more after, the case came into the hands of the writer, and, on examination, the following condition of the parts was found: A loss of substance in the vesico-vaginal wall involving the trigonum vesicæ and the tissue above it one and one-quarter inch transverse by one inch vertical; vagina contracted to one inch by a band of cicatricial tissue below the fistula, and rendering it very difficult to reach, and very painful for the patient to endure, and from one to one and one-half inch distant from the ostium vaginæ; and, lastly, a rupture or laceration of the perineum down to the longitudinal fibres of the rectum, and including the external sphincter. It should also have been stated that there was a laceration of the cervix on the left side, and the uterus was bound down by adhesions, low in the pelvis, which conditions were not interfered with, as they gave no trouble, and have given none since recovery. The

parts were all tender, exceedingly sensitive on any stretching or handling when examining them, and excoriated.

On May 3d, 1883, the operation on the vesico-vaginal fistula was made in the ordinary manner, except as to the sutures, which were inserted, as in other cases, through the lips of the fistula, but instead of twisting the ends of each suture together, as is generally done, the end suture and the next suture were brought together, crossed in the vagina like an X, the upper portion of the outer suture twisted with the lower portion of the second suture, and the lower portion of the outer suture with the upper portion of the second suture. In doing this, there was no puckering, and it seems to the writer greater security was achieved at the points of greatest danger. The centre sutures were also crossed in the same manner, but the remaining sutures were simply twisted.

A Nélaton catheter was allowed to remain in situ for eighteen or twenty hours, after which time the patient voided urine naturally. On the sixth day of May, menstruation began, although it was two weeks until the proper time, and continued until the 10th of May, no doubt brought on by the mental and physical condition superinduced by the operation. As had been the case at every menstrual epoch occurring since the injury, intense inflammation was set up on and around the perineal laceration, and the condition was such that the poor woman could not submit to an examination until the 18th of May, or fifteen days after the operation, when the sutures were removed, and the rent found closed. Thus, the first step of the procedure was successful. As Mrs. W.'s health seemed so good, and the dread of the menstrual flow passing over the perineal injury caused her to be very anxious for the completion of the second step, the necessary operation was done on May 23d, 1883. The cicatrices measured two inches forward towards the mons veneris and one and three-quarter inches upwards along the posterior vaginal wall. As, in the laceration, the rent had not been carried directly back, but toward the right side; after having proceeded about one inch, the effort to keep the posterior parts well in apposition was much more difficult than at first supposed. Still perfect union took place, the upper or vulvar half uniting by first intention; the posterior or rectal half by granulation, or, at least, more slowly than

the other half. In this operation, a Brickell's stay was used, which certainly did much to secure antero-posterior depth to the perineum, the great desideratum in this operation.

Since recovery from the operations, although the union of the lacerated perineal halves brought the posterior commissure nearly to the meatus urinarius, the patient says there is very little pain in the act of coition, except at the constricted part of the vagina within the pelvis, and which it was deemed most prudent to leave untouched by any surgical operation.

The power to control the urine at the neck of the bladder was completely gone for some time after the operation, owing, no doubt, to long disuse, and also to the loss of some of the muscular fibres of the sphincter vesicæ; but this is now being slowly overcome, and, except when laboring under great excitement, the patient can control the urine. Later intelligence shows complete control and restoration to normal condition as to menstruation and locomotion.

The accompanying diagram may show the manner of suturing the fistula as employed in this case:

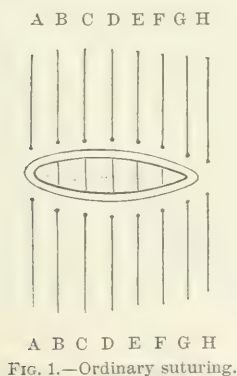


FIG. 1.—Ordinary suturing.



FIG. 2.—May serve to illustrate the crossing of the end and middle sutures.

Explanation of Diagrams.—Allowing the upper lip of the wound to be that immediately below the letters A, B, etc., the modification consists in inserting the sutures as usual towards the vesical surface, but in uniting the upper end of A with the lower end of B, and at the same time the lower end of A and the upper end of B; thus crossing on the vaginal surface in the form of an X, thereby making a more complete closure of the ends. The centre sutures, D and E, are treated in the same way; also, the end sutures, G and H. It will be noticed that the end sutures are slightly beyond the wound or freshened surface.

AN INQUIRY CONCERNING THE RELATIVE INFLUENCE OF THE SEX OF THE FETUS IN UTERO, ON THE MENTAL, PHYSICAL, PHYSIOLOGICAL, PATHOLOGICAL, AND DEVELOPMENTAL CONDITION OF THE MOTHER DURING GESTATION, LACTATION, AND SUBSEQUENTLY.

BY

JOHN STOCKTON-HOUGH, A.M., M.D., Mag. Chem.

(Continued from p. 263.)

13. *Influence of the Sex of the Fetus on the Site, Weight, and Conformation of the Placenta.*

INFLUENCE OF SEX OF FETUS ON SITE OF PLACENTAL ATTACHMENT.

M. JACQUEMIER¹ conceives that, under the operation of a vital cause, the ovum, upon its entrance into the cavity of the womb, has a certain selective power. After the remark that it is found most usually "upon a space a little elevated, to the right or to the left upon the posterior face, next on a corresponding point in the anterior face, next after this upon one or the other sides, sometimes exactly at the fundus, and exceptionally at the neck," he goes on to say: "That it fixes itself there, in obedience to a pre-existing, organic attraction, for it is not to be supposed that all parts of the surface (of the uterine cavity) are alike fitted for the development of the placenta. Like the germs of vegetables, it should have a portion which corresponds to radicles, which, in vegetating, direct themselves by a species of elective affinity, toward a determinate point."

The placenta is usually situated at the fundus of the uterus, according to Gusserow and Schröder, on the anterior wall more frequently than on the posterior (lateral insertions are rare), oftener on the right than the left side. The place of insertion has an influence on its form. Placenta previa, ascribed to multiparity, occurred 56 times in 41,169 cases, or 1 in 763.

While the uterus probably prepares at every menstrual period, and certainly at the time of every conception, in obedi-

¹ Manuel des Accouch., vol. i., p. 273.

ence to a law of its organization, for the reception of the fecundated ovum, whether that ovum reaches the interior of the womb or falls into the abdominal cavity, yet we know that the fetus in most extrauterine pregnancies develops almost as well as if it were in the womb. In these cases, the placenta is necessarily obliged to attach itself to any part where the fecundated ovum may chance to fall. In these cases, as there is no unusual predetermined vascularity in the point of attachment at the moment it is about to be made, we are left to conclude that the simple presence of the placenta must excite the necessarily increased vascularity at the point of its attachment by sending out radicles which penetrate the vessels of the maternal tissue, much as the radicles of parasitic plants penetrate the bark and reach the sap of trees, from which they extract the greater part of their nourishment. The fact that all this can and does happen is no proof that the preparation for the fecundated ovum, which takes place in the womb, is of no service in determining the site of the placenta in that organ.

There are very few accessible statistics in which the site of the placenta is mentioned, but "of 34 women who died while pregnant or soon after delivery, I found," says Velpeau, "upon examining the parts, that the centre of the placenta corresponded to the orifice of the (Fallopian) tube in 20 cases; it was in front of it in 3 cases, behind it in 2, below it in 3, and in 6 cases only toward the fundus."

From statistics collected from various sources, Read, in his memoir,¹ has constructed the following table, showing the relative frequency of the various attachments of the placenta:

	Cases.
At the fundus,	16
At the side, or on the anterior or posterior wall,	565
In the vicinity of the os uteri,	145
Over the os,	11
Undetermined,	187

Admitting that the sex of the fetus has an influence in determining the site of the placenta in the womb, and believing that "the attachment of the placenta to any portion of the

¹ *Placenta Previa: Its History and Treatment.* 8vo. Philadelphia, 1861. P 82.

uterus causes a development of that place which proceeds *pari passu* till the limits of growth in the placenta having been reached, the enlargement is continued, and kept up by the pressure constantly exerted on the uterine walls by the growing contents till the time of parturition" (Read, p. 105), we can readily understand that the development of one particular part of the womb must influence its *shape* and the shape of the uterine cavity. Hence we infer that the position and presentation of the fetus will depend very much upon the *shape* and *inclination* of the uterine cavity. This, then, we may regard as a most important factor in determining the position and presentation of the fetus.¹

Dubois found among 1,915 presentations of the head:

1,355	occipito-iliac	left anterior.
491	" "	right posterior.
55	" "	right anterior.
12	" "	left posterior.

"It has long been my practice," says Tuckey,² "whether rightly or wrongly, I know I have never done any harm by the practice, in cases of tedious labors in which I have given ergot or otherwise interfered, to remove the placenta manually after the birth of the child."

"In cases of female children, I have found the placenta to be attached to the left side of the fundus; when the sex is male, on the right side. Over and over again I have found this to be the case. I have listened with the stethoscope before birth, and have ascertained the position of the placenta by the *bruit*, and have settled in my mind the sex of the child, a fact which has afterwards been confirmed on its birth." He cites fifteen cases, six males and nine females, in all of which the placental site was as above.

"Now, I do not mean to argue anything from the fact that in true lateral hermaphroditism, the halves of sex are right and left, male and female. . . . I do not know whether my idea that the right ovary is intended for the production of males and the left for that of females has been thought worthy

¹ May not the greater frequency of the O. I. left anterior position be due to the greater frequency of the insertion of the placenta on the right side?

² The Position of the Placenta Relative to Sex. Med. Press and Circular, March 13th, 1878, p. 211; Practitioner, London, vol. xxi., 1878, p. 412.

of proof or the reverse. . . . Yet it seems such an easy solution of the matter."

Having discussed the influence of the sex of the fetus on the site of the placenta in general, it remains for us to inquire into the influence of sex in producing *placenta previa*, a condition that is roughly estimated to occur in one out of every thousand pregnancies,¹ and prove fatal to the mother in one out of five or ten cases. It occurs more frequently in multiparæ than primiparæ, from which medical men infer that it is due to a greater flaccidity of the uterus or larger cervical canal among multiparæ at the time that the fecundated ovum entered and fixed itself in the womb.

The only author whom I have thus far found who has expressed any opinion on the influence of sex in producing placenta previa is Silas Hubbard,² and he contends that there should be a larger proportion of female children where placenta previa exists, because he is of the opinion (in common with many other authors, ancient and modern) that women conceive with females during or at the finish of the menstrual flow, when the uterus is more relaxed and the cervical canal more patulent; hence female conceptions by simple gravitation would fall to a position nearer the cervix than males, who are conceived during the intermenstrual period, when the cervical canal is narrower.

The following is his (Hubbard's) own explanation. "According to my theories I should expect that in placenta previa cases the children are females because, as I have said, the ovum which grows to be a female is probably fecundated while within the uterus; it therefore seems that it is more liable to form an attachment at the neck of the uterus than if it had been fecundated while within the ovary or Fallopian tube. In this latter case I should expect that the semen had so impregnated it with growing and attaching powers that it would fasten to the uterus as soon as it reached its cavity, and thus it would not be so liable to reach the cervix uteri as if it had been fecundated at a later period when it had advanced much nearer the cervix without attaching powers."

¹ Schroeder gives 56 cases of placenta previa in 41,169 births, or 1 in 763. Collins gives 11 cases in 16,654 births, or 1 in 1,514.

² Buffalo Medical and Surgical Journal, 1855, p. 654.

Among the many memoirs and papers I have on placenta previa, and of those I have examined in libraries, very few mention the sex of the child; even those that treat the subject statistically make no classification by sex. In one paper only out of a hundred which I have consulted, have I found any mention of the sex, and in this one, I regret to say that the proportion of sexes does not indicate any influence of sex as a cause determining this condition, though the number is too small to be of much value. There were forty-one boys and forty girls, about the proportion of sexes in births in general.¹

Though placenta previa has generally little disposition to reappear in successive labors, yet a very remarkable instance of its recurrence in fifteen successive labors is recorded by Read, p. 140. The woman began child-bearing at eighteen years of age, and continued at intervals of eighteen and one-half months, until she was forty-five, to give birth to children, complicated in each instance by placenta previa.

In this case there was a permanent cause, whether it was mechanical or constitutional.

The following ancient wisdom concerning the cord may not be uninteresting. Joubert² says that there is a popular opinion that the number of nodes or wrinkles in the umbilical cord of the child of a woman who has just given birth, indicates the number of children that the woman is still destined to have. If the nodes are far separated the children will be born at greater intervals than when these nodes are near together. If these nodes are black or red, she will have as many males, if white, female. Antoine Garnier had the temerity to say in his practice (Chap. 31, *Malad. d. l. Matrice*) that in his time he found by experience that all this was true. The explanation given by Joubert of the significance of these signs is that the greater the number and prominence of these nodes or twistings together, the red or dark color indicates that the uterus is robust, hot, well-tempered, and not humid, and hence in the best possible physical condition, and consequently best suited to the production of males.

¹ Hartcop: *Achtzig Fälle von Placenta Previa*, 12mo, pp. 36. Berlin, 1872, p. 32.

² *Erreurs populaires touchant la Conception et Génération*, p. 157. 8vo, Paris, 1587.

According to Joubert, p. 154, the midwives in olden times, careful of the preservation of the human race, earnestly remonstrated and charitably required of their professional sisters, when it was a boy, to give him a good measure of cord between the belly and the ligature, for they thought that the virile member took after its *patron*, and that it would become larger in proportion to the amount of cord left dangling from the belly. If the cord were tied too near the belly, the bladder would be elevated and the penis shortened by being drawn inwards. In the girls, on the contrary, they taught that it was an advantage to cut the cord short, as such a procedure held the womb up in place and tended to prevent displacements of that organ.

Influence of Sex of Fetus on the Size, Weight, Shape, Structure, and Composition of the Placental Mass.

The size, weight, shape, and composition of the placenta are known to vary greatly, and much more frequently than any organ of the body. There are exceptional cases where small children have large placentæ, and large children have small placentæ. Again, various kinds of degeneration take place in this body, such as fatty, calcareous, etc., all these conditions due to causes as yet unknown. We expect to be able to show that the size depends, to a certain extent, upon the site of attachment, as the fundus, being the most natural site, affords better opportunities of development than the cervix. I cannot find a more appropriate place than this to mention the fact that this paper owes its origin entirely to the discovery, by its author, of the fact that *nature has provided less placental mass (by weight) to nourish a given amount (weight) of male fetus than to nourish the same amount of female fetus, or, in other words, the placenta of the female fetus is heavier in proportion to the weight of its fetus than that of the male is to its fetus.*

"It is undoubtedly true," says Read, p. 85, "for obvious reasons, that taking the average of any large number of cases, the heavier the child, the heavier will be the placenta, but the same facts which prove this also prove that there is no agreement between their weights in individual cases, and that for children of a certain size the variation of the weight of their placentæ will be between very wide extremes, showing most

conclusively that there is no regular parallélism between their growth."

Read found, in 338 cases taken without selection, the following relative weights of placenta to fetus:

		MEAN OUNCES.				MEAN POUNDS.		AVERAGE POUNDS.
19	placentæ weighed	12.	Children weighed	3 $\frac{1}{4}$ to	9 $\frac{1}{2}$.			5.31.
39	"	" 16.	"	"	2 "	10.		6.86.
105	"	" 20.	"	"	4 "	10 $\frac{1}{2}$.		7.89.
82	"	" 24.	"	"	3 $\frac{1}{2}$ "	10 $\frac{1}{2}$.		7.85.
30	"	" 28.	"	"	6 "	13 $\frac{1}{2}$.		8.13.
21	"	" 32.	"	"	7 "	12 $\frac{1}{2}$.		9.28.
4	"	" 36.	"	"	7 "	11 $\frac{1}{2}$.		8.25.
4	"	" 40.	"	"	6 $\frac{1}{2}$ "	12 $\frac{1}{4}$.		9.12.

It is conceded by all authors that usually the fundus of the uterus is the first to develop in pregnancy, and that the cervical portion begins to develop only when the seventh month is reached; now, may not the fixation of the placenta about the cervical portion of the womb, from the restricted space here, as compared with the fundus, prevent the placenta from attaining its full size and weight, and may we not say that the size, weight, and conformation of this organ are dependent, in a great degree, to its site in the uterus? And this is why we see such discrepancies between the weight of the fetus and its placenta.

Now we come to the influence of the sex of the child on the weight of its placenta, as compared to the weight of the fetus itself—a comparison (by sexes) not hitherto made, at least so far as we know. The average weight of the human placenta is about five hundred and fifty grams, based on fifteen thousand observations.

The umbilical cord and fetal membranes weigh, according to the best authorities, from sixty to eighty grams, say a mean of seventy-five grams in the case of boys, and sixty-five grams in the case of girls.

We know, by actual measurement, that the umbilical cord in children of primiparæ is, on an average, one centimetre shorter for girls than for boys (based on six thousand nine hundred and fifty-eight observations), and two centimetres shorter among the girls than among the boys of multiparæ

(based on eight thousand six hundred and forty-nine observations).

Not having any statistics of the comparative weight of the membranes of male and female fetuses, we are forced to conclude that they are larger and consequently heavier in the case of males than with females, because the male fetus is larger.

Now, having determined that the cord is longer by a mean of one centimetre, or one-fifty-third of its whole length, and the membranes larger and hence heavier, in the case of the male fetus, by—say a total excess of five grams (excessive weight of membranes and cord combined), in primiparæ.

Among the eight thousand six hundred and forty-nine multiparæ, we know, by actual measurement, that the mean average length of the umbilical cord of the male fetus exceeds that of the female by two centimetres, or one-twenty-seventh, and that the mean weight of the male fetus exceeds that of the female by two hundred and fifty-two grams, or one-thirteenth. If it be true, then, that the membranes alone weigh seventy-five grams, we must subtract one-thirteenth of this weight, or, roughly, six grams plus the weight of two centimetres of cord, or two grams; in all, say eight grams from the total weight of placenta, *with membranes and cord*, in order to justly compare the weight of the *naked placenta* of the male with the naked placenta of the female.

We pursue this plan, because the weights given by Tarnier undoubtedly include the membranes and cord with the placenta, and, as we do not know exactly how much to subtract from this gross sum to arrive at the weight of the naked placenta, we subtract from the total average weight of placenta, membranes and cord, what we have calculated to be the excess of weight of membranes and cord of males over that of females, viz., three grams in the case of children of primiparæ, and eight grams in the case of the children of multiparæ. Hence the following figures,¹ with the above excess eliminated:

¹ Elaborated by the author from the tables of M. Tarnier, for sixteen years, 1860-76, at the Maternité, Paris, out of Dict. Encyclop. des Sciences Médicales. 8vo. Paris, 1878. Mot Fetus, p. 487.

Primiparæ at Term (Actual Measurement).

No. observations.	Weight.	Length.	Weight.
3,794 boys; placenta,	527 gms.;	cord, 54 cm.;	child, 3,146 gms.
3,164 girls;	" 529 "	" 53 "	" 3,101 "

Multiparæ at Term (Actual Measurement).

4,623 boys; placenta,	548 gms.;	cord, 55 cm.;	child, 3,371 gms.
4,026 girls;	" 540 "	" 53 "	" 3,120 "

 15,607
Table *Corrected* by Eliminating *Excess* of Cord and Membranes with Males.

Primiparæ.

No. observations.	Weight.	Length.	Weight.
3,794 boys; placenta,	[524] gms.;	cord, [53] cm.;	child, 3,164 gms.
3,164 girls;	" 529 "	" 53 "	" 3,101 "

Multiparæ.

4,623 boys; placenta,	[540] gms.;	cord, [53] cm.;	child, 3,372 gms.
4,026 girls;	" 540 "	" 53 "	" 3,120 "

Table Showing Relative Weights of Placenta, *Assuming Girls to be as Heavy as Boys.*

Primiparæ.

3,794 boys; placenta,	527 gms.;	weight of boy,	3,164 gms.
3,164 girls;	" [537.7]"	" " girl,	[3,164] "

Multiparæ.

4,623 boys; placenta,	548 gms.;	weight of boy,	3,372 gms.
4,026 girls;	" [592.2]"	" " girl,	[3,372] "

Primiparæ (Corrected).

Boy's w't, placenta,	524 gms.;	1 gm. placenta produces	6.03 gms. boy.
Girl's " " "	529 " 1 "	" " "	5.86 " girl.
			—17

Multiparæ (Corrected).

Boy's w't, placenta,	540 gms.;	1 gm. placenta produces	6.24 gms. boy.
Girl's " " "	540 " 1 "	" " "	5.77 " girl.
			—47

¹ The above figures are obtained by proportion, as follows: If 3,101 grams of fetus require 529 grams of placenta to nourish it, how many grams of placenta will 3,164 grams of fetus require? 3,101 : 529 :: 3,164 : 537.7.

Primiparæ (Corrected).

To produce each gram of male fetus it requires.....	.1656	gms. of placenta.
To produce each gram of female fetus it requires1705	“ “ “
Excess of placenta required to produce	—	
1 gram of female fetus.....	+.0049	“ “ “

Multiparæ (Corrected).

To produce each gram of male fetus it requires.....	.1601	gms. of placenta.
To produce each gram of female fetus it requires.....	.1730	“ “ “
Excess of placenta required to produce	—	
1 gram of female fetus.....	+.0129	“ “ “

We have, therefore, clearly shown that, for every gram of female fetus, a greater weight of placenta is required to nourish it than for the same weight of male fetus.

We have also shown that, notwithstanding the greater average weight of the male fetus, yet the placenta of the female exceeded that of the male by one-fifty-second of its own weight in the case of primiparæ, and by one-fourteenth in the case of multiparæ.

The placenta, being so much heavier in the case of the female fetus, is necessarily larger, and consequently has a larger surface of attachment to the womb—a fact which we expect to help us to explain the reason for the Mosaic law (Leviticus, chap. xii., verses 2, 4, 5) which required a considerably greater length of time for the purification of women who had given birth to female children.

14. *Influence of the Sex of the Fetus on the Quantity, Quality, and Duration of the Lochial Discharge.*

Every woman, after the birth of a child, has, for a certain number of days, a sanguinolent and serous discharge from the genital organs, varying in quantity, quality, and duration, according to her age, temperament, social condition, number of pregnancy, condition of health, accidents of parturition, and sex of the child. The greatest factor, in its influence on the flow, is that of lactation—those who do not nurse lose 1,880 grams, while those who give suck, lose only 1,085 grams. From this, it is fair to infer that the temperament of the mother ex-

erts a great influence—those most humid losing more than those of a dry nervous temperament.

According to Gassner,¹ who endeavored to determine exactly the quantity of the lochial discharge, and constructed the following table based on his observations, we have the:

<i>Lochies Sanguinolentes</i> , 1st to 3d day . . .	1,000	grams.
“ <i>Séreuses</i> , 4th “ 5th “ . . .	280	“
“ <i>Blanches</i> , 6th “ 8th “ . . .	205	“
<hr/>		
Total lochial flow on first 8 days . . .	1,485	grams.
A woman who nurses loses in all only . . .	1,085	“
One who does not nurse at all . . .	1,880	“

Burdach, in his *Physiology*, states that the lochial discharge lasts to the 3th or 5th day—less bloody, 8th to 10th day—serous, 10th to 30th day, when the woman nurses, and as long as 6 weeks when the woman does not nurse.

The earliest and only intimation we have that the sex of the child has an influence in determining the duration of the lochial discharge is in the book of Leviticus. The following is the Mosaic law concerning the purification of women:

“If a woman have conceived and borne a man child, then shall she be unclean seven days; . . . And she shall then continue in the blood of her purifying three and thirty days, . . . But if she bear a maid child, then shall she be unclean two weeks, as in her separation; and she shall continue in the blood of her purifying threescore and six days.”—Leviticus, chap. xii., verses 2, 4, 5.

The various commentators on the Pentateuch are not at all agreed upon the reason for this discrimination. Weemse ascribes it to the greater humidity of the female sex. Lange contents himself by referring to Hippocrates and Aristotle, and concludes “that this law has its foundation in the belief of antiquity that ‘the bloody and watery issue last longer after the birth of a female than of a male.’”

In this paper we have to deal with the physical and physiological reasons for this difference in the duration of the lochies according to the sex of the fetus.

In the first place, what evidence have we that the flow is longer after the birth of females than after the birth of males?

¹ *Monatsschr. f. Geburtsk.*, vol. xix., p. 5.

And, in the second place—granted that it is longer—how do we explain why it should be? We have only the Scriptural assertion as an answer to the first, but, should we be able to explain why the flow should be longer with girls, we will have answered, in a measure, the first question.

Ten years since, I caused to be made, by a then resident physician of the Philadelphia Hospital, some observations on the duration of the lochial discharge in which the sexes of the children were kept separate. These observations were carefully made by a thoroughly reliable man, and the results recorded, but not published. When I inquired for them, they had been stored among some books and were inaccessible; so I was only able to get the general result, which confirmed the Scriptural teaching, viz., that the lochial discharge continued for a greater length of time after the birth of girls than after the birth of boys. It is greatly to be regretted that these tables are inaccessible; but I hope that some one else, who has the opportunity, will add to our knowledge of this subject, by making as many observations as possible, and publishing them in tabular form. It is evident that women in the same condition of life socially, and of the same temperament, should be compared. The following is a good form of table, the figures being imaginary:

PRIMIPARÆ WHO NURSE

GIRLS.

BOYS.

CASES.	AGE.	TEMPERAMENT.	SANGUINO- LENT DAYS	SERIOUS DAYS.	WHITE D'S. TOTAL D'S LOCH. DIS.	CASES.	AGE.	TEMPERAMENT.	SANGUINO- LENT DAYS	SERIOUS D'S	WHITE D'S. TOTAL D'S LOCH. DIS.
1 21		Sanguine..	3	2	6 11	1 22		Sanguine...	3	2	2 7
1 23		Lymphatic	2	4	8 14	1 24		Lymphatic	2	3	3 8
1 20		Nervous...	3	2	4 9	1 22		Nervous...	2	2	2 7
3 21.3		Average.			11.3	3 22.7		Average.			7.3

Another table for multiparæ who nurse.

Another table for primiparæ who do not nurse.

Another table for multiparæ who do not nurse.

There are three physical causes for suspecting that the flow is greater, or lasts longer, after the birth of females, viz., we have shown in this paper that the placenta is heavier in the

case of a female fetus than with a male, and consequently larger, leaving a larger denuded or bleeding surface on the uterine cavity at the time of birth. Second—the process of formation and development of the female fetus from the time of quickening is more rapid, the time being shorter than with a male, leaving the mother in a more exhausted state from the effects of gestation. Third—a female gestation determines a greater, or does not disturb the natural humidity of the mother, whereas a male gestation probably determines in the mother a larger proportion of fibrin in the blood; in short, the blood is less aqueous than when a female is born.

From all of which we conclude that the lochial discharge continues for a greater length of time after the birth of females than after the birth of males.

15. *Influence of the Sex of the Fetus on the Quantity, Quality, and Duration of the Lacteal Secretion.*

In the fifth proposition of this paper, we discussed the influence of gestation on the composition or quality of the blood of pregnant women, as compared with the same women when not pregnant. It was there shown that pregnancy exercised a marked influence on the composition of the blood. "The density, both of the defibrinated blood and of the serum, is diminished, the water, the fibrin, and the phosphorized fat are increased, while the corpuscles and the albumen are diminished."

Andral and Gavarret found, on examining the blood of thirty-four pregnant women, that in the last months the fibrin increased to 4.3 above its physiological standard in the non-pregnant state. Having thus demonstrated the fact that pregnancy has the power of altering the proportion of the different elements entering into the composition of the blood, we are persuaded that this proportion differs more or less in each pregnancy, according to the age and temperament of the mother, and the sex, size, and vigor of the fetus.

We can understand, then, that the quantity, quality, and composition of the milk may be perceptibly influenced by the operation of one or more of these causes.

That there is an appreciable difference in the composition of the milk of women who bear boys, from those who bear girls, is quite possible, but whether the greater proportion of

fluids entering into the composition of the fetal mass when a girl, than when a boy, causes the blood and milk of its (the girl's) own mother to become *more aqueous* or *less aqueous*, we are not prepared to decide, there being arguments in existence on both sides, as we shall see in the vulgar traditions of the women of Montpellier recorded by Joubert.¹ The two important questions upon which the decision of this point depends are: 1st, Does the excess in the proportion of fluids in the female fetus over that of the male diminish the proportion of fluids in the blood of its mother, or does it determine in the mother a condition of humidity corresponding to its own? 2d, Does the humidity of the mother correspond with that of the fetus when it is female, because both are of the same sex, and differ only when the fetus is male, or of a different sex?

We have no clue to the solution of these questions, except the fact ascertained by Martegoute, quoted in another place, that ewes that gave birth to female lambs were heavier at the time of conception, and lighter at the time of weaning, than those that bore males. If we take into consideration the fact that male lambs are heavier than females, this fact is increased in importance, indicating that it requires more nourishment after birth to produce a given weight of female mutton than even a *greater* weight of male mutton, corresponding exactly with what we have shown in this paper in proposition thirteen, on the placenta, wherein we have shown conclusively that it requires during intrauterine life a greater weight of placenta to nourish the female fetus than the male. We are taught that conditions tending to physical exhaustion are attended with what is vulgarly termed impoverishment of the blood, or the proportion of fluids is increased, or what is the same thing, the solids diminished. This condition of the blood and the attendant physical exhaustion may account for and support the commonly received opinion that women have a better color who bear boys. It seems then to be highly probable that the influence of the female fetus on the mother does not tend to greatly

¹ Joubert: La Première et Seconde Partie des Erreurs populaires Touchant la Médecine et le Régime de la Santé. 8vo, Paris, 1587. 1st Edit., Bordeaux, 8vo, 1579. 2me Livre de la 1re partie des Erreurs populaires touchant la conception et la génération, pp. 59-246. Italian translation of this last part—Gioberti, Errori popolari sulla concezione et generazione, 4to, Fiorenza, 1592.

alter the composition of the blood, and that little, in the direction of greater humidity; while the male fetus probably determines in its mother a larger proportion of solids in the blood, and a corresponding difference in the milk, rendering the process of lactation less lengthy and less exhaustive.

Morier tells us that the Persians suckle male children two years and two months, and females two years complete.

Our women, says Joubert,¹ hold that the girls ought to nurse a shorter time than the boys, that eighteen months are enough for girls, while boys ought to nurse twenty-four. The reason which leads the women to say that the girls ought not to nurse so long as the boys is (according to my views) because they are more humid. He contends that this is false reasoning, as the girl ought really to nurse longer than the boy, to keep her humid, which is her nature, to prevent her from becoming dry, masculine, and prematurely old.

He further observes² that the women of Montpellier have the opinion handed down from one generation to another that the milk of a woman who has given birth to a girl is better for a boy, and vice versa. He contends that the milk of the woman who has borne a son is less hot than the milk of the woman who has borne a girl, and that the girl has need of a milk less warm (more aqueous). For a proof of this it is only necessary to observe the color and consistence of the milk which, according to our authors, are as follows: *Celuy d'une fille est roussatre, clair et ichoreux ou sereux, comme la virulence, excrement bilieux et chaud. D'un fils, le lait est plus blanc et epais, significant la chaleur y estre moindre de beaucoup, par ainsi le lait de celle qui a fait un fils, conviendra mieux à une fille, d'autant qu'il est moins chaud, et naturelle complexion de la fille requiret (pour y estre conservée, selon la condition de son sexe) semblable nourriture, et le fils sera mieux nourri du lait de celle qui a fait une fille, p. 222.*

Concerning the influence of the sex of the offspring on the mother during lactation, we have undoubted evidence in the practical experiments of Prof. Martegoute³ who caused, at the

¹ *Erreurs Populaires*, pp. 241 et seq.

² *Ibid.*, p. 222-225.

³ *De la Production des Sexes chez le Mouton. Journal d'Agriculture Pratique*, 8vo. Paris. 1858, p. 37 to 39.

end of each month, all the ewes of the flock of Blanc to be weighed individually, and thanks to these monthly weighings, various tables were constructed in which one saw the diminution or increase in weight of the different classes of sheep, according to age, sex, and the end one had in view to determine.

"Two of these tables were devoted to breeding ewes, one to those which produced and nourished males, and the other to those which produced and nourished females.

"The comparison of these two tables furnished two remarkable facts :

"1st, The ewes that gave birth to females are, on an average, superior in weight at the time of conception to those that gave birth to males, and they lose sensibly more in weight than the latter during suckling.

"2d, The ewes that gave birth to males weigh less (at conception and during gestation) than those that bear females, and do not lose so much as the other class during suckling."

It is clear, then, from this practical test, that among sheep, the ewes that give birth to female lambs are heavier (fatter) at the time of conception, than those that bear males, and that at the time of weaning they have lost a larger proportion of their weight (which they had at the time of conception).

From all of which we are forced to conclude that gestation with and suckling of female offspring is physically a more exhaustive process on the mother than when the product is a male, corresponding with, and further confirming the fact already shown in the part of this paper on the placenta, that a greater weight of placenta is required to nourish the female fetus than the male.

(To be concluded.)

REPORT ON THE PROGRESS OF OBSTETRICS AND GYNECOLOGY IN GERMANY.

BY

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IN continuing my report on the progress of obstetrics and gynecology in Germany, I comply with the wish of the Editor of

this JOURNAL, and add to the general report some cases presenting special features of interest.

In reference to some points touched upon in my last report, I wish to note in the first place, as regards the possible danger of even weak *solutions of corrosive sublimate*, that, aside from a case observed at our clinic here, an analogous case of fatal poisoning after the employment of this agent in the puerperium has quite recently been published by Stadtfeldt, of Copenhagen. Our case was that of a puerpera, recently delivered, with complete rupture of the perineum extending very high up; the rent was stitched up while the wound was irrigated with sublimate solution 1 : 1,000. About the fifth or sixth day, the patient was attacked by moderate fever with low pulse and died on the twelfth day after a very fetid diarrhea. The autopsy showed extensive gangrenous destruction of the entire mucous membrane of the large intestine, continuing also, though of lesser intensity, into the ileum, where it gradually terminated. Examination of the intestines at the Patho-chemical Institute clearly demonstrated the presence of mercury in the tissues. A similar case is reported by Stadtfeldt in a recent number of the *Centralblatt f. Gyn.* A puerpera, having some fever, was given on the fifth day after labor an intrauterine irrigation of sublimate solution of 1 : 1,500. During the irrigation there was slight collapse, and five days later increased diarrhea, vomiting, and suppression of urine. The case terminated fatally. In the large intestine there were likewise numerous ulcerations, and besides, parenchymatous nephritis. Mercury was not found at the chemical examination of the organs. These two cases, in which comparatively small quantities of a moderately concentrated solution of bichloride of mercury were employed, must certainly impress upon us the need of the greatest caution in its employment in the puerperal woman.

In order to elucidate the question as to the most favorable management of the *post-puerperal period*—a question which, as I stated in my last report, was receiving attention by German obstetricians—extensive experiments were made at our institution by Dr. von Campe; viz., in one hundred of the most normal labors possible, the post-puerperal period was managed altogether on the expectant plan. Briefly the results are: Even when waiting as long as thirty-six hours, the placenta hardly ever came spontaneously, but had to be finally removed from the vagina or the cervix. Defects of the membranes, on the whole, were less frequently observed; but they occurred nevertheless. The loss of blood during labor, in general, was hardly less than with Credé's method; but it was certainly diminished during the first days of

the lying-in; during the later days, however, more considerable hemorrhages were remarkably frequent. As regards the complete exfoliation of the decidua, on which the apostles of the expectant plan lay particular stress, it must be admitted that at times very thick layers of decidua were observed, while formerly they were certainly very rare. Altogether, however, the proportion was not so noticeable as to lead us to change our former management of the post-puerperal period entirely on that account. Hence these observations could not induce us to follow a more expectant method, as opposed to the undeniable advantages of Credé's plan, if cautiously carried out.

Within the last few months there came under observation several practically most interesting cases of *retention of dead ova* which in part were watched for many months at the gynecological polyclinic. In one of the cases, in which the pregnancy was arrested after having advanced to about the fourth month, and which remained for nearly a year at the same point, an ovum was finally expelled which in size nearly corresponded to the fourth month, together with an embryo of about the end of the second month. In two other cases, which likewise had remained stationary at the same point of development, there were finally expelled, once spontaneously and once by artificial interference, empty membranes without fetus, but in a perfectly fresh condition. The three patients had an absolutely normal lying-in.

In the field of *operative obstetrics* some prominent communications and observations must also be mentioned. In the first place, we had at our clinic a case of *extrauterine pregnancy* which was in reality tubo-ovarian. The patient was a primipara, aged forty, married ten years, but sterile thus far, who had expected her confinement some weeks before. An examination under an anesthetic gave no positive result; the empty uterus could not be demonstrated; the sound penetrated to the left for a distance of thirteen centimetres without meeting any obstruction. The patient gradually began to have high evening temperatures; the cervix was once more dilated with tupelo tents so as to permit exploration of the uterine cavity. Inasmuch as this furnished no certain diagnosis either, nothing being encountered but a canal greatly contracted above, and as the patient's condition urgently demanded some interference, laparotomy was performed. The ovisac, firmly adherent all around, containing a greatly decomposed and fetid, very large fetus, was emptied, thoroughly disinfected, and loosened from its attachments. The detachment was successful and complete, so that finally the whole thick-walled ovisac was connected with the uterus by only a thin pedicle. The

latter was ligated and the ovisac thus completely extirpated. Unfortunately the patient succumbed to septic peritonitis. On closer examination, the ovisac proved to consist largely of ovarian stroma, but in immediate communication with the walls of the tube; the lumen of the salpinx directly merged with the cavity of the ovisac, both together forming a connected space similar to that in tubo-ovarian tumors. A more detailed description of this exceedingly interesting specimen will shortly be published by Mr. Beaucamp. This case again proves that it is possible, in certain extrauterine pregnancies, to remove the entire ovisac, as had been formerly proposed and done by Litzmann and Worth. The same procedure—to remove the ovisac in toto, though in earlier months of gestation—was recently proposed in a paper read before the Obstetrical Society of Berlin by J. Veit, who extirpated a *pregnant tube* in the third month, though, indeed, in consequence of an erroneous diagnosis. The unexpectedly favorable conditions be encountered there, together with the benign course, induced him to propose this procedure on principle, after the diagnosis has been made. It so happened that a short time afterwards another patient with tubal pregnancy came under his treatment, and J. Veit, after making the diagnosis, performed the operation. The latter proceeded smoothly without any particular difficulty, and the further course was quite normal.

In reference to another pressing question in operative obstetrics—the *Porro modification of the Cesarean section*—several recent publications have been submitted: one by Fehling, of Stuttgart, on four cases, and one by Carl Braun, of Vienna, on twelve cases of Porro's operation. Of his four cases, Fehling lost one by sepsis and secured four living children; Braun lost four out of his twelve patients and obtained eleven living children and one asphyxiated child. With the exception of one case, in all the operations the pedicle was treated extraperitoneally; they were, all of them, cases of absolute contraction of the pelvis due to rachitis or osteo-malacia. Nearly all were treated with the elastic ligature, and the parietal peritoneum was united to the peritoneal investment of the stump. The latter was seared with the thermo-cautery and treated dry. Altogether, therefore, the mortality in these sixteen cases amounted to twenty-five per cent.

As regards the occurrences in the field of GYNECOLOGY, there came under observation here quite an interesting case of secondary *atrophy of the ovaries* and of the *uterus* in consequence of *diabetes mellitus*, in a young woman whose menstrual activity had previously been perfectly normal. The probable cause of the rarity

of such cases appeared from the records of 14,000 polyclinical patients, inasmuch as diabetes usually manifests itself after the menopause, and hence its influence on the sexual functions is not likely to be noticed.

German gynecologists devote continual attention to the various *irritative states of the uterine mucosa*. Two interesting publications on this subject have been issued: one by Duevelius, of Martin's clinic, "Contribution to the Knowledge of the Uterine Mucosa," another by von Campe, of Schröder's clinic, on "the Alterations of the Uterine Mucosa in the Presence of Fibroids." The former paper in the main aims to disprove, on the strength of careful anatomical examinations, the objections raised by the opponents of the use of the curette in affections of the uterine mucosa. By means of a number of uteri from the cadaver he proved, in the first place, the possibility of thoroughly scraping the entire internal surface of the uterus, down to the deepest layer where the glands extend between the muscular bundles. Furthermore, in five cases, in which the uterus was subsequently entirely removed, he was able to prove the correctness of the microscopical diagnosis made from small particles removed by the curette. Finally, by means of sixty cases in which pregnancy followed the curetting, he disposed of the assertion that curetting renders subsequent pregnancy impossible.

Campe's article deals mainly with the changes of the mucosa in the case of myomas. Nearly always he found the mucosa of the entire uterine cavity diseased, usually affected with the more edematous form of endometritis, often associated with deep proliferation of the glands between the muscular fibres. Conspicuously ectatic vessels, the rupture of which might perhaps directly explain the profuse hemorrhages, were hardly ever encountered. These results tend to explain why, in the case of profuse hemorrhages from myomas, the treatment otherwise only employed for endometritis will be effective.

On the subject of *myoma operations*, two recent publications have been issued—one by Kaltenbach, of Giessen, and one by Kuester, of Berlin.

In the *Zeitschrift für Geburtshülfe*, Kaltenbach reports ten myotomies with one fatal result; in all of them, the pedicle was treated extraperitoneally according to Hegar's method. Kaltenbach adheres to the latter, though he believes that the intraperitoneal treatment of the pedicle will be so far perfected as to become the method of the future. The indications he formulates are the well-known ones; he strongly advises early operation.

Basing on a favorable case, Kuester advises, in the case of large myomas situated in the pelvic connective tissue, after enucleation to stitch the detached peritoneal investment into the abdominal wound so as to separate the large wound entirely from the peritoneal cavity. The latter, then, is to be drained into the vagina. In some cases, this proposition is worthy of consideration.

Finally, mention should be made of two clinical observations, the nature of which, indeed, is not as yet quite clear. Both of them occurred in Schroeder's clinic. One was a patient who had undergone a very simple ovariectomy in the morning; in the afternoon she presented all the symptoms of internal hemorrhage so that, in the absence of Prof. Schroeder, I was forced to reopen the abdomen. Then a large proportion of the coils of the small intestines was found completely distended with blood, while in the abdomen there was only a little bloody serous fluid. The abdomen had to be again closed, and the patient soon succumbed in spite of every effort. At the post-mortem, not only was the whole intestine tensely filled with blood, but the entire mesentery showed hemorrhagic infiltration, as did also the intestinal wall.

Another similar observation was made during an exploratory laparotomy, in a case in which no cause could be discovered for an ascitic accumulation. Rendered confident by a preceding very favorable result, Prof. Schroeder sprinkled some iodoform into the abdominal cavity. On the third day, the patient had very violent hematemesis, and blood was likewise present in the stools. At the autopsy, the intestinal walls were found very friable, and the bowels much distended with blood. In neither case could any cause be discovered for this peculiar phenomenon, despite a most careful examination.

BERLIN, February, 1884.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF NEW YORK.

Meeting, November 20th, 1883.

SARCOMA OF THE CERVIX UTERI.

DR. JAMES B. HUNTER presented a specimen which he had removed at the Skin and Cancer Hospital from a patient sent to him by Dr. Arango and Dr. Townshend. She was thirty-seven years of age, and the mother of one child. On examination by the va-

gina, a mass could be felt, bleeding easily, apparently malignant in character, and connected with the cervix uteri. It gave rise to considerable hæmorrhage, but not to pain. On the 24th of October the patient was placed under ether, and the growth was removed with the spoon-saw and scissors. It was found to be attached to the posteriorly lip only, but the attachment extended from the external to the internal os. Microscopical examination proved the growth to be a spindle-celled sarcoma. The patient made a good, but somewhat slow recovery. Dr. Hunter had found it rare for sarcoma to develop in the cervix uteri.

DR. HENRY J. GARRIGUES said that, while sarcomatous growths from the cervix were indeed comparatively rare, they seemed not to be so rare as had been supposed. This was the third case which had come to his knowledge within less than a year. The first case occurred in the practice of Dr. Burke, of South Norwalk, Conn., and the specimen was referred to Dr. Garrigues for examination, and was reported on at the meeting of December 20th, 1882. The second case was that of an old woman, from whom Dr. Garrigues removed the sarcomatous growth by means of Simon's spoon and Paquelin's cautery. The patient died of peritonitis. In those two cases the affection was the small-round-celled sarcoma. Dr. Hunter had just related the third case.

DR. JOHN G. PERRY asked Dr. Hunter whether the growth had not originated in the cavity of the uterus and afterward become attached to the cervix.

DR. HUNTER replied that it had evidently developed from the cervix in the first instance.

DR. JOHN BYRNE said that some years ago he removed what was regarded as a vascular tumor from the posterior lip of the uterus, and found that there still remained a projection from the posterior cervical wall, interfering somewhat with the passage of the sound. Microscopical examination showed that the growth removed was sarcomatous in character, and he then regretted not having removed the entire tumor. The patient had only suffered pain at the monthly periods. Two months ago she returned with the same symptom, and, on examination, the growth in the cervix was found to have remained stationary. Was it possible for sarcoma to remain quiescent for so many years?

DR. GARRIGUES thought it was possible that a mistake had been made as to the true character of the growth, based on the microscopical examination. As Dr. Polk had suggested, the fungous growths of a chronic endometritis often present the characteristics of diffuse sarcoma, and the two could be distinguished only by their clinical history.

DR. B. F. DAWSON reported further on the case of

PAPILLOMATOUS CYST

related at the last meeting. The patient did very well until the sixth day, when absorbent cotton carried into the drainage-tube showed slight signs of the presence of pus. Dr. Wilcox, the house surgeon, sent him word in the evening that the patient complained of pressure about the heart, and of numbness of the left leg and arm, that the tongue did not deviate to the left, but that there was some difficulty of speech, and that the pupils were normal.

Dr. Dawson immediately visited the patient, and found the temperature was 100.4° F. The tube, on being examined, was found to contain pus, which Dr. Dawson believed was probably due to part of the discharges from the wound under the seat of the clamp entering the peritoneal cavity alongside the drainage-tube. He decided, therefore, to remove the clamp, and adapted a hard-rubber Hodge pessary, flattened out, to take its place in holding the stump in the abdominal wound by silver wire twisted around the pedicle, the ends being fastened to the side of the pessary resting over the wound. The temperature began to fall in a few hours, and the patient soon felt better. Similar symptoms again developed about the tenth day, but disappeared on emptying a mural abscess. The drainage-tube was allowed to remain until the ninth day, when he was sure that an impervious wall of lymph had formed around it, and then its place was immediately supplied for a few hours by a roll of absorbent cotton, renewed twice. Three days ago a slight discharge had taken place from the vagina, but he did not think it due to the clamp, as this had been applied so low down on the fundus as to involve the cavity. He considered the patient out of danger. Dr. Dawson thought that doubtless death had resulted in more than one case from the presence of the clamp, which might have been avoided by early substituting the pessary and wire or other support for the pedicles.

TWO CASES OF VICARIOUS MENSTRUATION.

DR. GARRIGUES related the following cases: One was that of a woman, forty years of age, who had ceased to menstruate a year and a half before, since which time she had constantly had what she called "milk in the breasts." It consisted of a thick, yellowish-white fluid, which, on microscopical examination, was found to be colostrum. He prescribed Carlsbad salt internally, and belladonna locally. He had seen the patient but twice. The second case was that of a woman, forty-seven years of age, in whom the menopause had taken place fifteen months before. In her case there was constant and profuse perspiration over the entire body. Tincture of belladonna was prescribed internally. She had not been seen since.

DR. A. JACOBI had seen two cases of a discharge of milky fluid from the breasts after the menopause. In one it occurred in a woman about seventy years of age, who made no complaint, and therefore was not subjected to treatment. The other case was observed many years ago, and was taken for vicarious menstruation. The patient improved within six or eight weeks under the influence of cold locally and ergot internally. In that case there were very large fatty granules in the secretion, as was also true of colostrum.

DR. PERRY had seen three cases of excessive perspiration following the change of life—all of them in unmarried women. He had considered it due to a neurosis, and treated it by rest and regulated nutrition. Two of the patients quickly recovered. In the other

case the trouble lingered for two or three years, and was not relieved until after a sea voyage.

DR. POLK said that about three years ago a patient came under his care who had not menstruated for six months. The abdomen was enlarged, the breasts contained milk, and she supposed she was pregnant. The uterus was found to contain a fibroid tumor, but no fetus.

DR. GARRIGUES referred to the sympathetic relation between the mammae and the genitals, which was illustrated in the case of a virgin, both of whose breasts secreted milk for three or four days following an intrauterine injection of a dilute solution of chloride of iron, given to arrest hemorrhage from a fibroid.

DR. CLEMENT CLEVELAND had seen a woman, fifty-three years of age, who, since ceasing to menstruate, at the forty-fifth year, had had a monthly bloody discharge from the nipple. She had been under his observation for about two years, and he had considered the case to be one of vicarious menstruation.

LOCAL EDEMA, CONFINED ABOUT THE EYE, DURING MENSTRUATION.

DR. BACHE MCE. EMMET related the case of an unmarried lady, aged thirty-two, who, at each menstrual period, had a localized edematous condition of the face, confined about the left eye and the lids, together with ptosis. The sight was not affected except by the swelling of the lid. He regarded the condition as due to a neurosis, and it had temporarily improved with restoration of the general health. There also existed an obstinate eczema about the genitals.

DR. JACOBI thought the case related by Dr. Emmet specially interesting in the absence of pain.

PERSISTENT FLACCIDITY OF THE UTERUS AFTER DELIVERY.

DR. PERRY related the case of a lady, thirty-eight years of age, the mother of five children, the youngest being four years old. She had complained, since the birth of this youngest child, of bearing-down pain and inability to stand or walk, from the great pressure and weight in the pelvis. Menstruation occurred every three weeks, and lasted, with scanty flow, seven and eight days. On making a vaginal examination, he found the cervix in its proper position, but very short and flaccid, and, extending around it in a circular direction, a soft mass, apparently the result of a pelvic inflammation. On conjoined manipulation, the mass appeared very shallow, but nowhere could he find the fundus of the uterus. A sound was introduced, which penetrated but two inches. Not doubting that it was within the uterine canal, he essayed a little force, and with this was able to penetrate to the depth of eight inches, and, while doing so, found that the mass in the pelvis disappeared. He had, indeed, while forcing the sound upward, carried the uterine wall before it. An electrode was substituted for the sound, and a current of galvanism conveyed to the endometrium. This was done from day to day, without, at first, eliciting more than a slight flow of blood; but in time contractions followed,

and finally sufficient firmness was established to justify the use of a vaginal support. At this juncture ergot and galvanism were used together, and a complete cure was eventually established. The patient had since become pregnant, and promised to do well. Dr. Perry thought this a case of subinvolution resulting from sudden and complete cessation of contraction after labor, leaving the uterus in a state of collapse; but he thought some might regard it as essentially a case of paralysis.

DR. JACOBI thought that the treatment by electricity should vary according to the view taken of the cause of the condition. In cases of paralysis of certain muscles, or groups of muscles, relief was sometimes obtained by applying electricity to the nerve centres. The quick result following the application of galvanism to the uterus in this case would rather point to subinvolution being the cause of the paralytic condition. So soon as absorption of the abnormal, hyperplastic material had been accomplished by the action of the galvanic current, the uterus resumed nearly its normal shape.

DR. ROBERT WATTS said that two similar cases had been related to the Society, one of which occurred in a patient at Charity Hospital some years ago. On making an examination, he passed the sound a distance of nine inches up the uterine cavity, and it could be felt through the abdominal wall. It was believed that the sound had passed through the uterine wall, and an attack of peritonitis was awaited. The patient had no bad symptom, however, and the procedure was repeated several times afterward by different physicians. Dr. Isaac E. Taylor called the case one of "balloon uterus." The other case was similar, the sound passing a distance of eight inches and a half.

DR. POLK referred to his experience in hospital practice with Hegar's method, a modification of Simon's, for the examination of the pelvic organs, which he considered much superior to the ordinary method by conjoined manipulation. In his experience it had not been attended by any bad results. In many cases, he believed, the true condition could be determined with more certainty by this method.

PROCEEDINGS OF THE NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, April 3d, 1884.

REMOVAL OF THE AFTER-BIRTH BY CREDE'S METHOD.

DR. H. J. GARRIGUES read a paper on the above subject.¹

The discussion was opened by DR. ALEXANDER S. HUNTER, who said he believed that the uterus should expel the fetus from its cavity unaided, and also from the cavity of the vagina, unless there was danger that the child would suffocate or there was some other special indication for manual interference. He also believed

¹ See page 486.

that by withholding manual aid, except as indicated, the uterus would almost invariably be found to be firmly contracted upon the placenta after the birth of the child, and that the contraction would almost certainly be supplemented by others which would quickly complete the third stage of labor.

Moreover, such treatment, in his opinion, offered the patient one of the most efficient safeguards against excessive post-partum loss of blood. Emptying the uterus rapidly of the child by manual aid removed a most efficient stimulus to uterine contraction, and left behind an inertia which often called for the most pronounced "expression" to effect the delivery of the placenta.

DR. A. HADDEN regarded it as especially important to complete the third stage of labor thoroughly, not only for the purpose of preventing post-partum hemorrhage, but also to prevent the development of conditions so frequently met with by gynecologists.

He had attended 2,400 cases of labor, and in the last 1,000 cases, the records of which he had just consulted, he found that one death occurred from hemorrhage (the woman was suffering from purpura hemorrhagica). There were six deaths from causes incident to labor: one from cardiac paralysis after a difficult labor, two from rupture of the uterus, one from the effects of a tedious labor, and one from abscess.

His method of managing the placenta had been partly by traction and partly by pressure upon the fundus of the uterus, always determining that the uterus was firmly contracted, and the tumor globular before making the least traction on the cord.

With reference to traction producing inversion of the uterus, he had never seen any indication in that direction from the amount which he had employed, which had always been very moderate.

DR. MARY PUTNAM-JACOB I threw out a theoretical suggestion which might favor Credé's method. Up to the time the uterus is expected to contract and expel its contents, the circulation has been predominantly venous; but as the moment approaches when the contractility of the organ will begin, it becomes extremely desirable to have the arterial instead of the venous circulation preponderate, and in the natural order of events the venous blood begins to be cut off by the formation of thrombi at the margin of the placenta. The question had often occurred to her whether or not defective contractility on the part of the uterus might not be due to abnormal persistence of the venous circulation due to defective formation of thrombi. Credé's method further diminishes the quantity of venous blood, favors the formation of thrombi, and therefore possibly continues a physiological process already begun.

DR. P. F. MUNDÉ said that he had practised Credé's method for a number of years, and for a time was perfectly satisfied with it. Still, there were times—perhaps, he was in too much of a hurry, a little too active in expressing the placenta—when the placenta could not be expressed without inflicting too much suffering upon the woman, and too much pressure upon the uterus.

The method of procedure, however, which had given him the greatest satisfaction, and which he recommended, was, as soon as the head of the child was expelled, to make steady, gentle manipulation and friction of the fundus, and follow it up until the uterus has diminished in size, so that it reaches only about half-way between the umbilicus and the symphysis pubis, and when no higher than that and the organ has a globular outline, one might be sure that the placenta was no longer within the uterine cavity. It may, however, be in the cervical cavity, or partly in the cervix and partly

in the vagina, and in such cases Credé's method would often not effect its removal. Then by the introduction of one or two fingers, or even of the whole hand, either of which he regarded as exerting no deleterious influence whatever if proper antisepsis was observed, the after-birth could be readily removed.

The point he wished to make was that Credé's method was excellent if followed to the letter. The method aims to effect expulsion of the placenta by first producing contraction of the uterus, which brings about loosening of the placental attachment, but not until detachment of the placenta has taken place should expression be made.

Dr. Mundé then referred to a case of adherent placenta, also to a case of encysted placenta, in which Credé's method could not have been of any avail.

He made it a rule to examine the after-birth in every case, and if it was incomplete, he invariably introduced his hand into the uterine cavity at once and removed whatever was left. To leave such cases to nature he regarded as criminal. He did not accept the plan at all of allowing the placenta to remain and come away when it gets ready.

Dr. J. H. FRUITNIGHT regarded Credé's method as a good one to practise in most cases; but probably there were some in which it would prove of no avail. He did not favor the let-alone policy, but thought that, inasmuch as the placenta had completed its function, it should be removed.

Dr. ISAAC E. TAYLOR thought more harm than good was done by interfering with nature's method of delivering the placenta. During fifty years he had not introduced his hand into the uterus more than four or five times for the purpose of removing the placenta, nor had he made traction upon the cord for that purpose. Indeed he knew of no authority for making traction upon the cord for the purpose of delivering the placenta. But if the cord was taken hold of with the fingers close to the vulva, and slight traction made, it would be easy, if the uterus was contracted, to determine where the placenta was attached; if anteriorly, lift upward with the cord, and if posteriorly, separate the thighs of the woman and push downward, and the general traction, with this manipulation would cause the prompt delivery of the placenta, and it should not be expressed. He disapproved strongly of introducing the hand into the uterus at once after the delivery of the child, for the purpose of removing the placenta.

Although Credé's method might come in as an adjunct, he thought it better to allow nature to do her own work, which would not be attended by harm if delayed for an hour or even two hours.

Dr. E. D. RAMSDELL had been obliged to peel the placenta off in only two or three cases out of seven hundred and forty-three cases of confinement which he had attended in private practice. He had had one death, which he believed to be due chiefly to the fact that the nurse, who regarded herself as extremely intelligent, gave, unbeknown to him, vaginal injections every hour for four days, and the woman died of peritonitis. He once waited forty-eight hours for the delivery of the after-birth. He then proceeded to deliver it, found hour-glass contraction to be the cause of the retention; septicemia followed and the woman died. He then began to apply the method of making gentle traction on the cord, not too much, moderate, with moderate but firm pressure over the region of attachment of the placenta, and had, on

an average, removed the after-birth within fifteen or twenty minutes after delivery of the child.

DR. GARRIGUES, in closing the discussion, said all would agree concerning the desirability of conforming as much as possible to the physiological processes of nature, and the diversity of opinion was only with reference to how nature's method could be best imitated. There certainly was a difference between traction on the cord and manipulation of the uterus through the abdominal wall. He believed that the best way was not to touch the cord at all, but try first to determine whether or not the afterbirth would come away as the result of gentle pressure. With reference to inversion of the uterus, he thought traction on the cord was one of the recognized causes of the accident.

With regard to Dr. Mundé's way of practising Credé's method, he thought it was rather a modification than the real method, because Credé contemplated expressing the placenta from the fundus of a contracted uterus, not allowing it to stop until expelled from the vagina, and in Dr. Garrigues' cases it had nearly always been thrown completely into the bed. If the placenta was left until it entered the cervical cavity, surely Credé's method could not remove it. It was quite another thing to make pressure while the afterbirth was within the body of the uterus. Dr. Garrigues was unable to see how the location of the attachment of the placenta could be determined without introducing the hand into the cavity of the uterus. He agreed with Dr. Taylor entirely, that it was rash to introduce the hand into the uterus within half an hour after delivery of the child. While, in his cases, the average time was perhaps five minutes before the placenta was expelled, yet if it did not come away within that time, he would wait, if needs be, an hour or even more, but he would not allow it to remain and decompose.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, Thursday, February 7th, 1884.

The President, R. A. CLEEMANN, M.D., in the Chair.

A YEAR'S WORK IN OVARIOTOMY.

DR. WM. GOODELL read a report of his cases of ovariectomy during the past year. Of these he had twenty-five with seven deaths. Eleven of them were performed in the private rooms of the Hospital of the University of Pennsylvania with one death. Ten were operated on at their own homes with five deaths, and four at his private hospital with one death. He attributes the majority of these deaths to three causes: *First*, that he made it a rule never to doom a woman to certain death by refusing to operate on her, however forlorn the hope of her recovery might be, and three of these fatal cases were very ill indeed at the time of the operation, from septicemia and purulent cysts. *Secondly*, that the women of this country, being unwilling to go to a hospital, either insist on being tapped, which increases the risks of the radical operation; or else

postpone the operation until the tumor has become very large, adhesions have formed, and the health has become greatly undermined. Out of his twenty-five cases he had but eight without adhesions, and one of these, a case of double ovariectomy, died at home, he not having seen her or treated her after the operation. *Thirdly*, that five of his fatal cases had been operated on at their own homes, which were so distant that he was able to see but one of them after the operation, and she did not rally, but died in eight hours from the shock of the removal of an intra-ligamentous cyst universally adherent. The fatal case at his private hospital was one of pelvic abscess bursting into the bladder. The operation was not a difficult one, although the lower portion of the cyst had to be separated from the two layers of the broad ligament by which it was enveloped. The sole fatal case out of eleven performed at the Hospital of the University was due to hospitalism. This he deemed a good record, for the building is used as a general hospital, with many railroad accident cases and suppurating wounds. In this hospital he always operated in one of the private rooms, where the patient was kept until all danger was over.

While acknowledging that his results were not so good as those of British ovariectomists, he said that on the other hand it was to be said in his favor that he never refused to operate on a woman, however slim her chances of recovery. He made this statement because, according to remarks made by Dr. Sutton at the last meeting of the American Gynecological Society, Billroth is the only one among European ovariectomists who "refuses to operate upon nothing that deserves the chance of life," and his success amounts to about sixty-five per cent, whilst one of the most successful of British ovariectomists "does not remove very large tumors, viz.: those which weigh from sixty to sixty-five pounds, with extensive adhesions, etc."

Out of the twenty-five cases of ovariectomy, there were twelve in which both ovaries were removed. In all these cases the second ovary was positively diseased. Yet with a larger experience he (Dr. Goodell) was becoming more and more inclined to remove both ovaries in all cases of malignant degeneration of one ovary; in all women who have passed the climacteric, or who are approaching it, and in all cases complicated with fibroid tumor of the womb.

Finally he referred to the fact that some ovarian cysts, although they may appear to the eye benign, show from their subsequent history that they are malignant; the woman dying a few months later from carcinoma of the pelvic organs or at the site of former adhesions. Very unfortunately there are no diagnostic criteria to indicate the character of such a cyst.

DR. A. H. SMITH feels that Dr. Goodell is hardly right in saying that in England desperate cases are rejected. In his own experience, while in England recently, several cases which were very desperate, and which would have been generally rejected here, were operated upon successfully and were ready for discharge

three weeks after operation. It occurred to him that the lower average temperature had much to do with the result. The thermometer is not allowed to rise above 65° F. in the room in which the patient is kept; and in Lawson Tait's cases it rarely goes over 60°, and never up to 65°. Perhaps this has a good effect in preventing hyperpyrexia. The English ovariologists are also very careful in details, especially in preventing hemorrhage, and use an immense number of hemostatic forceps. Ovarian cysts are remarkably common in London.

DR. B. F. DAWSON, of New York, upon invitation by the President, spoke of the English ovariologists and alluded to what he had himself seen while on visits to England as to very desperate cases being operated upon. His own late experience has been so good that he was inclined to attribute it, as well as the good results of the English operators, to absolute painstaking in the most trifling details, with absolute cleanliness; not simply antisepsis, but the utmost care throughout every step of the operation. Among other things a very large number of hemostatic forceps are used to secure each bleeding point the moment that bleeding commences. His own last six cases of laparotomy, performed this winter, two of which were very desperate in their character, have been successful and the results he felt were largely due to the avoidance of any preventable hemorrhage and great care through all stages of the operation to prevent blood or fluid from the cyst escaping into the peritoneal cavity. This care is exercised throughout the "toilet of the peritoneum," and he always uses a flat sponge as does Mr. Wells inside of the abdominal walls, upon the intestines while introducing the closing suture, which is removed only when it becomes necessary to do so to twist the last wires.

He always uses napkins to fold over the edges of the abdominal incision to prevent the oozing from the edges escaping into the peritoneal cavity or soiling the operator's fingers. He also enfolds the cyst in towels as it is withdrawn from the cavity, to guard against even a trace of fluid soiling the intestines or wound. He clamps the pedicle temporarily when possible and places a napkin under the clamp before dividing and cauterizing it. Every adhesion that shows any risk of bleeding is cauterized as soon as it is separated, even when ligated. Douglas' pouch especially is sponged out until the sponges come away absolutely clean. In a recent very desperate case, one in which Dr. Emmet expressed his sympathy with him as to its almost certain fatality, the operation occupied two hours and forty minutes, owing to painstaking observance of the above details, and although half of the uterus was removed and adhesions were numerous, calling for many ligatures and frequent use of the cautery, yet convalescence was uncomplicated and speedy.

DR. GOODELL remarked that the quotation in his paper, to which exception had been taken, referred to Mr. Tait. It was taken from a paper read last autumn before the American Gynecological Society by Dr. Sutton, who was with Mr. Tait three months. He (Dr. Goodell) had seen about a dozen ovariectomies in Great Britain and only one was a difficult case, and in that the patient died. There he had seen patients refused on account of their age, while in this country they are not. Dr. Goodell had operated successfully in patients aged, respectively, sixty, sixty-one, sixty-two, sixty-four, sixty-five and sixty-seven years, and some one in this city had a successful case in a patient over eighty-three. Dr. G. had once operated upon a patient seventy-six years of age, of a long-lived

family. This was before the days of antiseptis and the patient died. He believes that minute attention to details is the chief element of success. One important point is to make the abdominal incision sufficiently large to see every adhesion as it is separated, and to ligate or secure immediately every bleeding point. This requires a courage which it took him years to attain. He always observes Spencer Wells' method of placing a flat sponge beneath the abdominal walls when introducing the closing sutures. He does not like wire, but always uses carbolated silk sutures, each eighteen inches long. After all have been placed the ends are twisted together into one strand on either side and caught in the bite of a pressure forceps. The lips of the incision are then widely separated and a final search is made for any oozing, leakage, or accumulation of serum. They are then rapidly tied and the wound dressed. This final toilet of the peritoneum cannot be made when wires are used. He now believes in the use of a drainage tube, but he had hard work to bring himself to the point of accepting it, as he still looks upon the tube as a foreign body, a necessary evil. He had once seen death caused in a healthy man by the simple perforation of the peritoneum, without wound of the intestine by a small stilette, and this had made him fearful of the effect of the presence of a drainage tube. But he now considered that the peritoneum in the case of an ovarian cyst has by thickening and attrition lost much of its vulnerability and does not resent slight causes of irritation. He occasionally resorts to the actual cautery at a black heat to stop bleeding from torn adhesions, but he prefers the pressure forceps, the ligature, or the application of Monsel's solution. He thinks there may be some truth in Dr. Smith's idea concerning the effect of the low temperature of the operating and of the convalescent room. He was struck with the absence of ill results in one operation in London by Dr. Bantock of which he had been a spectator. The day was cold, damp and foggy, and the operating room was chilly, the windows being open; but the patient promptly recovered. One case of his own in which the extreme emaciation and prostration of the patient forbade postponement was operated upon in severe winter weather: the steam pipes at the hospital had been frozen over night, and although they had been thawed out, the temperature of the operating room was only 54°. In this case the cyst had contained colloid matter and had burst spontaneously; all that could possibly be removed was scooped, sponged, and washed out. Much remained behind, yet the patient recovered and afterward became quite fat. This operation was performed three years ago, both ovaries were removed, but another tumor can now be discerned in the abdomen. The patient being fat and also very sensitive, it is not possible to determine accurately its character, but he hopes it is a pedunculated fibroid of the uterus and not a malignant tumor.

DR. DAWSON requested Dr. Goodell to give his opinion respecting the management of the drainage tube and of the importance of the spray.

DR. GOODELL, when operating in private houses, has begun to dispense with the spray. Carbolic acid spray poisons some patients, and he, being afraid of it, only uses it when he does not consider it safe to omit it, as, for instance, in a general hospital, or when there are many spectators present. Year before last, he lost one patient from its use; she had contracted kidneys. Only last week, in a case operated on in the Hospital of the University, the patient was profoundly affected by it, and was with difficulty re-

stored by means of digitalis, stimulants, etc. When he omits the spray, he uses the drainage tube more frequently. If he finds in Douglas' pouch, after the sutures have been placed, and the sponges removed, two or three drachms of bloody serum, and the source of this oozing cannot be found, he inserts a drainage tube; and he also uses it in all cases in which there has been peritonitis or when septic symptoms are present from a purulent cyst. Over the mouth of the tube, he springs a piece of rubber sheeting, which is folded over a sponge squeezed out of a five-per-cent solution of carbolic acid. The sponge is examined and squeezed out every few hours, and the tube moved slightly to clear the openings, and allow any accumulation of serum to escape. As soon as a moderate amount of serum escapes without any tinge of blood, the tube is removed. This generally happens within forty-eight hours, but in one case it was necessary to allow it to remain five days. He never irrigates the abdominal cavity, but he sometimes introduces the long nozzle of a hard rubber syringe into the tube, and draws out any serum that may have accumulated. When he operates in the country, and leaves the patient in the hands of the family physician, he tries to avoid the use of the drainage tube as much as possible.

DR. DAWSON had given some attention to the question of the drainage tube. In New York, two deaths had lately happened after ovariectomies, and in each case the surgeon had ascribed the fatal termination to the too early removal of the tube. Soon after these unfortunate cases, he had operated, and had been obliged to use the drainage tube: on the second and third days after the operation, the serum exuding from the tube was bloody. On the fourth day, it was pure serum. The patient was doing well, and Dr. Dawson concluded to leave well-enough alone, and, with the result in the two other cases before him, allowed the tube to remain. He used a wire, with a pledget of absorbent cotton attached to the extremity, to soak out the accumulated serum, and continued to use fresh pieces until they came away clean. The tube was evidently being walled in by a deposit of lymph around it. On the sixth day, after finding very little serum, the last pledget of cotton brought away a little yellow spot that might have been either lymph or pus. That night, he discovered another trace of the same appearance, and the microscope showed it to be pus. The next day, the discharge was fetid. A salt-spoonful of iodoform was put into the tube every time it was cleaned, and on the tenth day it was removed, and a roll of absorbent cotton saturated with iodoform was put in its place. This was replaced in a few hours with another until all signs of pus ceased. The opening closed in forty-eight hours. The patient recovered. He would like to ask if Dr. Goodell had ever regretted removing a drainage tube too early? Is the drainage tube more of a foreign body on the third or fourth day than it is on the first?

DR. W. H. PARISH had recently performed abdominal section for the removal of a fibroma of the uterus. In this case, he used a drainage tube, although he was not able to pass it down into Douglas' pouch. On the third day, he noticed that the serum escaping from the tube was cloudy, and had a slight odor. On the fourth day, the odor was quite objectionable, and as the tube had evidently become separated from the abdominal cavity by the formation of a canal of lymph around it, resembling in appearance diphtheritic membrane, he decided to withdraw the tube. He washed out the canal with a two-per-cent solution of carbolic

acid. The canal soon closed, and there was no return of the offensive odor.

DR. GOODELL could not recall the loss of a case after the removal of a drainage tube. He has had, however, a number of deaths while the tube was still in position, but that meant simply that the cases were complicated and difficult ones. Was not the tube itself the cause of the pus in Dr. Dawson's case? Air containing germs was of course admitted into the tube in the cleaning-out process, and these germs would cause decomposition of the lymph. Might not also the tube, acting as an irritating foreign body, cause pus? He introduces a stitch in such a manner, beyond the drainage tube, as to make tension of the skin around the tube, and this closes the opening as soon as the tube is withdrawn. He has occasionally followed the removal of the glass-tube by the introduction of a rubber one of smaller calibre, but he has discontinued this practice, believing the glass tube to be the least irritating.

DR. W. H. BAKER reported for Dr. Walter F. Atlee a case of

ABDOMINAL SECTION.

Mrs. E. C., of Erie, Pa., was brought to this city by Dr. Edward Cranch. She was forty-five years of age, married, but had had no children. Her menses were regular, and very abundant. She was a stout woman in excellent health. Four years ago, she first noticed the tumor in the centre of the abdomen. Dr. C. reports that the probe enters the womb the normal distance only.

Diagnosis.—A multilocular cyst, with thick walls and very glutinous liquid, or a fibroid tumor of the uterus. The sense of fluctuation was not distinct.

Operation.—February 4th. After the usual preparation, cut down, found a solid tumor, opened it, took out as much as possible of the contents, and got out the envelopes: the incision was about five inches in length. The tumor grew from the fundus of the uterus. It was transfixed, tied, and dropped. The patient never rallied after the operation, and died on the 6th of shock. There was no hemorrhage. The operation was all over in twenty-five minutes.

DR. R. P. HARRIS saw the patient prior to, and assisted in the operation. The lady had an appearance of health, was quite robust, and there was nothing in the contour or expression of her face indicative of ovarian cystoma. In a conversation with her sister and family physician, he learned that the diseased growth was first noticed by this sister, who remarked upon the central prominence of the abdomen of the patient, who was at the time lying on her back on the floor engaged in playing with a little child. When the attention of the patient was directed to the fact that her abdomen presented a central elevation even when flat upon her back, she readily detected the existence of something abnormal. Dr. Harris remarked to the physician that the history of the case indicated the existence of a tumor at the fundus uteri. When the morbid growth was exposed, it did not present the appearance of an ovarian cyst; neither did percussion indicate the presence of fluid prior to incising the abdomen, except by a surface wave. There was no wave transmitted from hand to hand. The surface wave was found due to some ascitic fluid. As no fluid escaped by tapping, the tumor was opened, and its contents torn

away in pieces so as to reduce it sufficiently, when it was drawn through the wound, ligated at its union with the fundus uteri, and cut away. During the shelling process, a considerable loss of blood took place, and the appearance of the patient was that of great prostration. The tumor had no pedicle, being sessile in its attachment to the uterus.

Stated Meeting, March 6th, 1884.

The President, R. A. CLEEMANN, M.D., in the Chair.

DR. WM. GOODELL made the following

CORRECTION OF A MISSTATEMENT.

"At the last meeting of this Society, I made a misstatement with regard to that distinguished ovariologist, Mr. Tait, which I greatly regret, and which I wish here to correct. I was misled by some remarks made by Dr. Sutton at the last Meeting of the American Gynecological Society. These were so reported as to convey to my mind the impression that Billroth was the only European operator who did not refuse any case of ovarian tumor, however unpromising it was, and that Mr. Tait, to borrow Dr. Sutton's language, 'does not remove many large tumors, those which weigh from sixty to sixty-five pounds, with extensive adhesions, etc.' In quoting this, by a careless slip of the pen I changed the word 'many' into 'very,' and in addition, I wholly misapprehended the the purport of the above sentence. Dr. Sutton has since, in the *Medical News* of February 23d, explained that he did not mean that Mr. Tait selects his cases, for he was 'not aware that this British ovariologist refuses to remove a tumor because it is large;' but that 'Mr. Tait has the largest line of ovary and tube-cases and the shortest line of big ovarian cysts of any man I (Dr. Sutton) visited in Europe.' In making this correction here, I wish to repair the injustice which I unwittingly did Mr. Tait before this Society."

DR. GOODELL then exhibited an

EXTRA-OVARIAN CYST,

with the following history: The lady, aged twenty-eight, and the mother of four children, had a miscarriage early in last October. At that time her family physician discovered the tumor. It slowly grew, but gave the lady so much inconvenience from pain and pressure that she was brought to his office late in the following December. It was not large, but was very sensitive, and was diagnosed to be an ovarian tumor. Both ovaries were removed early last February, and the lady recovered promptly. The peculiarities of the cyst were to him unique. The ovary lay to one side of a thick-walled cyst, and at such a distance from it that the cyst could have been removed without injury to the ovary. The latter was, however, extirpated along with the cyst because it was diseased. Hitherto all parovarian cysts which he had encountered were thin-walled and contained a clear fluid, but this one had thick

walls and contained a turbid brown fluid. It started from the left broad ligament, and was adherent to the bladder, omentum, and abdominal wall. Another point of interest was the fact that the right ovary had doubled its size from follicular degeneration, and yet pregnancy had taken place.

DR. ROBT. P. HARRIS suggested the possible existence of a third ovary as the starting point of the tumor. He also thought that the presence of a third ovary might explain the persistence of the menstrual flow in some cases, after the operation of double ovariectomy.

DR. GOODELL also exhibited a

COCYX REMOVED FOR COCCYGODYNIA.

The patient had met with a fall down-stairs some years previously, and the injury was followed by a vaginal abscess of some kind. She had all the classical symptoms of a very bad coccygodynia, and had fallen into a nervous condition which bordered on insanity. Dr. G. had intended merely to sever the nervous attachments of the coccyx by the sweep of a tenotomy knife, but after the patient had been put under ether, the tip of the bone was found unnaturally movable and giving distinct crepitation. The loose bone was therefore removed, and as the articulating surfaces were found rough and denuded, the whole coccyx was removed by bone forceps. Great relief followed this operation.

Although he had seen very many cases of coccygodynia, this was the first case on which he had operated. In a very few traumatic cases he had wished to operate, but was not permitted to do so. The vast majority of these cases are, in his experience, those of nervous or neuralgic coccyx, and they get well in his hands under rest, massage, electricity, and appropriate constitutional treatment. The great difficulty, in cases of severity, is to decide between the nervous mimicry of the disease and pure traumatic coccygodynia in which positive lesions have been sustained and their effects have not yet passed away, as for instance in a sprained or a fractured coccyx, or in a rheumatic, a gouty, or an inflamed coccyx. There is yet another difficulty in the way of diagnosis, for sometimes an injury received in an hysterical woman is followed by local nervous phenomena, which will last long after the original lesion has been cured. For instance, on one occasion he had been so greatly deceived in the diagnosis between traumatic and nervous coccygodynia as to make him very cautious in resorting to the use of the knife. A highly intellectual lady who spent her leisure in reading metaphysical works, received an injury to her coccyx by the sudden "bucking" of the horse on which she was mounted. She was at that time suffering from nervous prostration, and the blow started up very exacting coccygeal symptoms. Dr. Goodell found retroversion and a prolapse of both ovaries. These dislocations were remedied, and the patient put on a rigorous constitutional treatment; but she grew no better, and an operation was proposed and agreed to. As soon as the day and the hour were decided upon, she lost all pain in her coccyx, and

has not since had a return of it. This happened about six years ago. On another occasion he saw a very obstinate and severe case of coccygodynia, which he had been treating unsuccessfully for a long time, and which had a traumatic history, quickly disappear under an exciting family jar. In view of this experience, he believed it always safer at first to consider coccygodynia as a local expression of a general neurosis, and to treat it accordingly.

DR. J. H. PACKARD asked why Dr. Goodell had preferred the bone nippers to disarticulation in the first case.

DR. A. H. SMITH asked if Dr. Goodell had removed the entire coccyx. (Dr. Goodell was not sure, but thought so. There had been an abscess in connection with the injury, and the bone was dead and somewhat necrosed, he had cut off one piece with the nippers and then disarticulated the remainder.) Dr. Smith, *continuing*, said there had been suppurative action probably following anchylosis. Such a condition might result from injuries received in labor or from falling astride a chair-back or a rail. Most cases were reflex hysterical or uterine pains, as will be proved by the freedom from tenderness when the finger is pressed on the coccyx when making a vaginal examination. He has never removed one, because he has seen such poor relief from the operation in any cases that have come under his observation. Why should relief come unless all the nerves and other painful tissues be also removed? He will be glad to hear the result of the operation in the case reported by Dr. Goodell this evening.

DR. PACKARD demonstrated to the Society a new method of applying

AXIS TRACTION

to any ordinary obstetric forceps. The device consists of two steel hooks arranged to catch in the fenestræ of the blades of the forceps, and terminating in rings through which a wooden handle is to be passed. The handles of the forceps are to be lashed together.

DR. SMITH remarked that Dr. Tarnier's first suggestion was to pass a cord through holes drilled through a widened portion of the blades at the point at which handles are now attached. The hooks exhibited by Dr. Packard did not draw from the right point, and he thought there would be difficulty in adapting them when the head was high up.

DR. GOODELL thinks that Tarnier was anticipated in the cord attachment by another French physician.

DR. B. F. BAER read a paper on

THE SIGNIFICANCE OF METRORRHAGIA ABOUT AND AFTER THE MENOPAUSE.¹

Metrorrhagia recurring about the menopause is as likely to be the result of disease of the uterus or its appendages as it is at any period previous to that time. The popular belief that floodings at the change of life are physiological often results in harm. That the blood loss is depuratory or critical and that it protects the vital organs from injurious congestion is erroneous. Where health exists, the cessation of menstruation will be attended by no more aberrations of function than are seen in its establishment. An

¹ See p. 449.

analysis of twenty-two hundred cases treated in hospital and private practice show that nearly the same number of women sought advice during the establishment and the decline of menstruation, and it further shows that the numbers rapidly increase as the period of greatest fecundity is reached, and decline after it is past.

Epithelioma of the cervix may result from injury of that organ, but also requires some peculiarity in the structure of the tissues which renders them susceptible to an induced dyscrasia. When a woman, in the midst of the fertile period, suddenly ceases to bear children, there is often some local cause for it. There is some causative relation between acquired sterility and cancer. It is safer to believe the disease of local origin, for we will then endeavor to discover and remove all sources of irritation, and possibly prevent its development. Detailed histories of a number of cases are given to illustrate the truth of the positions assumed. Where the menopause is retarded beyond the usual period, the cause can often be found in some diseased condition connected with the sexual system, and, as a rule, it is an old standing trouble. When metrorrhagia recurs, after the menopause has been fully established, it is almost invariably the result of a pathological change in the tissues of the uterus.

DR. GOODELL agrees almost wholly with what Dr. Baer has said; he thinks the dangers of the menopause much overrated. Cancer and fibroids of the uterus occur more frequently at that age than any other, and have caused the popular dread. Although hemorrhage is always pathological, its cause cannot always be discovered, and in this dodging period serious hemorrhage may occur and no dangerous condition exist. He would like to believe that cancerous growths had a benign incipency, but cannot go so far. The microscopists make many mistakes in ascribing malignancy to growths removed from the uterus. Dr. Goodell then gave a number of cases in which experienced microscopists had given prognoses of early fatal termination based upon the cell formation of growths removed from the uterus, but these cases had recovered and now showed no evidence of any diseased condition. With regard to the small proportion of cancerous growth following laceration of the cervix uteri, the doctor called attention to the large number of Irishmen using clay pipes, and the small number of lip cancers, and yet it is universally acknowledged that the use of a clay pipe is the principal cause of such growths.

Blood-letting is practised very freely in Turkey and the East, and women, as a consequence, get very stout; such are more liable to profuse hemorrhage at the dodging period.

DR. WM. T. TAYLOR reported a case of

MALARIAL POISONING IN A NEW-BORN BABE.

We have frequently observed fevers of a malarial type in very young children, in some even during the first year, which were ushered in by a convulsion or other prodrome, without a rigor, as occurs in older persons, and their character is only recognized by a repetition of the attack in a day or two. But the youngest subject of this disease which I have met with is the following case.

Mrs. A. R., during her second pregnancy, was affected with malarial fever, and, although she was then residing at the sea-shore, was obliged to take occasional doses of quinine, to control it. She returned to her city residence at the end of the season, but continued using quinine from time to time until the end of her uterogestation, which was completed in November last, when her babe was born. Her labor was natural and easy, and she had no unfavorable symptoms. The child appeared healthy, was of good color, but was smaller and feebler than her first-born at its birth. As she had a good supply of milk, it soon drew the breast quite vigorously.

About one week after its birth, the nurse called my attention to "weak spells" which it had occasionally, accompanied by coldness of the skin, a feeble circulation, and prostration, which continued for fifteen or twenty minutes and were followed by a clammy perspiration. By the application of heat to the body, and giving it a little brandy and water, or other stimulant, it would revive.

I observed that these "spells" had a periodicity occurring every two or three days, and, considering them malarial, I gave the mother quinine and valerianate of iron, which, acting therapeutically through the milk, soon caused the "spells" to cease, and the babe became well and fat. I also gave it small doses of the elixir of cinchona, for several weeks.

This child must have contracted this disease whilst in utero, through the placental circulation; for, being born in a perfectly healthy locality, it was not exposed to any external malarial influence.

When labor began, the quinine was stopped and was not resumed until the condition of the child required it, when it soon showed its antiperiodic action, by completely arresting these "weak spells;" for now the child is perfectly well.

DR. R. P. HARRIS related a case of parallel character which had occurred, some years ago, in a malarious neighborhood. The mother was under treatment before labor. The child had chills and fever when quite young, and was treated through the mother.

TRANSACTIONS OF THE WASHINGTON OBSTETRICAL AND GYNECOLOGICAL SOCIETY.

Stated Meeting, January 18th, 1884.

DR. C. S. BUSEY, *President, in the Chair.*

DR. JOSEPH TABER JOHNSON read a paper on the importance of
TRACHELORRHAPHY

in certain cases, and gave brief histories of eight operations, all of which were successful with one exception. In this case, the pa-

tient was a subject of hystero-epilepsy, the convulsions continuing after the operation in an aggravated form, causing the sutures to tear out.

In one case, pelvic cellulitis occurred as the result of slight exposure on the third day. The patient was confined to bed four months, but eventually made a good recovery.

In three cases the double operation upon the cervix and perineum was performed at one time, with good results in each case. By having all arrangements perfected, the time of the patient under the anesthetic could be much shortened. Sutures such as could be left indefinitely were recommended.

Dr. Johnson referred to the conclusions arrived at by Dr. Murphy in an article in the *AMERICAN JOURNAL OF OBSTETRICS* for January, 1883. Dr. Murphy holds that "the operation is usually followed by sterility," and that when pregnancy does occur, "labor is unusually severe and protracted," and that relaceration frequently takes place. Much evidence to the contrary was adduced and the belief expressed that Dr. M.'s conclusions were erroneous. If such harm follows trachelorrhaphy, all the great men who approve and perform it, as well as their followers, must be wrong and have already done an immense deal of malpractice.

Dr. Johnson closed his paper with a quotation from Thomas, urging its performance for the purpose of preventing epithelioma.

DR. J. FORD THOMPSON, in opening the discussion, said the subject presented in the paper was interesting and well worthy of discussion. At the same time he felt that he was not ready to go into the merits of the question to the extent it deserved. Dr. Johnson had followed the best masters, viz., American authorities, for in spite of the objections urged against the operation in Europe, we had every reason to say that in this, as in other fields of gynecology, we were ahead of the rest of mankind. Emmet especially deserved the greatest credit for the work done in this special field. While holding that Emmet's ideas were too sweeping, inasmuch as he set aside amputation of the cervix, and ulcerations and their treatment by other means as representing malpractice, still there was so much truth contained in his views that he did not feel like criticising them. He thought the treatment of lacerations of the cervix opened a useful field for future successes, because the operation was easy, and if no previous inflammation existed, we could not say there was any danger in performing it. Thus, Thomas had never lost a case. We simply pared the surface of the laceration and stitched the sides together; no vital part was exposed to danger, and we could confidently expect a success as far as the mere healing process was concerned. In fact we might do the operation oftener, which at present seemed confined to specialists. In Vienna they attached little importance to this operation. He had seen hundreds of cases of ulceration of the cervix in Vienna, and they were all treated by the old remedies; while according to Emmet, all these cases represented laceration and should be treated by trachelorrhaphy. We all knew of cases of badly lacerated cervix without special symptoms. He had in mind a case of this kind delivered by the President; and had examined women for other purposes and found the cervix lacerated. He thought, there-

fore, that the operation should be confined to cases where the symptoms could not be traced to other causes. At the same time, he thought the operation would become more common in the future, like the operation of circumcision for nervous troubles. He had witnessed the operation many times, and it was successful as regarded union, but he felt less confidence in the permanent relief of the general symptoms, inasmuch as he had not been able to see the patients afterwards.

DR. MCARDLE agreed with Dr. Thompson that European gynecologists charged us with the opinion, that in all cases there was lacerated cervix which caused the symptoms. We all remembered cases like Dr. Busey's. Thus, he had seen a case where deep laceration existed, yet the woman did not know it, and enjoyed good health so far as the uterus was concerned. Another point, in none of Dr. Johnson's cases had the patients become pregnant after the operation. Gardner thought the laceration was the cause of sterility, but it seemed that sewing up the cervix might have a tendency in that direction. Dr. Mundé, when presenting the subject before the District Medical Society some years ago, said the operation was common in the United States, known in Germany, rare in England, and not performed at all in France. He also stated at the time that 50 per cent of all women had laceration, more or less, after labor. 25 per cent of these needed no treatment and did not know they had laceration, while another 25 per cent could be treated successfully by astringents and rest, although many of them might be treated by operation, but 12 per cent absolutely demanded it. He also spoke of what he called the "cut-celery stalk" (Goodell) appearance of the cervix, and said that a pre-existing cellulitis contra-indicated the operation.

DR. W. W. JOHNSTON inquired of Dr. Johnson what, in his opinion, were the acknowledged causes of the laceration.

DR. JOHNSON said that in most cases, according to Barker, Emmet, and Thomas, it occurred in those cases where there had been no medical attendance during labor. Barker said that in a period of several years he had seen no case of it in the upper walks of society, but had seen a number, in consultation, in patients in the lower classes, who had been delivered by ignorant midwives. The first cause was lack of skilful assistance; the second, unwise interference, as the pushing up or down of the cervix by the accoucheur. The third cause was the too early use of the forceps and too rapid delivery before the os was sufficiently dilated. And the fourth was ergot.

DR. J. F. THOMPSON thought that Emmet held that lacerations were more frequent in women in high life.

DR. W. W. JOHNSTON said that many cases of lacerated cervix which he had seen were in women who had been without a medical attendant during labor. He had often tried to dilate the os by the finger, and to push up the anterior lip over the head, but he had never felt the neck give way. In fact, the very condition which we relieved by this manoeuvre was the cause of the laceration, for when the lip was down and the head under the pubis pressing the soft parts against it, there were changes of nutrition produced which caused the neck to give way after an hour or so of pressure. He could, of course, understand how application of the forceps would produce it. It was a frequent remark of gynecologists that the laceration was produced by the accoucheur, either by doing or omitting to do something. Gynecologists and obstetricians differed; the former do not appear to appreciate fully the difficulties

so often encountered by the latter, who sometimes cut or tear unavoidably, to save life. As to the question of conception after the operation he had now two cases with all the symptoms relieved, and both pregnant, although they tried in vain to avoid pregnancy. One of these had been operated on without anesthetics, and described the pain as intense, saying that she would never consent to it again if there was need for it.

DR. S. S. ADAMS mentioned the case of a prostitute operated on by Dr. Ashford, where the laceration had been produced by a criminal abortion. After the operation she did not menstruate, but suffered intense pain, so that Dr. Ashford feared that he would have to reopen the os. She was a confirmed taker of morphia by hypodermic injections.

DR. J. T. JOHNSON.—As to conception, the operation had been performed to cure the sterility and tendency to abortion produced by the lesion. In proof of this he read from the Transactions of the New York Obstetrical Society, the evidence of gentlemen in favor of the operation, it being followed by conception, and delivery where no new laceration occurred, and other cases where the cervix gave way in a new place. Speaking of an article published by Dr. Murphy of this city, against the operation as preventing conception, he quoted from a paper by Dr. Van de Warker, in reply to Dr. Murphy's views, in which the author stated that he performed the operation with a view of curing sterility produced by the laceration. He had not referred to this point in his paper, because he had not followed up his cases, and besides they were nearly all widows. It seemed that the operation was likely to be abused, and in order to treat it fairly, he had spoken of the objections made to it. While Emmet stated that nearly all cervices gave way and were lacerated more or less in labor, yet he also said that he did not find it necessary to operate on one-half of the cases. Misled by successes, we might be led to do the operation when it was not required. Thus many slight lacerations healed spontaneously, others gave no symptoms. He had seen a case in which he was anxious to operate on account of the lesion, but there was no need for it. But where there were bad symptoms, where the woman could not walk, as in one of his cases, with bad leucorrhœa, or otherwise incurable ulceration of the os, etc., when we were compelled to give up medication as useless, this was the class of cases that demanded the operation.

ABSTRACTS.

1. Panum: Ovulation, Coition, and Impregnation (*Nordiskt Med. Archiv*, 1882, Vol. XIV., No. 29).—Dr. P. L. Panum, Professor of Physiology at the University of Copenhagen, has published an interesting case, which is a contribution to our knowledge of the relation between menstruation and ovulation and the vitality of the spermatozooids in the female genital tract. He says himself that the whole number of exactly observed cases are yet much too small to determine whether the impregnated ovum, as a rule, was detached at the last menstruation or at the time when the next menstruation should have occurred; but in his case he proves that the latter condition obtained.

A married lady in the higher walks of society, forty-three years old, had borne seven children, the last two and one-half years ago, and had had two abortions in the third month, one and two years before the present one. Since then, she had had no sexual intercourse except once, namely, on the 10th of April, 1882. The last menstruation had been present from the 3d to the 5th of the same month. On the 8th of June, she had again an abortion. Menstruation had been regular up to April, except that it had come three times with an interval of only fourteen days, the last time on April the 3d.

Dr. Panum argues that during the first two or three months the development of the fetus is so regular that every week has its own signs. That, in the present case, it corresponded to the fifth week. The antero-posterior diameter of the head was four millimetres. By the development of a median and two lateral frontal processes, the nasal fossæ had been transformed into grooved canals. The median frontal process showed in the centre a small shallow notch. Under the eye appeared on either side the upper maxillary process, and it rested on the lower edge of the lateral frontal process without reaching the median frontal process. The inferior maxillary processes protruded further than the superior maxillary processes, and touched one another in the median line. The oral fissure, starting from the oral orifice under the median frontal process, extended between the superior and inferior maxillary process below the whole of the eye. The fissure-shaped foundation of the outer ear had an entirely horizontal position. Otherwise, the visceral clefts were closed; but as remnants of the three anterior visceral arches, three corresponding rounded folds were still visible on the neck. The caudal extremity was distinct, and rolled inward with a sharp curvature. The extremities showed the beginning bend of the knee and the elbow. The distance from the vertex to the caudal extremity was eleven millimetres. The heart and the large arteries contained bright-colored blood, and the whole fetus was so well preserved that there could be no doubt that it had lived until near the time the abortion occurred.

It is evident from the period of development of the fetus that it could not have been formed in an ovum detached at the last menstruation, which occurred more than two months before the abortion took place, but that it must be referred to the next monthly ovulation, at which no flow appeared, and which, according to the development of the fetus, must have taken place in the beginning of May, four weeks after the last menstrual discharge, and about three weeks after the single connection. During all this time, the semen must have been preserved in the female genitals in such a condition as to retain its fertilizing power. It is probably kept in the upper end of the Fallopian tube, which is developed so as to form a *receptaculum seminis*. H. J. GARRIGUES.

2. Kubassow (St. Petersburg): **Concerning Endometritis Dissecans** (*Ztschr. f. Geb. u. Gyn.*, Bd. IX., Heft 2).—The author tells us that the object of his paper is to give a pathologico-anatomical analysis of certain preparations which he had obtained from three women in the course of several years, who were under his observation with the disease which is mentioned in the text. The disease is, properly, an exfoliative inflammation of the inner layers of tissue of the uterus, that is, there is a total removal, not only of the mucous membrane, but also of the con-

figuous bundles of muscular fibres. This disease is comparatively unknown, though the parallel process in the vagina has been observed and described frequently. The specimens came from the uteri in the form of a sac representing a complete cast of the uterine cavity, and consisting of mucous membrane with a considerable layer of muscular tissue. They were supposed, by some who examined them, to be the result either of an abortion or of membranous dysmenorrhea, but the author is confident, from microscopic examination, that they were not. In one of the cases, the uterus also was obtained after death, and thus afforded additional means for studying the disease. The literature of the disease, says the author, is confined to two cases which were reported in a Russian journal by Dr. Siromjatnikow, under the name *metritis dissecans*. [Dr. H. J. Garrigues alludes to these cases in a communication to the Obstetrical Society (N. Y.), and adds two cases of his own—see *New York Med. Jour.*, Dec., 1882, p. 587.] The specimens in the cases of Siromjatnikow differed from those of the author in the fact that the former contained only muscular tissue without any trace of mucous membrane. One of them was taken from a puerperal woman, with whom typhoid fever was present as a complication; the other was from a woman who was sick for five weeks with remitting fever. In two of the author's cases, no disease in addition to the one under discussion was present; in the third and fatal one, serious inflammation of the tubes and peritoneum, and hemorrhage into the peritoneal cavity, were found. In all of these three, there was evidence of inflammation of the tubes, but none whatever of tubal pregnancy or of pregnancy in any form, so that by no hypothesis could the tissue be considered as chorial or decidual. The process which gave rise to this exfoliation is considered, from the microscopical study of the specimens, to have been partly a phlegmonous and partly a hemorrhagic one. The study of these cases also makes it probable that in every uterus there is, in a greater or less degree of development, a submucous stratum underlying the mucous membrane. The author cites the studies of Williams upon the uteri of sheep and deer, in which he (Williams) asserts the existence of a muscular coat or stratum underlying the mucous membrane. If this same condition obtains in the human uterus, it is easy to understand the possibility of a separation of the uterine mucous membrane and its muscularis mucosa by means of extravasation of blood into the underlying tissue, viz., the submucosa, from the vessels which it contains, and which would thereby produce an endometritis dissecans. From his investigations, the author concludes that this disease is not so rare as has been supposed, but that it has frequently appeared under the names of dysmenorrhea membranacea, or an early abortion. The author then gives the symptoms in detail, which he thinks would characterize the disease, but which seem to us to be founded upon experience which is insufficient to warrant such a classification. The tubal complications—hydro- and hemato-salpinx—may or may have not been accidental.

A. F. C.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY . . . GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

CLINIC AT BELLEVUE HOSPITAL MEDICAL COLLEGE, APRIL
15TH.

BY

PROF. J. LEWIS SMITH.

Thrombosis of Umbilical Vein; Acute Diffuse Peritonitis.

THE specimens which I now exhibit were removed from the body of an infant that died at the age of fourteen days. Nothing unusual attended the birth of this infant, and the convalescence of the mother was in every way normal; weight at birth seven pounds six ounces. The cord dropped on the seventh day, and the surface of the umbilicus looked raw; on subsequent days until near the death of the infant a slight oozing of a purulent appearing substance occurred from the navel. When a few days old the baby was observed to be unusually fretful, and when I was called to see it, which was on the fourteenth day after its birth, and within about twelve hours of its death, the physician in attendance had already diagnosticated peritonitis. I was told that the infant weighed on the tenth day had been found to gain one ounce. Vomiting had not been a prominent symptom, and two stools of a yellowish appearance had occurred in the last twenty-four hours. The abdomen was much distended and tympanitic, and pressure upon it evidently increased the suffering and caused the infant to cry. The rectal temperature in the morning of my visit had been $100\frac{3}{8}^{\circ}$; in the preceding evening it was $102\frac{2}{3}^{\circ}$. The urine had been passed as in health. Death occurred on the morning of the fifteenth day.

Autopsy by Prof. Wm. H. Welch. On opening the abdominal cavity, which remained distended after death, six ounces of turbid serum escaped, containing many soft yellowish flocculi of lymph. Fibrin loosely attached lay upon the surface of the peritoneum, along the course of the umbilical vein, and upon the under surface of the liver, especially in its transverse fissures; no marked congestion of peritoneum. A number of lymphatic vessels filled with pus can be seen under the peritoneal covering of the diaphragm. A probe can be readily passed from the navel along the whole length of the umbilical vein. This vein is filled, from the navel to the transverse fissure of the liver, with a grayish-red thrombus, broken down, and of a purulent appearance in places. The thrombus does not extend into the ductus venosus nor into the portal vein; it is closely adherent to the inner surface of the umbilical vein.

Heart.—Along auricular surface of tricuspid and mitral valves, near the free edge, are black specks from hemorrhage into substance of valve; the right auricle filled with soft yellowish decolorized clots; foramen ovale nearly closed; the pericardial cavity contains more than the usual quantity of serum, in which flakes of fibrin float.

Lungs.—Posterior margins bluish and non-aërated; the bronchi contain brownish mucus; no foci of pneumonia.

Spleen rather large, and its upper margin covered with fibrin; its parenchyma dark; *kidneys* pale, and a few urates in the pyramids; many small ecchymoses in the mucous membrane of bladder, and a few upon the serous surface; yellowish turbid fluid in bladder; greenish turbid fluid in stomach. Intestines, uterus, ovaries, supra-renal capsules normal.

Remarks.—The cause of the peritonitis in this case was obvious. A probe could be readily passed from the umbilicus into and along the umbilical vein. In this vein, and extending the whole length of it, was a thrombus undergoing disintegration and decay, and furnishing the conditions of septic poisoning. It seems probable, from the putulous state of the vein, that air came in contact with the external extremity of the thrombus, thus allowing the admittance of minute organisms, the active agents in the putrefaction which followed. Another important vessel concerned in the fetal circulation, the ductus arteriosus, usually closes in like manner by the formation of a fibrin-

ous plug, but being in the interior of the body, remote from the air, the thrombus does no harm. It appears to soften, and be absorbed without producing any deleterious product. Usually, I think, when the umbilical cord is ligated, most of the blood flows out of the vein, it collapses and its walls contract, and hence the infrequency of septic poisoning from thrombi in this vessel. To the fact that the vein remained open and permeable to air, and contained a decaying thrombus in its entire length, we must attribute the death of the infant.

The deposit of fibrin was considerable upon that part of the peritoneum which covered the umbilical vein, and it seems probable that the peritonitis, which, when of septic origin, is attributed to bacteria or septic matter making its way along lymphatics of the peritoneal surface, commenced over the vein. The fact that lymphatics were discovered in the diaphragm under its peritoneal covering was interesting, since it is well known that they open upon the pleural surface. Had the infant lived longer it is scarcely to be doubted that pleuritis would have been added to the other lesions, since this infectious substance would have made its way to the pleural surface. The occurrence of pericarditis, which was probably late in the disease, showed that systemic infection through the medium of the blood was complete.

Lead Poisoning.

These two boys are brothers aged six and eight years. Their cases will be more fully reported when their urine has been examined. We will now cursorily state points of interest in their history. Their father is a painter, and he occupied small rooms in one of the tenement houses. In the latter part of 1882 he painted two of the rooms occupied by the family, and the paint tubs were allowed to stand in one of the rooms. In December both these boys had colic, referred to the umbilical region, continuing more than a week and wearing off gradually. After a respite for a few weeks the colic returned, but the diagnosis does not appear to have been made by the attending physician until the other characteristic symptoms, the drop wrist, appeared on both sides in both boys. The boys, improving but little at home, have been removed to Bellevue Hospital, where they can be more satisfactorily treated and more cer-

tainly secure from further lead contamination. It is stated that the blue line was noticed upon the margin of their gums, but it has now disappeared. Although the drop wrist was so marked that the power of extension was almost totally lost, the extensors have regained considerable power, so that the hands, with the exception of one, can be raised to the level of the forearms. The paralysis has not been confined to the extensors of the fore arms, but those of the legs were also involved. Even now, after considerable improvement, you observe that the boys walk slowly and unsteadily with a short and peculiar waddling step. Their countenance has the dusky pale color common in lead poisoning, and the left hand of the younger boy is, you observe, tremulous, which is usual in chronic plumbism. Constipation, another effect of lead, has also been present in these cases, especially during the times of colic.

Remarks.—Formerly lead poisoning, among painters and others, was much more common than at present, for the carbonate of lead, which is the usual contaminating agent, was ground by painters in its dry state when they prepared it for use, and the powder which they inhaled was very apt to poison their systems. Lead in those days was also very commonly found in the inferior wines, in rum from the West Indies, derived from the leaden worms of the stills, and even in cider which forty years ago was a common beverage in our Northern States. The cider became impregnated with lead salts from the vessels in which it was contained. I remember many years ago a physician who suffered severely from lead colic produced by cider of which he was very fond. At the present time, by greater precaution, by the knowledge which every one possesses of the danger of lead poisoning, on the part of those who are exposed to the salts of this metal, plumbism is not often observed except among painters, and nowadays their occupation, as we have said, involves much less risk than formerly.

It appears that some people are much more readily poisoned by lead than others. These boys were certainly very susceptible to it, or else there was some other and greater exposure to this agent than appears from the history. The blue line along the edges of the gums, first noticed and described by Dr. Burton, should always be looked for in cases of suspected lead poisoning, but it may not continue long, and it

is apt to disappear long before the paralyzed muscles regain their power, as in the cases before us. The blue color is supposed to be due to the sulphide of lead, produced by the metal coming in contact with sulphuretted hydrogen contained in the tartar. In a system saturated with lead a similar blue color is said to occur in the mucous membrane of the anus. Under treatment designed to eliminate the lead, the blue color has entirely disappeared, and the edges of the gums have their normal appearance.

The colic in lead poisoning is attributed to exaggerated peristaltic contraction of the intestines, and from the fact that the pain is usually referred to the umbilical region, we conclude that this exaggerated action occurs in segments of the small intestines. As in other forms of colic, the pain is not increased by pressure, but is rather relieved by it. That there should be this powerful and irregular contraction of the muscular fibres of the intestines is remarkable, since the effect of lead on voluntary muscles is to paralyze, instead of increasing their contractile action.

It is universally known that, as regards the voluntary muscles, the extensors are more affected than other muscles by the action of lead, and the extensors lying on the forearm are those which suffer most, becoming paralyzed so as to no longer raise the hand. Pathologists and neuropathists, for the most part, agree that this paralysis is not produced by the direct action of lead on the muscular fibres, but to its action on the nerves which supply the muscles. It is the musculo-spiral, arising from the brachial plexus and distributed to the extensor communis digitorum, extensor indicis, extensor minimi digiti and their associate muscles, which first of all is paralyzed in plumbism, and hence the "drop wrist" which on both sides and in both these boys so soon followed the colic. In severe cases other muscles, most frequently extensors, as the deltoid, are apt to be affected. In the cases before us, as we have seen, muscles in the lower extremities have been notably weakened, so that their gait is still slow and unsteady.

In the treatment of plumbism such remedies should at first be employed as tend to eliminate lead from the system, or render it inert. These boys have been taking iodide of potassium, which, according to M. Melsens, forms a soluble double salt

with the lead, and apparently a considerable part of the lead has been eliminated from their system by this agent. Muscles affected with this, as with other forms of paralysis, soon begin to waste, but as they are recovering their function in these boys, their normal development and rotundity will probably be restored. If there be not progressive improvement as regards the paralysis, galvanism must soon be employed.

CORRESPONDENCE.

TO THE EDITOR OF THE CHILDREN'S DEPARTMENT OF AMERICAN JOURNAL OF OBSTETRICS.

DEAR SIR:—Previous to the perusal of Dr. Clark's article upon Subcutaneous Injection of Morphia in Infantile Convulsions, in the April number of your valuable JOURNAL, I had been laboring for some time under the erroneous impression that the practice referred to was widespread and generally accepted by the profession. It has been in use by me constantly since 1868, and I have also witnessed its successful employment in the practice of my associates. I think that I can safely say that my experience extends over several hundred cases of infants and children, ranging in age from three weeks and upwards, as well as in puerperal convulsions. The results of my experience at that time, together with a theory of the nature and relations of convulsions, and the application of opium as a remedy, were published by me in the June number of the *Canada Medical Record* of 1876, and I think in the *Canada Medical and Surgical Journal* about the same time.

I was accredited at that time with the introduction of this method of treating convulsions, but later I found an old practitioner who had employed opium in puerperal convulsions for upwards of forty years, and recently, in perusing the excellent writing of Dr. Cullen, I was very happy to find the principles distinctly enunciated, so that I became aware that our ancient fathers conducted their practice upon rational deductions. We are glad to come to the support of Dr. Clark in his practice, but would advance a caution to those who might be disposed to follow blindly in an empirical manner without the guidance at least of a rational theory. We would be pleased to read the rationale from Dr. Clark's pen, by which he has arrived at the conclusion

that morphia is sufficient, and the remedy *par excellence* in convulsions. A little friendly criticism will doubtless prove useful by bringing out ideas, and by exciting an interest among our medical brethren upon an important subject. It is better, perhaps, to place the question of originality with mental development in general, since it is a matter of common observation that all reflective individuals with the same means of observation generally arrive at the same conclusions, and it ought to be acknowledged by the medical profession that its members are actuated by higher motives than a mere desire for distinction.

I think that I have noticed that morphia is of little use in those convulsions which are attended with a contracted pupil during the fit, or where there is an excessively high temperature. I am persuaded that its use is positively dangerous in those cases where "the tendency to death" points primarily to a failure of the respiratory function, or in a condition where the capillary circulation is sluggish in a very marked degree.

A short report of a case of eclampsia occurring in my practice on Tuesday last, the 7th inst., will serve as an illustration.

A primipara, of slight build, aged eighteen years, pregnant eight months, was taken with a fit at 6.30 A.M., and had three attacks before my arrival, which was about 9 A.M. I found her in a semi-conscious condition with a full and rapid pulse, and good capillary circulation, but her breathing attracted my attention, being characterized by a prolonged expiration with a wavy motion of the chest and a tough mucus râle in the trachea. She had another fit shortly after my arrival, from which she soon recovered into the condition above described. I gave $\frac{1}{3}$ gr. morphia by hypodermic injection. There was another fit in about twenty minutes, when I repeated the dose with the addition of gtts. iij. of fluid ext. of veratrum viride. Chloroform was administered during the convulsions. The interval was prolonged for an hour, but the respiration becoming manifestly more alarming in character, I concluded to deliver as soon as possible.

The only sign of labor present was a moderately increased amount of vaginal mucus. The os uteri resisted the introduction of the finger, and its margin was thick and rigid. The entire uterus was in a state of chronic spasm apparently relaxing at no time since my arrival.

Dilatation was practised by the hand, forcibly during the convulsions and the deep insensibility immediately following them, and gently in the intervals, during which she offered considerable resistance. There was a little highly albuminous urine secreted.

At 2 P.M., the os uteri was sufficiently dilated to permit the introduction of the hand, the membranes having been previously separated from the uterine wall as far as the finger could reach. Regular pains had been in operation for about an hour. The

membranes were now ruptured and the forceps applied, when, by gradual tension and by dilating the os upon the head, I succeeded in completing delivery about 2.30 p.m., immediately before which and shortly after which she had a convulsion.

I had omitted the morphia after the second dose, as well as the chloroform, but continued the veratrum viride in two-drop doses every hour, given hypodermically. She had seven spasms before delivery and four afterwards.

Her circulation remained good, but her respiration became more and more alarming after each attack. I learned that she had been troubled for some time with bronchitis, which indicated that the nerve-centres of respiration were previously in a weakened condition, and was the cause of their giving out in the present emergency.

At 4 p.m. it became a question of choice between allowing my patient to smother slowly but surely under the oppression of an accumulation of viscid phlegm, and the risk of immediately drowning her by rapidly loosening it up. I choose the latter alternative, and administered fl. ext. jaborandi 3 i. in a half-pint of warm water per rectum. Perspiration was shortly induced, together with a profuse frothy bronchial secretion which was blown from the mouth and nostrils, and forced in and out with each act of respiration, breathing being performed with great difficulty.

The pulse became irregular, slow, and intermitting, and the extremities got cold and bluish. The tongue was seized with a forceps and drawn forward sufficiently to free the throat, and with a cloth drawn over a stick I wiped out what phlegm I could. The hand was occasionally pressed over her mouth so as to produce a slight strangulation, when she would clear the back of the throat by blowing the mucus from the nostrils, which were continually wiped by an assistant. I found also that I could produce a little cough by forcibly drawing upon the tongue.

In an hour or two the crisis was passed, the air-passages being comparatively free and the breathing good, only that it was necessary to support her tongue for some little time afterward. The next morning she was somewhat conscious of her surroundings for short periods when aroused, and she has gradually improved so as to be out of danger at the writing of this report, April 12th. Until to-day her pulse has ranged from 120 to 144, while her temperature has scarcely reached 100° at any time, which shows how near also her circulation was to giving out as well.

I am afraid, in the same degree, to give morphia where there is a failure of the respiration as I am to give jaborandi where there is a tendency to a sinking circulation.

I would not be understood that morphia is contra-indicated in stertorous breathing, but where there is a manifest debility or obstruction of that function continuing throughout the interval. Nor not always in this case, but only that a degree of caution is

necessary, lest too great an insensibility of the respiratory centres be induced.

Tracheotomy might have been of great assistance in the above case, as it would have economized her strength by relieving the obstruction produced by the tongue and soft palate, and would have facilitated the removal of mucus. WM. FULLER, M.D.

GRAND RAPIDS, MICH.

ABSTRACTS.

Prepared by J. FEWSMITH, JR., M.D., Newark, N. J.

1. Politzer: The Value of Single Symptoms in the Diagnosis of Many Diseases of Childhood (*Jahrbuch*, XXI. B. 1 u. 2 H.).—The opinions of a man who has had the experience of forty years' practice, and who is at the head of the Vienna children's institute, are certainly worthy of consideration. Prof. Dr. L. M. Politzer arranges a series of symptoms of disease which, even in their individuality, their occurring singly, in many cases settle the diagnosis and in many others lead the physician on the right path toward it. He recognizes the importance of examining all symptoms present in any case, but believes there are certain ones to which sufficient weight has not been given when occurring singly. It is the object of his paper to study and ponder the weight which should be given to symptoms of this kind. The symptoms are not to be considered as pathognomonic, but they at least lead us medium in rem and greatly shorten a long examination. Without further introduction the author considers first a few symptoms which make his position especially clear. These are :

I.

The symptom of a strongly marked nasal or palate sound in the child's cry. This symptom at first glance may not seem in its individuality to mean much, but in many cases it may lead us to early diagnosis of *retropharyngeal abscess*. This disease is not rare between five and six years, but is very often overlooked, especially in the first few days of its existence. Other symptoms of it may all be occasioned by other diseases, and inspection of the throat may not discover it. This nasal tone should *always* lead us to palpate with the finger in the throat, the only sure way to discover a retropharyngeal abscess. The cry is not pathognomonic, but P. narrates some interesting cases where he made the diagnosis from it.

II.

A long-drawn, ten to fifteen times lengthened, loud, sounding expiration, with normal inspiration and no dyspnea. This symptom, even when alone, is sufficient for the diagnosis of *chorea major*. Cases are detailed where for days this was the only symptom of a commencing attack, and some in which it was almost the only symptom of the whole disease. It takes place without any dyspnea, with perfectly quiet movements of the diaphragm, and no effort of the accessory muscles. There is another analogous mode of respiration which is acknowledged by most physicians as sufficient for the diagnosis of *chorea major*. This is also a long expiration, but more bellowing, more like a protracted cough sound. This has

been called chorea laryngitis and sometimes classed with hysteria or considered as simulation. The author very strongly combats the theory that chorea major has anything in common with hysteria, brings up several cases in which he proves that *isolated* motor psychological disturbances were really to be classed under this name, and shows how in these same cases the symptoms would *always* yield to a few large doses of quinine. This is rather a hobby of the Professor's, but he is certainly right in trying to define a little more closely the term chorea major. The clinical symptoms of it, however, are well known, and these two forms of expiration are, even as isolated symptoms, sufficient for its diagnosis. They must, however, have the characteristic type. This may be either that the symptom, during several weeks, occurs every day at the same hour or about the same hour, or that, if it comes on at various times, yet at each occurrence there is a clock-like regularity of time between the single characteristic bellowing expiratory sounds, so that they may be timed by the watch, every seven, eight, or ten seconds. Such cases may last for months without other symptoms accompanying. They will, however, quickly yield to quinine. The author cites cases of the occurrence of other isolated symptoms as the only sign of the disease. These also yielded to quinine.

III.

A high thoracic, continually sighing inspiration. This is one of the more important "single" symptoms mentioned, and one to which certainly not enough attention has been paid. More than any of the others it has a positive diagnostic worth. It shows the commencement of *cardiac weakness, cardiac paralysis*, and in certain cases fatty degeneration of the heart. It is not a stenotic respiration (as in croup, etc.), shown by forcible action of the diaphragm principally, but high, thoracic, the upper part of the thorax doing the work, and instead of the stenotic sound we have the sighing or groaning. Long before other symptoms of cardiac weakness occur, such as cyanosis, coldness of extremities, etc., this sudden change in the respiration may be a guide-post to what is coming. The author goes into an elaborate explanation of the cause of it, which we may omit, but whenever, either in health or *in the course of any disease*, we see this high thoracic respiration, either commencing suddenly or gradually increasing, we may, without waiting for any other symptom, even if all other symptoms seem favorable, make the diagnosis of cardiac weakness and probably commencing cardiac paralysis. Occurring in health, the author believes it very frequently to be the first and best symptom of primary acute cardiac fatty degeneration. The symptom is equally valuable in cases where the cardiac weakness has been caused by drugs.

IV.

Strongly marked diaphragmatic expiration, accompanied by a fine, high, whistling sound. This single symptom is almost the opposite of the last. It points to *asthma bronchialis*. The author guards himself here. The respiration of croup or even of capillary bronchitis may greatly resemble this, but the point is that when we hear it we should always incline to the diagnosis of asthma, and if other symptoms bear us out, we shall not have to give so bad a prognosis as otherwise. The author's differential points may be omitted.

V.

Pauses between the end of expiration and the beginning of inspiration. This is a valuable single symptom to differentiate between *catarrhal laryngitis* with submucous edema and catarrhal, spastic stenosis, and *true croup*. As long as the pause is clear and distinct the croup may be excluded. This is an important aid to diagnosis. In true croup there is such an urgent need of oxygen that the powerful inspiration follows precipitously on the scarcely ended expiration. The younger the children the surer is this sign. But the ear must be placed close to the child's mouth to make sure that there really is an appreciable pause. Cases are given in which the author advised against tracheotomy in apparently fatal cases and yet the patients recovered, and this, too, without the expulsion of any membrane.

VI.

A habitual, noisy, interrupted, bleating expiration, the so-called *respiratio stridula*. The principal thing to know about this is that it is a symptom which always is single, that is, it points to no other disease and may exist for years without further developments. It is probably entirely a local, nervous phenomenon.

VII.

A class of single symptoms which are of worth in the diagnosis of brain diseases. These may be classed under three heads :

1. *Sleepiness*, lasting some time and occurring without fever or other apparent disturbance to account for it. The first question is, has the child fever? If it has, this is enough to account for the sleepiness, though even here we must inquire whether the latter is not out of proportion to the former, and if so, we must look out for cerebral trouble. But when we find sleepiness for twenty-four to thirty-six hours absolutely without fever, we may regard it as a pathognomonic, initial symptom of idiopathic cerebral disease. This is almost as sure when the sleepiness comes on without fever or in the stage of defervescence in the course of any disease. In such a case we think at once of basilar meningitis. The latter very frequently occurs in the course of other diseases, and there is no single symptom on which its commencement may be so surely predicated as this feverless sleepiness; not even the vomiting, the long continued headache, the slow, irregular pulse. Any of these may occur alone and yet disappear. Of course, if all occur together the case is clear. But the sleepiness, even alone, is sufficient for a diagnosis. There are only two other things which could cause it, narcotics or uremia. In some very rare case the sleepiness might point to an acute hyperemia instead of a meningitis, but this is so rare that its existence is doubtful.

2. *A prominent, firm fontanelle, not disappearing on pressure*. This means increase of the quantity of contents of the cranium, *exudation* of some sort. It cannot be caused by fulness of the vessels alone. In some acute hyperemias there is a prominence of the fontanelle, but it is not firm and resisting as here described. This means exudation, whether from causes within or disease without the cranium. The symptom may be valuable in the following cases :

(a) Its *absence* in the delirium, sopor, and convulsions of violent fever will show us that the causes of the symptoms are extra-cranial.

(b) In intermittens and fevers with high temperatures we can deter-

mine by its presence or absence the existence or non-existence of cerebral complication.

(c) In pneumonia, when we have sopor, convulsions, and even contraction of the neck muscles, a soft fontanelle will show us that there is still no exudation and in a few days all these symptoms pass away.

And so in the beginning and course of typhoid, in nephritis and in other diseases, the condition of the fontanelle is a valuable guide. When we find this firm, resisting prominent fontanelle we may know that we have an autonomous cerebral disease with exudation. When the fontanelle is soft, whatever the symptoms, we may exclude such trouble.

When the fontanelle, in addition to being enormously distended, is also entirely free from pulsation, it can mean but one thing, infantile apoplexy, intermeningeal hemorrhage. We all know how, when *deeply sunken*, it points to loss of blood or other nutritive juices, as in cholera, enteritis, etc. When the fontanelle is deeply sunken and the eyes in the same condition, the diagnosis of loss of fluid is evident at the first glance.

3. Very slow movement of the head and a staring look in the wide open eyes. This is one of the early symptoms of basilar meningitis, though not so valuable as the sleepiness already spoken of.

VIII.

A series of single symptoms depending on the *variety of crying*.

(a) A sharp, shrill cry, accompanied with an expression of fright or great anxiety, and occurring typically about an hour after the child has fallen asleep, is a single symptom in the strictest sense of the word. It is in fact the only symptom of the "alp" night terrors, the sudden awakening from bad dreams. The author recommends quinine.

(b) Periodical crying, lasting from five to ten minutes, sometimes occurring day and night, but oftener only at night, should always make us think of spasm of the bladder, or painful urination. Of course, other symptoms must be sought for to confirm this opinion.

(c) Violent crying at stool, with fear of the act and avoidance of it, points to fissura ani, generally accompanying constipation. The sphincter ani should be examined and, if necessary, a salve of zinc and belladonna applied.

(d) A violent cry, full of pain and almost continuous, with a throwing about of the head in the pillow and grasping it with the hands, means earache or otitis.

(e) Crying continued for weeks, increased on touch of the extremities, accompanied by fever and incessant sweating, may be a symptom of acute general rachitis.

(f) The crying of habitual agrypnia may point to numerous causes.

Other kinds of crying, as the so-called hydrocephalic, etc., are passed over by the author as not belonging in his article or of no value.

IX.

Some single symptoms, often overlooked, but very important:

(a) The peculiar physiognomy of children with hereditary syphilis. Once seen this is not likely to be overlooked in a second case.

(b) A peculiar falling together of the *alæ nasi*, with complete immobility during inspiration. This means chronic enlargement—hypertro-

phy—of the tonsils and is generally accompanied by snoring at night and a narrowness of the chest amounting in some cases even to pectus carinatum.

(c) Weakness and immobility of a child after a comparatively slight or short sickness. This is an important finger-post to make us watch for spinal paralysis. We should accustom ourselves after any sickness to always notice whether the child is more weak than is to be expected from the amount of disease, and especially if it shows an aversion to movement. In this connection we must at the same time examine the child in regard to the next two points.

(d) Slight deafness, especially when it follows some apparently trivial disease. It is apt to point to some trouble at the base of the brain. In young children deaf-dumbness may commence thus.

(e) Depression of psychical activity. Overlooking this we may overlook the beginning of an idiotismus acquisitus and our best chance for treating it. It is easy, in noting the condition of a child after a sickness, to observe these three points, and we should make it a rule to do so in every case. We may, in regard to the latter, thus early discover congenital idiotismus. One of the best single symptoms of this is a continual, purposeless, automatic holding of the hands before the eyes and looking at them. No matter how often they are taken away, or other objects of interest substituted, they are again brought forward and studied by the child. This alone is enough to lead to a diagnosis of idiotismus.

(f) Abnormally delayed ossification of the cranial bones. This should never escape our systematic examination of the child. It is the earliest sign of rachitis and at this stage the disease yields well to treatment.

(g) An anxious, stiff attitude in walking, sitting, rising or bending, and, in children who are not able to walk, an expression of pain when picked up or laid down. This is sufficient almost for the diagnosis of a commencing spondylitis.

(h) Vomiting, of all kinds of food, continuing several weeks, in children with closed crania but of large cranial measurements. The absence of fever excluding any acute, idiopathic disease and the absence of pain excluding a cerebral tumor, we may diagnose that a chronic hydrocephalus exists and has become somewhat acute.

(i) An attack of eclampsia should always make us consider and determine by other symptoms if it is not the first sign of epilepsy.

The author cursorily mentions other single symptoms of slight importance and concludes by mentioning some which have been regarded as of more worth as single symptoms than they really deserve. The vomiting of undigested food, for instance, is taken as a sure sign of the fact that this food is the etiological corpus delicti of the "spoiled stomach," that from this comes the fever, the gastric fever, etc., while the true condition is one of fever, causing decrease of pepsin, indigestion and vomiting, and in the latter the undigested food happens to be thrown out. The passage of a worm is also considered sufficient explanation of many symptoms which have no connection whatever with worms and which are not caused by lumbrici, even in great number, nor even by tænia.

2. Heubner: Multiple, Infectious Inflammation of the Serous

Membranes (*Jahrb. f. Kindhkd.*, XXI. B., 1 u. 2 H.).—Within the last six years Prof. Heubner has seen five cases of the above. Analogous cases have been noted in the literature, but nothing definite stated about them. In 1880, Wiedemann reported two cases under the title "can lung plague (*lungenseuche*) occur in man?" These cases greatly resembled those now reported by the author.

The disease is a purulent inflammation of the serous membranes, always having a multiple localization. In all five cases, at least one pleural cavity was affected, while otherwise the localization varied greatly. In two cases there were involved both pleural cavities and the pericardium; in one, both pleural cavities and the peritoneum; in one, the left pleura, the pericardium, and the right ankle-joint, and in one, both pleuræ and probably the meninges. In spite of this multiple localization of purulent dépôts, there were none to be found in other tissues, so that the idea of ordinary pyemia may be excluded. In the pleural cavities the inflammation was always circumscribed and the pus encapsuled while the rest of the pleura was comparatively normal. In the pericardium and peritoneum this was not so. The pus in these pleural collections was thick and heavy, and they gave rather the impression of pleural abscesses than an ordinary purulent pleuritis. There was some affection of the lungs in all the cases, generally in the shape of bronchitis, but this was not a marked feature. In some cases there was compression of the posterior portions of the lungs with edema of the rest. In no case was there genuine (croupous) pneumonia.

The clinical course of the disease was as follows. The beginning was sudden. In only one case was there some malaise for a few days preceding. From the first day, the fever was high, pulse frequent, dyspnea, short respiration, pain in side, red cheeks, in short, all the signs of pneumonia. But day after day the physical examination failed to discover any pneumonia. Circumscribed areas of dulness might be found, but over them could be heard only friction sound. These spots of dulness become more clearly circumscribed as the case advances. Sometimes the fever loses the pneumonic type and shows all sorts of variations, intermissions or remissions, short or long—in short a type to be distinguished only by its irregularity. The symptoms become more alarming, dyspnea increases, the pulse rises enormously (200), the nights are sleepless, the younger patients become convulsive, the appetite is entirely lost, vomiting and diarrhea may set in, the urine becomes scanty, then follows collapse and death. In three of the cases the course was rapid (seven to eight days). In the other two, the youngest, it lasted five weeks, a discrepancy which the author explains by the more rapid or later involvement of the pericardium and perhaps the meninges.

What is the etiology of this peculiar disease? In the last three cases most careful search was made for any primary purulent deposit, but none was found. In the last two cases the author undertook a strict histological examination of both the affected tissues and the kidneys, blood and lymph vessels. The indisputable result was the finding of a very large amount of bacteria, not only in the affected tissues and the purulent and fibrinous deposits, but also in the veins of the lungs and kidneys, in the form of closely packed bacteria-colonies, and also separately. Without going into the author's accurate description of these bacteria, suffice it to say that they are of the variety known as round, or micrococci, or

"microbe en huit." There were not found in the bronchial or alveolar spaces. These examinations were made twenty-four hours after death, and the author acknowledges that the number of bacteria and their collection into colonies must have greatly increased in this time, but that their presence was not wholly a post-mortem fact his immense experience in post-mortems without his ever having found them in other cases is sufficient proof.

From careful study of the comparative quantity of bacteria in the lymph-vessels and the blood, the author comes to the conclusion that "the original starting-point of this peculiar disease is in the serous membranes and especially in the pleura, that it is here at first a local disease somewhat analogous to erysipelas, that it extends per contiguitatem and only affects the general circulation after a varying time." In the peritoneal inflammation the start was at the diaphragm, opposite the pleural deposit.

Our first thought, in a case of multiple purulent deposits of this kind, due to micro-organisms, is of pyæmia. Opposed to this idea is the location of the disease in one certain set of organs and the absence of any entry-port for the virus. If there was any point of inoculation the disease would greatly resemble the experimentally discovered disease called by Koch rabbit-pyæmia. By the injection of the septic fluid he in each case caused purulent peritonitis, and the deposits contained the same form of micrococci as the author found in his cases. Is there any possible entry-port? One point may be noticed. All the cases reported (with one exception) have occurred in children under two years. Wiedenmann has endeavored to show that the disease entered the system in milk drawn from cows suffering from lungenseuche, the plague, cattle pest. The author's cases do not fully substantiate this view, nor do they fully contradict it. The interesting question must be left open for further experiment and research.

3. A. Steffen: The Action of Kairin (*Jahrbch. f. Kindhikde.*, XXI. B., 1 u. 2 H.).—Kairin has been recommended as an antipyretic by Filehne, Hallopeau, Guttmann, Kohn, Riegel, Freymuth, Merkel, and others. In the *Philadelphia Med. News*, No. 584, Dr. James K. Crook publishes an article in which he gives an account of the drug and its effects on adults. The alkaloid quinine contains a large proportion of hydrogen, and recent investigations have shown that it exists in the quinine molecule in combination with quinoline. Chemists have experimented in hope of finding a body similar to quinine by a synthesis of its elements. By a variety of combinations, beginning with hydrated quinoline, they have succeeded in producing "kairin," of which the hydrochloride is the preparation offered for use. It occurs in the form of a crystalline powder of a bright grayish-yellow color. It is readily soluble in water, and has a bitter, saline, somewhat aromatic taste, which, however, is very transient. Dr. W. Fischer, of Berlin, first used it. Girat has shown that the toxic dose is from one to two grains per pound of body weight of the animal. The symptoms of an overdose are those of heart failure. The reports of its effect upon adults have varied very greatly, some regarding it as an efficient, prompt, and safe antipyretic, while others have found its action in this direction to be uncertain or very transitory, while unpleasant or even dangerous symptoms ac-

accompanied its use. In the United States, it has been recommended by G. B. Shattuck, of Boston, slightly commended by F. W. Draper, of New York, and somewhat more strongly by T. A. McBride. Dr. Crook's conclusions were as follows, in adults:

1. It is a decided febrifuge, rapid, though somewhat fugacious in its action.

2. It diminishes the frequency of the heart's action to some extent, though the pulse rate does not fall *pari passu* with the temperature.

3. The symptoms of collapse may be entirely or to a large extent avoided by close attention and the proper use of stimulants.

4. It is a tolerably constant diaphoretic.

5. Its action in intermittents, though not fully tested, warrants the belief that it possesses valuable antiperiodic properties, and as such should be carefully tested.

6. Though kairin itself may not be found to possess all the qualities of the alkaloid quinia, enough has been learned of its action to justify the hope that a perfect substitute may yet be found, and to inspire us with renewed confidence in the resources of organic chemistry.

During the past year, Steffen has been making careful experiments with it in his children's hospital. It was given to six children, from four to eleven years old—three cases of typhoid, two of pneumonia, and one of scarlatina. A dose was given whenever the temperature went above 39° C. Tables of temperature and pulse of all cases are given. The action of the drug varied according to the disease and the individual. In the typhoid cases, it caused a lowering of the temperature after each dose. In the pneumonia, this was not so constant, and there was sometimes a rise before the fall. Collapse sometimes threatened, but was guarded against by stimulants, etc. The pulse and respiration varied usually with the temperature. The dose given was from 0.2 to 0.5 gm. The urine was dark or greenish some hours after the remedy was given.

Steffen's conclusions are that kairin is an antipyretic of about equal value with hydrochinon, and worthy of further trial. The necessity of more frequent doses, the slowness of its action, and its very high price are disadvantages which at present make the author prefer the hydrochinon described by him some years ago.

In the *Amer. Jour. of Med. Sciences* for April, there is an abstract of an article by Dr. Gueirolo (*Gazz. Degli Ospitali*) on the hypodermic use of kairin. His solution was from grs. iss. to grs. viij. in ℥ xvi. of warm water, as the drug is not soluble to this extent in cold. No general or local accidents were caused. The smaller dose produced only a slight and temporary lowering of temperature. Injection of grs. iij. produced a maximum depression of .7° C., beginning in about half an hour, and lasting two hours. From grs. v. the depression was 1.5° C., lasting about the same time. Grs. viij. produced a depression of 1° to 2.4° C., beginning soon and lasting two or three hours. When grs. xv. are injected, the temperature is lowered from 2.5° to 5° C., and this lasts about five hours. Gueirolo concludes that when kairin is used hypodermically, the effect is quicker and greater, and that smaller doses are necessary.

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ORIGINAL COMMUNICATIONS.

THE POSSIBLE DANGERS, IMMEDIATE AND REMOTE, OF
TRACHELORRHAPHY.

BY

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IN venturing to discuss so well-worn a topic as laceration of the cervix uteri, I am well aware that every point relating to the causation and significance of the lesion, to the distressing symptoms which it produces, to the indications for and to the technique of the operation for its cure, has been dwelt upon in print and in debate by so many authorities, great and small, that the subject has almost become the bugbear of gynecological readers and of gynecological audiences.

Nevertheless, it must have occurred to every one who has looked over the literature of the subject, that there is a uniform and singular absence of information concerning the possible *dangers* which may accompany, and *unfavorable sequelæ* which may follow this operation, scarcely a writer having even touched upon the subject and no one having investigated it in anything like a complete manner.

At the suggestion of my preceptor, Dr. Paul F. Mundé, I have endeavored to follow out this line of inquiry and to present in a condensed form the results and conclusions which may

be arrived at from a careful study of the subject, both from published statistics, from private sources, and from what I have observed in some fifty odd operations in which I have assisted my preceptor.

It is true that trachelorrhaphy is, considering the parts concerned, singularly free from danger, yet it occasionally gives rise to lesions which severely tax the vital powers of the individual and may even cause death. When speaking to one of our highest authorities in gynecological matters, a short time since, he said, "in this operation *good* operators never meet any dangers, the bungler finds all the complications."

While this remark may be in the main true, it is much too sweeping in its latter assertion, for those who have had the courage to report their "complications" are assuredly neither tyros nor "bunglers."

Before proceeding with the subject proper, I trust that I may be pardoned if I turn for a moment to review a few points in the history of the operation and the numerical importance of the lesion.

Though for more than two centuries the thinking minds in the profession had recognized, more or less clearly, the dependence of the then called "ulcerations" upon previous laceration of the cervix, no one seems to have realized fully the feasibility of curing the lesion by closing the laceration, and the honor of inaugurating a new era in the treatment of these fissures was reserved for the clear-seeing genius of Emmet, who recognized and showed, as had no one before, the real nature and importance of this frequent "ulceration" of the parous cervix, and further originated and, on November 27th, 1862, executed for the first time, the operation for its cure.

Emmet's views and his operation were not made known to the world at large for several years, but when published they at once gained many disciples among the progressive men in the profession, though, as is always the case with an advance which deviates widely from the beaten track of tradition or custom, more scoffed at them as the visions of an enthusiast.

Though somewhat exaggerated, the real truth of his views and the palpable benefits of his operation steadily gained converts, and they were soon acknowledged throughout America,

though in the older and less progressive countries it was long before they were received at all, and, indeed, it is only within the last few years that, after oscillating between exaggeration and rejection, their truth and value have been placed at their proper level of appreciation by every enlightened country except France, which alone, as yet, seems to consider it unworthy of notice, there being not the slightest mention of the subject in the latest and best French text-book. (Courty, 1883.)

Of the operation itself nothing need be said; its steps are familiar to every student of medicine; in it, as in every other, gentleness, that manual dexterity that comes from frequent repetition, attention to what may seem to be but minor details, and despatch, are important elements of success.

In regard to the frequency of lacerations of sufficient importance to cause symptoms, authorities differ but slightly, though at first sight the statistics of different men seem to vary widely, one set ranging from 15.1 to 18.8 per cent, the other, from 29.6 to 35.1 per cent.

On examination, the difficulty vanishes, for we find that some gynecologists have taken the ratio of lacerations from the *whole* number of their patients, while others have included only those who had borne children. The following tables show how closely observers agree.

Table I.—Percentages of parous women lacerated, as given by various authorities.

In 500 parous women	Emmet	finds 164 lac. = 32.8%.
" 930 "	" Mundé	" 293 " = 31.5%.
" 456 "	" W. H. Baker	" 135 " = 29.6%.
" 396 "	" F. H. Davenport	" 139 " = 35.1%.
<hr/> 2,282		<hr/> 731 32.0 + %.
Average 32.0%.		

Table II.—Percentages of all women lacerated, as given by various authorities.

In 1560 women	Mundé	finds 293 lac. = 18.8%.
" 992 "	" W. H. Baker	" 135 " = 13.5%.
" 918 "	" F. H. Davenport	" 139 " = 15.1%.
<hr/> Total, 3,470		<hr/> 567 16.3%.
Average 16.3%.		

Though the figures given by these tables may seem at first sight to be approximately correct, a little thought will show that they exaggerate the frequency of the lesion to no slight extent, for in every case they have been deduced from results obtained by the examination, not of the whole number of those who have borne children, nor from the *totality* of a number of women during the child-bearing period, but from a selected proportion of that totality, namely, from *patients*. Certainly a large proportion of the women included in the above two divisions do not need to come to the office of the gynecologist, so that to get at the true proportion of lacerated cervixes we must search more deeply.

Dr. Mundé, whose opinion on this point is derived from wide experience, considers that of all cervical lacerations, one-half suffer no inconvenience at all, the injury becoming insignificant when involution is completed; one quarter give symptoms which may gradually disappear spontaneously or require palliative local treatment to reduce the injury to a dormant state; while of the remaining twenty-five per cent, one-half may be for a time relieved of their symptoms by appropriate treatment, but will eventually require the radical operation, the other twelve and a half per cent being absolutely incurable except by the operative closure of the laceration.

Most authorities agree that at the time of labor nearly every cervix sustains some laceration, slight or severe. Assuming that this proposition would include *eighty per cent* of all parous women we would have as the twelve and a half per cent of lacerations which absolutely require operative interference to restore them to a condition of health so far as the cervix uteri is concerned, *ten per cent of the whole number of fertile women*.

[According to the last census, there are in the United States 7,600,000 woman between the ages of twenty and forty years. Using the percentage deduced from the tables of Matthews Duncan (Fecundity, Fertility, and Sterility, page 13) regarding the number of fertile women in the total female population of Edinburgh and Glasgow between these ages, as a basis, and remembering that the fertility is greater where the density of the population is less; we find that *more than* 1,175,000 of these women have borne one or more children. Ten per cent of this number gives us 117,500 as the number of those in

whom trachelorrhaphy is indicated. Surely the coming operators in this line need not fear a lack of material !]

The possible dangers which await the operative closure of these lacerated cervixes naturally divide themselves into two groups ; *first*, of those which may occur during or immediately after the operation, and *second*, of those which present themselves months or years afterward and which may be called the remote dangers.

I. IMMEDIATE DANGERS.

1. *Primary Hemorrhage.*

This accident, naturally the first to demand our attention, though common to almost every surgical operation, presents in connection with this subject certain points of interest. Although it does not usually prove dangerous, or even troublesome, yet occasionally there will be an excessive and persistent parenchymatous bleeding from the denuded surfaces; and, occasionally, when the laceration is very deep, one of the branches of the circular artery may be wounded by the scissors while removing the dense cicatricial mass which in these cases is present at the deepest point of the laceration and the complete removal of which is necessary to insure the success of the operation. If the operator cuts away the tissue to an unnecessary depth, or if the artery be unusually superficial, he may wound it, even though the laceration be not very deep.

In the first case, the oozing may generally be controlled by the pressure of sponges wrung out from cold, or preferably from very hot water (120°–130° F.), or, this means failing, as it occasionally does, the sutures should be passed deeply through the parts already denuded and moderate traction made, which will control the bleeding until the paring is complete, when the twisting of the sutures will almost invariably cause a complete cessation of the hemorrhage. When oozing persists for several hours, we may be driven to the use of a chemical styptic; this, however, as we wish, if possible, to get primary union, should not be used before the wound has been closed, and may consist of a hot alum douche, or of a saturated solution of alum applied on a tampon.

When the circular artery is wounded, the hemorrhage is often profuse, and, as the tissues are dense and brittle, time is

merely wasted in trying to suppress the bleeding by pressure, torsion, etc. The proper method of procedure is to pass a wire suture deeply under the bleeding vessel, if possible in such a way that it can be utilized in the final closure of the wound, and either making traction upon its ends or twisting it tightly on one side of the cervix. Usually the suture nearest the upper angle of the rent answers this purpose perfectly.

2. *Secondary Hemorrhage.*

Through the courtesy of Dr. Emmet, I was enabled to procure the records of the following interesting case which lately occurred in his service at the Woman's Hospital, and which is especially valuable as showing the late period, in this case not until the *ninth* day, at which secondary hemorrhage may occur, and also the necessity for and the efficacy of the method finally adopted for its control, which was, I believe, first suggested by Dr. Emmet some years ago.

Mrs. M. C., aged thirty-two. Has been married fifteen years; has had four children, and no miscarriages; has been ill four years. After her last labor, two years ago, her convalescence was very slow. She has deep laceration of both cervix and perineum.

On *December 5th*, 1883, trachelorrhaphy was performed, eight sutures being introduced. On *Dec. 17th*, when the sutures were removed, union was found to be imperfect. On *Jan. 19th*, 1884, a second operation was done, five sutures being introduced on each side. On the *20th*, the patient had some tenderness over the abdomen, was given opiates, etc. On the *28th*, she had quite a profuse hemorrhage, which was temporarily arrested by the hot alum douche. The sutures were left in and alum tampons applied. Shortly afterwards there was a most profuse and exhausting hemorrhage; she was soon put on the table, tampon removed, hot alum douche given, and a pledget of alum cotton placed in the os. The anterior and posterior cervical walls were thoroughly compressed by packing cotton around them. She was sent to the ward, given liq. opii gtt. xx., and brandy $\bar{5}$ ss. External heat was applied, and the lower part of the bed elevated. The patient soon became insensible, was anemic and apparently dying, but rallied during the afternoon, and by night was fairly comfortable.

January 31st. The opium, stimulants, and soft diet continued, tampon removed, and hot douche given. There was no hemorrhage at this time, but soon after she bled profusely. A loop of silver wire was now passed, *high up, directly through the cervix, the ends being brought around and tightly twisted on the side from which the bleeding came*, the tampon was then replaced. *February 2d.* Tampon was removed and, as there was

no hemorrhage, was not replaced. *February 3d.* Hot douches ordered every day. *February 6th.* Patient doing well, temperature never having risen above 100°; pulse from 80 to 100, appetite good, general condition excellent.

In a somewhat similar case in the practice of Dr. Pallen, which Dr. Emmet saw in consultation, and in which the hemorrhage was very alarming, the same method of passing the loop of wire directly through the cervix high up (as one would pass the double ligature in tying off hemorrhoids, or in ligating the pedicle of an ovarian tumor) and twisting it tightly on the side from which the hemorrhage came was successful. Though one-half of the tissues of the cervix are included in the grasp of this deep suture, there is no danger of sloughing, for the vessels of the other side anastomose freely enough with those whose circulation has been cut off by the ligature to keep the parts nourished.

Dr. Goodell in the *AMER. JOUR. OF OBST.*, Vol. XV., page 122, reports a case of slight secondary hemorrhage, checked by vaginal douche of a saturated solution of alum.

Two cases have occurred in the practice of Dr. Mundé:

In one, the patient seemed almost of a hemorrhagic diathesis, for there was excessive oozing from the slightest cut or scratch at the time of the operation, the bleeding continuing even after the sutures had all been secured and necessitating the application of the alum tampon. No artery of appreciable size was divided, though a small one could be seen pulsating in the bottom of the wound. The sutures employed were of silver wire. The secondary hemorrhage appeared on the fifth day and was most alarming. On cleaning out the coagula from the vagina through the speculum, the bright arterial blood was seen to gush from the cervical canal, and doubtless came from the vessel referred to which was eroded by one of the sutures. The instruments for introducing a deep suture not being at hand, the vagina was tightly packed with flat alum tampons and the hemorrhage arrested. On the twelfth day the sutures were removed and union was found to be perfect (some months later, the same tendency to free hemorrhage was noticed when the patient's perineum was repaired).

In the second case, the laceration was very deep, and a small artery was divided. Catgut sutures were used because it was thought best in this case to repair the lacerated perineum at the same sitting.

The hemorrhage appeared, as in the first case, on the fifth day, and as the cervix could not be exposed or tampons applied without sacrificing the new perineum, the only means at disposal to arrest the bleeding were vaginal injections of water and vinegar.

These were first used hot, but only effected a temporary check, when equal parts of ice-water and vinegar permanently arrested the hemorrhage. The coagula were removed by the fingers several days subsequently, and complete union of cervix and perineum was found to have taken place.

These cases show most forcibly how serious a danger secondary hemorrhage may be and demonstrate the necessity of prompt and efficient action when it occurs. The last case may also, according to Dr. Mundé, be used as an argument against the routine practice of closing both cervix and perineum at the same time, for should severe secondary hemorrhage take place from the cervix after this double operation, it would inevitably necessitate the tearing open of the freshly united perineum, if astringent injections failed to check the bleeding.

3. *Too deep or too thorough Denudation.*

This is one of the dangers which the *good* operator scarcely ever sees, and one in avoiding which the tyro is apt too err in the opposite direction by not completely denuding the surfaces which he wishes to have unite, although he may, in his endeavor to completely excise the oftentimes much enlarged cervical glands ("ovula Nabothi") remove too much in depth of the cervical tissue. When this occurs, there is, in addition to the increased liability to hemorrhage, a difficulty in bringing the lips of the womb smoothly and accurately in apposition, the thinned cervical tissue having a tendency to wrinkle or pucker under the presence of the sutures, or to roll in upon itself in such a manner that when the operator has finished, he will find that he has the same result that would happen had he left too narrow a strip of undenuded tissue to form the cervical canal, namely, that he has a narrower os than he desires and one which may give trouble in the future.

4. *Sloughing of Cervical Tissue.*

In the endeavor to get perfect apposition, care should be taken that the sutures are not drawn too tight. When the cervix appears pallid, we know that this has occurred, and that unless we remedy the compression by lessening the tension, certain results will follow: either that there will be gangrene and sloughing of the constricted portion, with its possible sequelæ, or that the sutures will, as a result of the in-

evitable swelling, cut partly through the tissue, while at the same time there will be a sanguineous oozing, caused by the congestion, from the lips of the wound ; any one of these factors may effectually prevent union, and thus defeat the object of the operation.

5. *Menstruation immediately following the Operation.*

This complication having only one significance, will be discussed under the following head.

6. *Non-Union.*

Table III.—Cases in which Non-Union occurred to such a degree that a Secondary Operation became necessary.

Reporter.	Where Reported.	No. of Cases.	No. where secondary operation was req'd.	Remarks.
Mundé.....	Not before reported.	137	10	
Goodeil.....	Am. J. of Obs., xv., 121.	113	6	
Schenck.....	St. Louis C. of Med., March, 1881.	110	2	3 others not perfect.
Warren.....	Bost. M. and S. Jr., Aug. 20, 1881.	83	10	
Ill.....	Trs. Med. So. of N.Y. 1882, p. 157.	44	3	One case required third operation.
Davenport...	Bost. M. and S. Jr., 1882, p. 488.	37	4	In 7 out of 12 cases where results were not perfect, menst. came on shortly after the operation.
M. A. Smith..	Bost. M. and S. Jr., April, 1883.	37	2	In 3 cases which failed to unite, menstruat'n occurred soon after operation.
Howitz.....	Am. J. of Ob., xiii., p. 677.	26	2	
Montgomery..	Obs. Gaz. of Cin., 8-13, 1882.	20	2	
Macdonald...	Edinb. Med. Jour., July, 1882.	9	1	
Boulton.....	British Med. Jour., 1882, p. 1205.	4	1	
Mann.....	Proc. Conn. Med. So. 1881, p. 165.	17	1	
Total.....		637	44= 6.90%	

An analysis of the above 637 operations would seem to show that *non-union*, to such an extent that a secondary operation may be required, occurs in about *eight* per cent of all opera-

tions; the exceptional results obtained in St. Louis, however, lower the average to *seven* per cent.

In most of the cases the supposed cause of the failure is not given; in ten, where the union was not perfect, the advent of menstruation shortly after the operation is mentioned as the probable reason of the non-union, but I consider that this condition may very properly be set aside from the causes of failure, it having at most only very slight influence in this direction.

In a number of cases in the practice of Dr. Mundé and others, some of which I have seen, where menstruation came on unexpectedly before the sutures were removed, the result was not in any way influenced by the complication. The sutures were merely allowed to remain in situ a little longer until the flow had ceased, and when removed union was found to be perfect.

Strange as it may seem, inflammatory conditions of the surrounding tissue do not seem to affect union, that having been good in some of the most severe cases where this complication existed.

For reasons to which all plastic operations are susceptible, non-union is more liable to occur in hospitals than in private practice. Many of the cases in the table belong to this class, and in the ten of Dr. Mundé, more than one-half occurred in hospital, though the number of his operations there was much smaller than in his private practice.

This result is especially apt to follow where, in cases which have been insufficiently prepared, there is a flabby and hyperemic condition of the cervix, often with much parenchymatous hemorrhage at the time of the operation, this being followed by persistent bloody oozing, so that, though the sutures may be left in longer than usual, there is no adhesion, and the wound again gapes when they are removed. *Too tight* or crossed sutures, by constricting the tissues, or too great a number of sutures, by producing irritation and suppuration, will also contribute to this result, and it may occur even where every precaution has been faithfully carried out and where everything has promised well.

II. INFLAMMATORY SEQUENCES.

Table IV.—*Circum-uterine Inflammation following the Operation.*

Reporter.	Where Reported.	No. of Cases.	Variety of Inflammation and result.
Howitz	AM. J. of OB., xiii., p. 677.	1 case	Parametritis, ascribed to infection.
Hunter	AM. J. OF OB., xiv., p. 895; and in Dr. Johnson's paper.	4 cas's	One died on tenth day exhausted. Distinct evidence of peritonitis on second day. No autopsy. Three cases had severe cellulitis.
Mann	3 cas's	of severe cellulitis. In one case the patient got out of bed, assisted in moving furniture, and tore out stitches.
Goodell	AM. J. OF OB., xv., p. 121; and in Dr. Johnson's paper.	7 cas's	four of peri- and parametritis. Two cases light and two severe. Due to hospitalism. Patients recovered, but in one convalescence was delayed by the formation of 2 abscesses in the leg; in the other erysipelas occurred on face and trunk. Union was perfect; stitches remaining in much longer than usual. In three cases death occurred.
Lyman.....	Boston M. and S. J., April, 1880.	1 case	Severe peritonitis and cystitis. Temp. on 3d day 103.4°. Discharged 74 days after oper'n cured. Good union.
Emmet	1 case	Pelvic cellulitis. Sutures not being removed for 5 weeks. Good union. Dr. Emmet has had several other cases of cellulitis.
M. A. Smith .	Bost. M. and S. Jr., April, 1883.	1 case	Cellulitis (previous cellulitis) treated for ten weeks.
Moses	St. Louis Cour. of M. Vol. ix., No. 6.	1 case	There was tenderness at the time of operation. Uterus was moved though not to a very great extent. A violent cellul'is followed which threatened the patient's life for some days. Union was perfect.
Mundé.....	4 cas's	Two of fatal septic perito's, both in hospital. In the first, cervix was perfectly healed. No suppuration. Total prolapsus, therefore no traction whatever during operation. In the second, peritonitis occurred on the 3d day, death on the 8th day. Autopsy in neither case showed the slightest connection betw'n wound and peritoneum.

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Reporter.	Where Reported.	No. of Cases.	Variety of Inflammation and result.
Mundé.....		One of very severe peritonitis and cellulitis. Patient had had cellulitis four years before, but it had entirely disappeared. One of severe cellulitis (consultation) followed by pelvic abscess.
Jackson....	Reported in a paper read by Dr. Johnson before Amer. Med. Association, May, 1884. Am. Med. Assoc., Washington, 1884.	3 cas's	Cellulitis.
Lusk.....		A few	cases cellulitis.
Scott.....		Sev'l	" "
Lee.....		" "	" "
T.J. Johnson		2 cas's	1 of cellulitis, & 1 of periton's.
Reamy.....		6 cas's	" In six out of two hundred and thirty-one cases the operation was followed by perimetritis parametritis, or peritonitis. In but three of these cases, however, were the symptoms sufficiently severe to cause material delay in complete recovery. In one case, included in these three, there were perimetritis, parametritis, and general peritonitis. The patient was confined to her bed for three months. During the attack (acute stage) the peritoneal cavity was aspirated twice. On one occasion, seventeen ounces of serum were removed. This woman finally recovered her health completely. Considerable immobility of the uterus remained for a long time; but now, at the expiration of two years and a half, its mobility is about normal, and menstruation normal and painless; whereas, prior to the operation, she suffered both from menorrhagia and dysmenorrhea."
Totals....	Whole No. cases.....	43 +	No. of cases of cellulitis ... 34
	No. deaths.....	6	" " peritonitis.. 9

Inflammation may be kindled in the connective tissue and peritoneum of the uterus and its adnexa by roughness of manipulation, by too great traction in drawing the uterus downward, by the stretching or rupture of ligamentous legacies of previous inflammation, or, where such inflammation still smoulders, though but slightly, it may be fanned to a blaze by

the most careful and gentle of operative procedure. Zymosis must be blamed as the cause of some of our most severe cases. Finally inflammation is often set up by indiscretion on the part of the patient, who may, as in the case reported by Dr. Mann, get up too soon, or in some other way disobey instructions.

Of these causes, the first suggests its own remedy; the second, the presence of adhesions, cannot always be avoided or foreseen. The history of a previous cellulitis, or even the flat exudations of the chronic form, does not absolutely contraindicate the operation, though the patient will need very careful preparation, and besides the ordinary treatment by means of tincture of iodine painted over the vaginal vault, hot water injections, and so on, the parts will need to be gradually accustomed to manipulations by gently moving the uterus about, using slight downward traction from time to time, and also, if there be much gaping and eversion, by approximating the lips of the cervix by one or two silver sutures and leaving them in for a week or longer. If these procedures, which should extend over several weeks, do not cause the parts to rebel, we can proceed to the operation and not fear the result.

In several cases where these conditions existed and where this plan was carried out in addition to the usual methods of preparation, the results were uniformly excellent. One of Dr. Mundé's patients, whose vaginal vault was solid and whose uterus was immovable since an attack of diffuse pelvic peritonitis several years before, even left her bed, contrary to express orders, on the fourth day, rode several miles in the horse-cars to the store where she was employed as cutter, and worked all day, without in any way disturbing the result of the operation; union being found perfect when the stitches were removed on the ninth day. In a case where very severe peritonitis and cellulitis did occur, the patient had had cellulitis four years before, but all traces of the disease had entirely disappeared long before the operation. Acute inflammation of course contraindicates for the time all operative procedure.

Hospitalism and infection are potent elements and both, but especially the last, are apt to set up very severe types of inflammation. Two of the fatal cases given in the table were unquestionably caused by septic infection, and one case of severe

parametritis had the same origin, while six of the other cases may be attributed to hospitalism. In many of the cases the cause of the inflammation is not given.

Of the six fatal cases, Dr. Hunter's, where the uterus was also curetted at the time of the operation, died on the tenth day exhausted, there being distinct evidence of peritonitis as early as the second day. This peritonitis was thought to be caused by the rupture of a small cyst into the peritoneal cavity, though this supposition could not be confirmed, as no autopsy was allowed.

Of Dr. Mundé's two fatal cases, the first died of septic peritonitis on the fourteenth day. The stitches were removed on the eighth day and the cervix found to be perfectly healed. There was no suppuration. Two days later the temperature went up and general peritonitis set in. The patient had suffered from prolapsus so that there was no traction whatever made on the uterus at the time of the operation.

The second, like the first, a hospital case, showed signs of peritonitis on the second day after the operation, and died on the eighth day, the stitches having been left undisturbed. At no time did the temperature go above 101.5° F.

III. REMOTE DANGERS.

The immediate dangers having been discussed, there remain for consideration those which may occur as a sequence of the operation and these, except possible undue narrowing of the cervical canal, may all be included in the important question, which as yet has not been satisfactorily answered, concerning the influence of the operation upon subsequent fertility and the probability of relaceration or of obstruction to labor by cicatricial tissue, should pregnancy recur.

1. *Undue narrowing of the Cervical Canal.*

This consequence can only be caused by careless or unskilful operating. Occasionally a case will be met where the canal has been left so narrow as only to admit the uterine probe, and very rarely even that cannot be passed.

In the very few cases noted where this condition has resulted

from the operation, there was no marked bad effect from the narrowing; there was no dysmenorrhea from obstruction, and though the chances of the patients subsequently becoming pregnant were unquestionably lessened, still it cannot be doubted that spermatozoa might travel upward through any channel that would suffice for the escape of the menstrual flow.

2. *Subsequent Sterility.*

Murphy, of Washington,¹ from the examination of a phenomenally small number of cases, concludes that sterility usually follows the operation, and that, when pregnancy does occur, a difficult and prolonged labor and relaceration is what we may expect; but others, who have looked into the matter more deeply, differ with him.

Dr. Gehrung² also thinks concerning the establishment of fertility by the operation, that it is to be considered as but "accidentally successful." The views of both these gentlemen are based, so far as I have been able to learn, more on theoretical reasoning than on their own experience.

Dr. Thomas believes that the operation very much increases the probability of subsequent pregnancy, and his views are held by many other eminent gynecologists.

As the great majority of the cases are operated upon by specialists and then returned to the family physician, most of them are of necessity soon lost sight of by the operator, who seldom is able to learn the subsequent history of fecundity or sterility of even a minority of those upon whom he has operated. Consequently accurate and full returns are difficult to procure, many of the cases recorded being isolated ones, which, though they may help to swell the number of subsequent births reported, cannot be used as factors from which to deduce the percentage of those who become pregnant after the operation.

¹ AMER. JOUR. OBST., xvi., p. 28.

² St. Louis Courier of Medicine, vol. ix., p. 534.

Table V.—*Statistics of Conceptions after Trachelorrhaphy.*

Reporter.	Where Reported.	No. of Cases.	Pregnant at term while lacerated.	Aborted while lacerated.	Sterile while lacerated.	Pregnant after operation.	Aborted after operation.	Cervix relacerated.	Cervix not relacerated.	Character of subsequent labor.
Goodell	AM. J. OF OB., xv.	113	1	1	1	1	1	3	1
Hunter	1	1	1	1	1	1	1	1
Clark	1	1	1	1	1	1	1	1
Pallen	6	1	1	1	6	1	6	1
Hanks	AM. J. OF OB., xvi.	3	1	1	1	3	1	3	1	Normal.
Lee	Ibid.	1	1	1	1	1	1	1	1	Normal.
Murphy	AM. J. OF OB., xvi.	3	1	1	1	3	1	2	1
Baer	Trans. Ob. Soc. of Phil., Feb., 1883.	21	3	12	13	6	1	5	1
Smith	Ibid.	1	1	1	1	1	1	1	1
Montgomery	Ibid.	30	1	1	1	5	1	5	1
Cleemann	Ibid.	1	1	1	1	1	1	1	1
Sinkler	Ibid.	3	1	1	1	3	1	1	1
Beates	Ibid.	23	1	1	1	2+	1	1	1
Githens	Ibid.	1	1	1	1	12	1	1	1	Normal.
Ill	Trans. Med. Soc. of N. J., 1882, p. 157.	44	1	1	1	12	1	1	1
Baker	Bost. M. and S. Jr., Nov., 1882.	4	1	1	1	4	1 3 ²	1	1
Davenport	Ibid.	1	1	1	1	1	7 m.	1	1
Peaslee	AM. J. OB., xiv.....	1	1	1	1	1	1	1	1
Watts	Ibid.	1	1	1	1	1	1	1	1
Mackensie	Ibid.	1	1	1	1	1	1	1	1
Papin	St. Louis Cour. of Med., vol. ix. No. 6.	6	1	1	6	4	1	1	1
VanDeWarker	AM. J. OF OB., xvi.	31	11	1	1	9	2 2 2	2	2
Harrison	(Virg. Med. Monthly)	1	1	1	1	1	1	1	1
Cushing	AM. JR. OBS., xvi.	1	1	1	1	1	1	1	1
Janvrin	(Pac. M. and S. Jr.)	1	1	1	1	1	1	1	1
Janvrin	AM. JR. OBS., xvi.	15	1	1	1	3	1 ²	2	2
Basset	AM. J. OBST., xvii.	2	1	1	1	2	1	1	1
Basset	St. Louis Cour. of Med., vol. iv.....	2	1	1	1	2	1	1	1
Balls	Aust. Md. Jour. 1882.	1	1	1	1	1	1	1	1
Ford	Obstet. Gaz., vi.	1	1	1	1	1	1	1	1	Normal.
Mundé	137	1	1	1	13	3	10	4
Jackson	118	1	1	1	9	10	4	1
Mann	50	1	1	1	now of	4	1	1
Lusk	From paper read by Dr. J. T. Johnson before American Medical Association, May, 1884.	300	1	1	1	fre- que't know of 4	1	3	2	O. K. Not se- vere.
Wilson	100	1	1	1	of 4	1	3	2	natural.
Skene	300	1	1	1	Ma- ny.	1	1	1
Lee	100	1	1	1	12	1	1	1	protrac. 11 natur'l
Johnson	16	1	1	1	3	1	1	1
Richardson	17	1	1	1	3	2	1	1	Normal.
Cleveland	1	1	1	1	1	1	1	1
Total	1456	14	13	20	141+	3	17	54

¹ Others not examined. ² Slight. * Commonly. † Don't know of any.

The above table comprises the cases which bear upon the subject now under consideration which I was able to procure up to March 1st, 1884, including those kindly furnished me in advance by Dr. J. T. Johnson, of Washington, from his paper read before the last meeting of the American Medical Association.

Of the 1,456 cases, we can only take, for our present purpose, those of Baer, Montgomery, Ill, and Van De Warker, as they are the only ones where *all* of a number of cases have been kept in sight and the subsequent pregnancies recorded. Of the 126 cases mentioned by these observers, 32, or 25.4 per cent subsequently bore children. Duncan¹ gives the proportion of women who having borne children are sterile at the fifth year of married life as 47.9 per cent; at the tenth year, as 62.1 per cent, and at the fifteenth year as 79.2 per cent. If we consider that an average number of the women operated upon in the 126 cases were at about the *tenth* year of their married life, and that of the possible 37.9 per cent of pregnancies, we have 25.4 per cent actually occurring after the operation, and, if we do not forget the many other causes of sterility, we must admit that trachelorrhaphy does not in the least diminish the fecundity of the women operated upon. Moreover, when we take into account the number of abortions, the tendency to which all admit to be much increased by the lacerated and irritable state of the cervix, it is obvious that the operation materially swells the number of births at term, not only by enlarging the number of conceptions, but also by lessening the number of abortions, so that we may safely say that *Emmet's operation increases the fertility of those on whom it is performed.*

3. *Relaceration at Subsequent Labor.*

Of the 77 cases where the condition of the cervix was noted after labor, 62, or 80 per cent, were not relacerated, while, of the remaining 15, 8 were but slightly torn. Considering that in many cases the same conditions would be present that were present in the previous labors, we could not ask or wish for a more brilliant result than is here shown.

¹ Duncan: *Fecundity, Fertility, and Sterility*, p. 204.

4. *Obstruction to Labor from Cicatricial Tissue.*

This one remaining danger can be dismissed with few words. In cases where trachelorrhaphy has been performed, there is often somewhat more of resistance, and the first stage may occupy a longer period than in the average of cases where laceration has not occurred, but serious obstruction from this cause is almost unknown. It has been noted by all observers who have had occasion to examine the cervixes operated on by them a year or more after the operation that scarcely a trace of the cicatrix remained, the cervical tissue being in this respect apparently exceptionally prone to a complete restoration to a healthy condition.

Dr. Reamy, in his paper read before the last meeting of the American Medical Association, in Washington, holds almost precisely the same views concerning the ultimate results of the operation, which are sustained in this article, saying: "It is impossible for me to give exact data as to the influence of the operation on sterility, as many of these (two hundred and thirty-one) cases soon after recovery passed out of my knowledge. And as the last fifty cases were operated on within the past thirteen months, sufficient time has not elapsed to test the question. I know, however, of fifteen cases in which conception has occurred and delivery took place at term. I have no doubt that as many more have proven fruitful. Of these I attended in labor six, one of these within the past month. In no case did relaceration occur, labor being normal except in two cases. In these dilatation of the cervix was slow, but finally complete; in neither case was it protracted. I have learned from medical gentlemen, who attended several of my other cases, that labor was normal. I believe that the operation properly done favors fertility—and often cures sterility."

RÉSUMÉ.

The most important points which may be deduced from what has been said concerning the *primary dangers* would seem to be:

1. That primary hemorrhage, though not uncommon, is rarely alarming, and when severe, is easily controlled by traction exerted upon the cervix, or by one or more sutures passed deeply under the bleeding points.

2. That secondary hemorrhage is rare, but, when it does occur, is a serious danger. That it may happen, not only when the circular artery has been wounded during the operation, but also at times as a consequence of the cutting of a suture into a previously intact arterial twig. That when it does happen, if very severe and the instruments are at hand, time should not be wasted in trying other means, but that we should at once apply the deep suture, twisted tightly on the side from which the bleeding comes.

In the absence of the proper instruments, and in moderate cases, tight tamponing with discs of alum-cotton will suffice, and not interfere with union.

3. On account of the danger of secondary hemorrhage from the cervix, it is an open question whether, in those cases where both lesions exist together, it is not best and wise to defer the repair of the lacerated perineum to some time after the closure of the cervical rent, and not, as a routine practice, do both operations at one sitting.

4. Menstruation coming on before the removal of the sutures does not necessarily cause trouble, if only they be allowed to remain *in situ* for a few days longer, or until it ceases.

5. Non-union occurs in about eight per cent of all operations, the percentage of failures being larger in hospital than in private practice.

A flabby, hyperemic condition of the cervix is most apt to lead to this result, but it may also be produced by too tight or too many sutures.

6. Serious inflammation is a not very infrequent sequence, and even death occasionally follows.

7. Inflammation frequently occurs where there has been previous cellulitis, and it can be best avoided by recourse to the manipulative measures described.

What have been considered as *secondary dangers* by some writers, are shown to be in most cases palpable benefits, the facts given proving the following:

1. Trachelorrhaphy does not cause sterility.

2. On the contrary, it causes a decided increase in the productive fertility of the subjects of the operation.

3. After the operation there is even less liability to subsequent cervical laceration than there was at first.

4. There is no danger of anything like serious obstruction to subsequent labors by the cicatricial tissue formed in the cervix.

5. There is very little danger of producing serious stenosis of the cervical canal, except through inexcusable carelessness.

DELIVERY OF THE HEAD IN THE SECOND STAGE OF
LABOR, WITH REFERENCE TO THE PREVENTION
OF LACERATION OF THE PERINEUM.

BY

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IN a practice of over fourteen years, it has been my lot to attend a large number of women in confinement, and, like every conscientious physician, it has been my endeavor to bring them through all the perils attending parturition with as small an amount of physical damage as possible, and the minimum injury to future health; the grade of the former, as we all know, being in direct and inseparable relation to the latter. The perineum, owing to its situation, being the structure to which so much damage occurs, and its functions so important, both in relation to parturition and subsequent health, it is that part of the female procreative organs which accoucheurs are so solicitous to protect from injury in the second stage of labor, and about which so much has been written, in regard to laceration, and the prevention of which still remains, as Goodell remarks, the riddle of the sphynx. Understanding the anatomical make-up of the perineum teaches us most thoroughly its usefulness. But it appears that its anatomy is better understood and studied than is any *method* to prevent its laceration. More study should be given to method, and less to the anatomical rising and insertion of muscle. It has been proven again and again that the most expert physician has failed many a time. The obstetrician who lectures day after day, acquainted with all the minute anatomical bearing and relationship of perineal structures, who can describe all the muscles, fasciæ, and fibres in its formation, who can point out all the

weak and strong points, where and when to press, where and when not to press, how to place your hands, "thumbs up or thumbs down," comes to grief and offers us no better methods than we can devise ourselves. So we have to look out for the reputation which may follow us by the production of post-parturient cripples; that, when the patients we have attended, in describing their feeble health, do not have good cause to attach their attendants' names to the beginning of their troubles, which date back anywhere from the time the first child was born until the last one came into the world. The *method* to pursue is that one which the physician learns to succeed with in his own way, having at the same time high respect for the authors of other methods, and being acquainted with the manner the "masters" recommend, but at the same time being master of his own hands, head, the situation, and the manœuvres necessary to come out safely, if there be even the slightest show for success—success at the trying moment. To learn to take in the *individual* situation and its bearings, and directing the forces at command to best advantage, by understanding less of the anatomy of the perineum, and more of the axis of the parturient canal, the mechanism of labor in regard to fetal diameters, flexions, and extensions; perineal resistance and elasticity, by directing practical attention to this important organ, upon whose well-being the whole maternal health revolves for time to come, and patiently testing all means under the proper indications, is the only way to get a true conception, quickly on the spot, of the *method* to be adopted by which we are to conduct a second stage through with safety, and leave the posterior-wall foundation of the uterus without any bricks or mortar knocked out, or any permanent weakening in its masonry. All the written or lectured wisdom of a Duncan, Barnes, Mundé, Thomas, Garrigues, Goodell, and a host of others, will not do this for us lesser lights in the accoucheur-firmament. It is only somewhat gratifying—a rather selfish gratification—to find that they, all of them, have gone down horse, foot, and dragoons, before this same enemy, repeatedly, and have probably a number of times sworn a religiously mild "condemn it," like Josh Billings' schoolmaster, at their want of success, and at not being able, with great experience and knowledge, to

prevent it. In attending over five hundred successive cases of labor, I believe I have worked as faithfully and as perseveringly as any one could possibly do to save the perineum of my patients, have tried all known methods, and yet have gone home, many a night, chagrined at that want of success which I thought my incessant watchfulness entitled me to, and wondering why Providence did not add a little more India-rubber to the patients' perineums, make the heads a little smaller, the pubic arch wider, or the sub-occipito-mental diameters smaller. In the last few years, I have come, rather mechanically, and I might say involuntarily, to use the method I wish to describe, which has not been wholly successful, but has undoubtedly given me the very best results, if in not preventing lacerations in all cases, by preventing the *rent extending*, controlling it to the smallest extent, taking into consideration all circumstances surrounding each individual case. The one great defect in the methods taught by authorities, and which I have proved by repeated trials, is a want of a powerful directing force over the head, a want of power in lifting it away from a *weakened and thinned anterior perineal edge*, which will be spoken of further on, a want of resistance sufficient, to a certain extent, to annul the uterine force, which often drives the head downwards too far toward the anus, before extension takes place, or begins to take place properly, placing too great a stress upon the perineal centre, deficient force in combating this stress upon a part of the perineum, making a normal descent abnormal before the head glides upward in correct extension upon the arc of the circle it forms, reaching from the perineal beginning up to the anterior edge of its own structure; the perineum, of course, being the natural external prolongation of the curved surface over which the face must pass, and this curve made by the uterine force propelling the head somewhat downward and backward and then, through the axis of the fetal body, by an extended chin, upward, and eventually over the symphysis pubis. Consequently, our chief aim is to have such control that the head can be *directed* from any position in the perineum likely to become abnormal, to re-inforce the *perineum at the same time*, and see that the normal curve is carried out as nearly natural as possible. Therefore, a method which will combine power in *directing the head*, and

also at the same time *strengthen the thinned and weakened* (for thinness here means weakness) *edge* of the perineum, and annul the backward pressure of the head engaged in the perineum, and send it on its proper curve, is the method which will give the most satisfaction, outstrip any single or dogmatic method, and will succeed wherever success is possible.

Goodell's method will do this only in an imperfect manner. It is a filthy method, and in directing the head exerts but little power, say one thumb and two fingers against a powerfully acting uterus. Playfair's will do to kill time, and how by itself it can do any good, anyway, I can't see; might as well try, when the rectum is passing an unusually large bolus, to put the thumb on one side of sphincter, and fingers on the other, and press the fibres up toward the centre to make the mass pass more easily. Again, any method to succeed must be one that in recognizing Carus' curve (true or so called), considering the head as passing through it, the sub-occipital part must impinge upon a point around which it to a certain extent must revolve.

The sub-pubic surface is this point. If the head departs from this pivotal point by too much downward pressure, or does not hug this point sufficiently close, all the influence of extension which is about to take or is taking place, is lost. Now, in the first place, then, at the proper time, the head must be kept firmly to its pivotal place until the sub occipito-bregmatic, the sub-occipito-frontal, and sub-occipito-mental diameters have successively presented, and passed.

So that any method to succeed must be one which will meet all indications possible to fulfil, and to supply a powerful directing force, when necessary, to the head,¹ applying the pressure, the force, to the head in directing it, or combating uterine vis-a-tergo, moderate and judicious pressure to the perineum, or labial sides, or intra rectal manipulations of the chin if necessary. The method which we adopted and used in the last five years is as follows, and in describing it, I feel as Professor Hodge once said,² "in all such scientific discussions it is very

¹ "It is not the perineum that needs support. but the head that needs it. By supporting the head we support the perineum." Goodell: *Lessons in Gynecology*, page 94.

² "Synclitism of the Fetal Head in Natural Labor," *American Journal of Medical Sciences*, Oct., 1870, page 325.

difficult to express our ideas with so much precision as to be clearly understood," especially in regard to a subject where there is not that well-defined understanding as to extent of meaning of names and of parts. Anyway, the medical brethren are always charitable, even the college professor, who keeps his vocabulary, general, medical, and scientific, well oiled by every-day use, and by virtue of being a teacher of men, a "master," will probably overlook crudities, and not consider the writer "asinus a lyrum," nor the opposite.

When called to a case, and when the second stage is well advanced, and the head is pressing upon and bulging out the perineum, and the occiput is approaching or has reached its sub-pubic position, and in the judgment of the accoucheur artificial assistance is necessary to guide this stage through successfully, then the patient should be, if not already, turned on the left side,¹ and a pillow rolled up and tied securely, or a folded quilt placed between the knees. Have the patient always brought sufficiently near the edge of the bed, leaving room enough for the obstetrician to take his seat at the woman's back, the face, of course, looking toward her buttocks. The *left* hand should be passed over the patient's right groin, down between her legs, over the vulvar orifice, until the ends of the fingers *touch* the *anterior* edge of the perineum, their palmar surfaces, *covering* the presenting part of the fetal head, which is alternately advancing and receding between the labia. This is the position, at present, for the left, by all means the most important hand.

For the right hand, take a folded napkin, thick and soft, and place it against the perineum, the fingers spreading out reaching to the sulcus between the labium and thigh on one side, and the thumb to the sulcus between labium and thigh on the other side. The duty of this much misused, and often confused, right hand, is to furnish support, if needed, to measure pressure and force of pain, to note resistance and elasticity of perineal structures as they swell out into the open palm, to determine if pressure be too much toward the rectum,² the head

¹ Leishman states in his "System of Midwifery," page 268, that some eminent authorities object to this position, at this stage, but he indorses it, and advises it

² "Rupture occurs mostly in consequence of the head being driven too much backwards." Fancourt Barnes: Manual of Midwifery, page 101.

being driven too much downwards and backwards before extension begins to take place. Again, if needed, the fingers and thumbs on opposite sides can press the perineum laterally toward the centre, relaxing it and strengthening it by concentration of fibres, modifying the pressure on the raphe, agreeably to Playfair's method, and spoken of by Goodell as like a good general hurrying up re-enforcements to a weak point. If necessary, the fingers can be brought up *toward the anterior thinned edge of the perineum, to meet those of the left hand* from above, to assist in making stronger this weak point, where the *rent always begins*, also to render aid in keeping the head under the pubis. Or, if found answering a better purpose, especially between the pains, the fingers can be introduced into the rectum, the chin searched for, and if reached, hooked up, and extension completed, or so nearly completed that in some cases the head is just ready to be born by the time the next pain comes on, or gets to its acme.

There is rarely need of firm, direct pressure, as advocated by Ramsbotham in days gone by, with the elbows resting on the bed, as a fixed point forcing the distended perineum against the oncoming head. This form of injudicious support has been relegated to the realms of desuetude, as hurtful, impeding circulation, causing the tissues to become hot, dry, and inelastic, but with a napkin between the hand and perineum, and common sense added, no harm can possibly be done, even if in the obstetrician's judgment it may be necessary to exert a little pressure *intermittingly*. The bare-hand pressure is rarely, if ever, called for, but with the protected hand as recommended, and placed as described, intermitting pressure can be used if deemed safe and necessary; the distensibility, dilatability, and tension of perineum can be determined—matters very necessary in some crucial cases—the correct *direction* in which to apply force can be appreciated, and manœuvres, deemed correct to meet emergencies, be executed. In all probability, many other features of the usefulness of the right hand have been omitted, but I have no doubt I have mentioned the major part, and the most important. I do not wish to make the right hand so narrow in its functions as to confine it to any dogmatic single method in preventing lacerations, but as acting a part in a scheme with the left, on certain principles, but made to meet

emergencies and overcome difficulties by the guiding spirit of the physician, the brain. As said previously, according to the method I have adopted and practised during the last five years, I consider the left hand the most important, because, in the manner in which I use it, it can exert so much more power in resisting uterine action, and so much more directing force over the head, than the right. The woman being on her side, the physician sitting at her back, he can with the left hand pull from below up, with much more force than he can press with the right from below up. His purchase over the head is such the right hand could never obtain, is such as could be gained by no other method.

The power wielded in directing the head by the left hand, and keeping it away from the *thinned and weakened perineal edge*, will save that structure if any method can possibly do it. *The left hand is kept applied to the head while the perineum is on the stretch and the fetal head is passing the vulvar opening, except during the absence of pain*; it can appreciate all its movements, and calculate the force of the uterine pains, and as the tips of the fingers touch the anterior thinned edge of the perineum, the *condition of that structure* can be pretty accurately determined. The whole process, if not trusting to the touch as revealing the true condition of the perineum, can be made more useful by the eye, for when the patient is on the side, the labor can, and ought to be, especially in the latter part of the second stage, watched ocularly, as there is but little exposure to the patient, quite different from that when she is lying on the back, and at this point of the labor the pain is generally so great, the patient is oblivious to everything except her acute agony.

As labor advances, and the head is driven further down, and the sub-occipital point becomes fixed as a pivot, and extension takes place, and the vertex begins to emerge more and more between the labia, the perineum bulging greatly, put upon its utmost tension, and thinned out until its anterior surface glistens and turns livid, the points of the fingers are kept pressed steadily and yet more firmly, over the head and on a level with *where the perineal edge leaves off*, to take the weight, as it were, from tired and exhausted shoulders, to fresher and stronger ones (the fingers), offering the opportunity for the safe trans-

mission of the movement of extension, as the fingers receive first the sinciput, forehead, face, and chin, sliding them into the palm as they emerge, and continuing the fetal expulsion in the imaginary circle of Carus, over the symphysis pubis. I am not sure but that there is a decided advantage in pressing the tips of the fingers *below* the level of the anterior perineal edge, between the head and the perineum, as the strong force which can be exerted from below upwards takes the pressure from the weakened edge, transmitting it to the fingers, more than compensating for the increased thickness caused by the manœuvre in the head diameter plus the thickness of the fingers. After the head has been born, it can be held by the left hand, and the right can come into play to assist the shoulders, and support against the laceration which they cause, or prevent them from continuing a laceration begun by the head. Quite often the shoulders will do more damage than the head to the perineum, especially if the fetal hand of posterior shoulder is resting on side of child's face with the elbow sticking out, the forearm being so wedged that it is impossible to extend and deliver it without great danger of breaking its bones. My own conviction is, that the *strong point* of the method described lies in recognizing extension and its mechanism; that the head must come so far; be arrested in its down course at the sub-occipital point, the axis of the expelling power be changed from behind the occiput toward the chin, beginning a new movement in the mechanism of fetal expulsion; the face must take to the perineum and sweep over its surface, which surface is the *external prolongation of that circle* whose beginning is at the inlet of the pelvic cavity, and the *anterior perineal edge* is the *structural end of that curve*, from whence begins the imaginary segment or arc that carries it over the symphysis pubis to completeness. This external structural, material end of this important curve receives the *brunt* of the force of extension; it has the least inherent power to protect itself; it is the thinnest, muscularly the weakest, of all the parturient parts, the most important to the well-being of the woman, and then it must be persecuted by that long fetal diameter, the sub-occipital mental, which is rarely found parallel with the pelvic diameters, and hence it is this *anterior edge* that needs all the artificial assistance that can be given it,

and this the method which I have tried to describe does it the safest, and the most positively, and the most directly, and the most scientifically, because it aims to carry out artificially the expulsive curve which was begun at the pelvic inlet, by placing the fingers of the left hand in such a position the anterior perineal edge could be fortified until the extension could be safely transmitted to the accoucheur's fingers and hand.

The hand is there to *strengthen when the acme of the pain* is reached when the child is born, and the tired perineum slips back toward the fetal neck to rest and to recuperate. It must be recollected to succeed, before the chin is born, the head must be pressed firmly against the sub-pubic arch, for herein lies the great superiority of the left hand over the right, for the hand can have no very successful power of completing extension unless it exerts a *resisting force equal to the perineal edge, from which the force behind transmits its charge to the hand.*

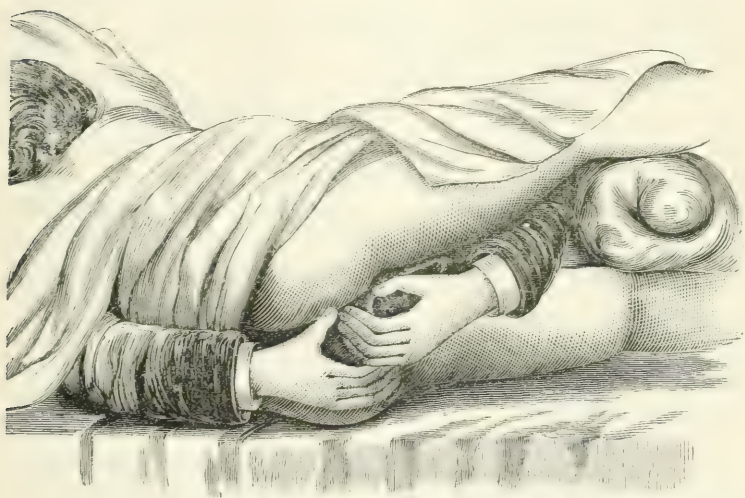
In the raphe, or at its sides, in the perineal edge, is where the rent begins, and it is just here where the great power the tips of the fingers possess, is exerted, so that even if a rent cannot be prevented, it can *be limited*, by receiving the face in the hand, and lifting it away from the gaping wound, and forcing it to continue its route in the so-called Carus' curve.

I am in hopes the accompanying woodcut will give some idea of the method which I have tried to explain, although it is imperfect in some important respects, as elucidating the subject under consideration; the artist was not experienced in such illustrations, and did it by verbal directions, except what information he could get from a study of Plate 102 in Playfair's work on Midwifery.

I have found I could succeed as well with some patients partially on the side as in a complete lateral decubitus; some choose the half lateral position because it is more comfortable. To be successful with any method, one must understand it in all its important details, and be faithful in carrying them out.

I have tried to be explicit, have attempted, even though in danger of repetition, to state what I expect to be accomplished both by the right hand and left in the second stage of labor, and if the method should seem tiresome to the physician, what does that amount to if it will get him through with that degree

of success which will repay him for a great many back-aches. I have never yet found a patient object to the procedure, although some have complained if, through forgetfulness, I have allowed the arm (the left one) to rest a little too much on the tender bowels. Sometimes the patient, in the agony of her last pain, when the perineum is greatly on the stretch—distended to its greatest capacity—if she has not good and watchful assistants, will seize the physician's arm, and completely nullify the benefit he is conferring just at the most critical time. I learned by experience to be prepared for this, and also to be sure and not let the patient slip away into the middle of the



bed at the time when she needs the most careful assistance and support to the head. Another benefit to be derived from the method we are now discussing, that, *seeing* our *inability* to *prevent laceration*, the whole process being directly under the eye, the fetal head can be repelled sufficiently to allow time to take the scissors and perform the operation of episiotomy, for this little surgical interference, as we all know from experience, succeeds admirably. It is important that every case should be *individualized*, and the amount of force, power, and directing manœuvres be used according to the judgment of the practitioner.

It is not my desire to criticise other methods, but with

Goodell's, two fingers in the rectum, and a thumb against the head, repelling force and directing power, according to my experience amounts to next to nothing; in counteracting the vis-a-tergo of the uterus, certainly nothing. The anterior resisting force is a couple of fingers in a sour-smelling rectum, and a thumb on the head of the slowly approaching fetal cranium, slippery and slimy with vaginal mucus, a corrugated scalp that under pressure will make a shifting foundation for repellant or guiding power, and the force applied be so uncertain in its direction as to be almost entirely lost. The stress of pressure comes between thumb and fingers, producing a tiresome strain on the hand, and if the patient is on her side, the whole arm must act as a lever, resting on no fulcrum except its own diffuse muscular strength, and if the parturient act is prolonged, fatigue will reduce the remaining resisting and directing power left, and if the practitioner gets off without some excrementitious matter lubricating his arm, he will be exceedingly lucky, as all women will not take injections—and I have noticed that in some who did, in the hidden recesses of the rectum there remained a quantity of fecal matter that would not make its appearance until the head drove it out, and the followers of Goodell's method would receive a parting shot.

I will close by giving a résumé of the manner of method or combination of methods I practise and advocate in the second stage of labor.

1st. Both hands can be used.

2d. The function of the right hand is principally that of a sentry to keep watch over the perineum, the advance of the head, and uterine force.

3d. It can, if necessary, be used to dilate the maternal passage, stretch the perineum, or be introduced into the rectum, when feasible, to hook up the chin.

4th. The fingers of the hand can be brought to bear at the fourchette to meet those of the left hand from above, to fortify and strengthen this thinned and weakened part.

5th. The right hand can be used to counteract too much pressure downward against the rectum, or can be used to fulfil the indications laid down by Playfair.

6th. The position of the patient and the position of the ac-

coucheur enables him to watch the advance of the head, and the action of the perineum under pain.

7th. The left hand fulfils those most important indications laid down by Barnes, Lusk, Goodell, and others, to support the head. It has great power to repel or direct the fetal head.

8th. It protects the thinned edge of the perineum, the fourchette, the *external structural end* of the posterior curved surface of the parturient canal, and directs the head and shoulders from this point in the imaginary line, the curve of Carus, over the symphysis pubis, carrying out the external mechanism of labor perfectly.

9th. If laceration cannot be prevented, it can be limited in depth.

10th. While the left hand directs the head, the right can look after the shoulders.

11th. The child's head having been born under the eye, the extent, externally, of laceration can be seen, and will give a correct idea of its actual depth. Sometimes mistakes are made as to actual depth and extent of perineal wounds, on account of the thorough relaxation and retraction of the perineum after the complete delivery of the child.

12th. In thinned and livid perineæ, where the utmost tension has been reached and laceration about to occur, the head can be repelled, and episiotomy be easily performed with the scissors in the right hand.

13th. With proper attention to details suited to each individual case, with perseverance and patience, it is my belief that more laceration can be prevented in the management of the second stage of labor by the method described, with its details, than by any other. There is one precaution to be observed. When the head is well engaged between the labia, and the pains are strong and forcible, and the chin is about to clear the perineum, the physician, in his eagerness to prevent a laceration, is apt, with the left hand placed in the position I have described, to exert too much force to keep the sub-occipital point hugging the under surface of the pubis, and might produce a laceration of the sub-urethral surface, or a solution of its continuity. This happened in one case under my charge; the head was rather small, and I forced the occi-

put too strongly against the urethra. On the second day the patient began to complain of scalding pain when micturating. Not having been relieved by treatment in two or three days, I made an examination, and found a raw surface about the size of half a dollar on the mucous membrane of the vagina covering the urethra near the vaginal outlet, where the occiput had rested as a pivotal point during the mechanism of extension. Touching the parts lightly with nitrate of silver and applying iodoform for a few days cured the abrasion.

February 19th, 1884.

A MUMMIFIED FETUS IN TWIN PREGNANCY—ONE PLACENTA.

BY

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THE interest developed in occurrences of this kind by the publication in the January number of this JOURNAL of the cases of Fruitnight, Edis, and Malius (in Transactions of Obstetrical Society of London), has led me to think that a brief report of a case that came under my care might prove interesting.

Mrs. M—, age twenty-six, mother of four children, all healthy, free from syphilitic or other taint, was taken with labor pains on the morning of November 26th, 1883. I reached her bedside at 11 o'clock A.M., and heard the following history: Pregnant seven months. At the third month of pregnancy, after a carriage ride, hemorrhage appeared, but after rest in the recumbent position for a short time, it ceased, and nothing more was thought of the incident until labor commenced at the time above indicated. Upon examination, I found that a mummified fetus of about three months had been delivered early in the morn-

ing, but had not been removed. The mother was quite confident that another and living child remained, which I found to be true. The os was dilated to the size of a silver dollar or a little more, the membranes were presenting, but there were no pains.

I now removed the first child, and awaited the action of the uterus. Contraction soon began, and, in a short time, a living female child of about seven months was delivered, surviving some three or four hours. Not one drop of blood had escaped until after the delivery of the second child. The placenta was soon delivered, there being but *one*, and herein was the difference between this and the other cases referred to. The peculiarity of the placenta consisted in its outline, resembling that of the kidney, the cord of the mummified fetus being attached near its edge at the fissure of hilus, while that of the living child was attached near the opposite or convex border, showing that that portion of the placenta through which the dead fetus was nourished ceased to develop after its death, while that through which the other child was nourished continued to develop until delivery.

ON THE MANAGEMENT OF FACE PRESENTATIONS.

BY

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IN this short paper I wish to direct attention to the management of labors in which the fetus presents by the face.

I have selected this subject, first, because I regard it as worthy of more than the cursory mention given it by obstetric authors; secondly, because I hope to acquire information from the experience of those members of the profession who may have met with such cases.

There is no great disparity in the views of obstetricians upon the *etiology* of face presentation. Certain clinical facts, in connection with face cases, present themselves with frequency too great to permit them to be regarded in the light of coincidence. We are led to conclude that the transformation

from vertex to face presentation occurs late in pregnancy, generally taking place in the ten days or two weeks immediately preceding labor. During this time there is a settling down of the uterus and its contained fetus upon and into the pelvic brim, and the occurrence of uterine contractions, varying irregularly, but notably more marked than those which occur during pregnancy up to this time. These contractions are often sufficiently severe to produce more or less irregular pain.

According to the prevailing opinion, the elements which, in the majority of cases, favor extension of the head and descent of the face, are uterine obliquity, a dolichocephalic child, and hitching of the occiput upon the brim of the pelvis. Lateral, uterine obliquity, to have an influence in the production of face cases must be toward that side of the abdomen at which the occiput originally, the forehead at a later stage, lies. If such obliquity is present, and especially if of a marked character, it happens occasionally that, as the head descends and impinges upon the soft structures at the pelvic brim, the occiput, which is the most advanced part of the head, hitches upon them, the natural position of flexion of the head on the thorax is overcome, and finally the head reaches a condition of extension and the face is then the presenting part. That an unusually long occiput (which characterizes the dolichocephalic fetus) should greatly favor the occurrence of these phenomena is obvious.

In order to better illustrate certain other points in connection with my subject, a brief narration of cases will be of service.

CASE I.—Primipara, age twenty-seven, labor at term. Membranes ruptured and liquor amnii discharged twelve hours before the occurrence of pains. When the os would admit two fingers, the presentation and position were determined to be face, with chin posterior and to mother's right, brow anterior and to the left. The pains were short, occurring at long intervals. When the os was nearly dilated, chloroform was administered, my right hand passed into the vagina, and by conjoined manipulation the child's head was flexed on its body and a left occipito-anterior position thereby established. The manipulation required the introduction of the fingers only into the uterus. The palms were passed over the occiput, and, by slight downward traction, the change in the presentation was easily effected. The left hand

assisted in the manœuvre by external upward pressure, lifting, to some extent, the head out of the pelvis. During the next three hours the presentation showed no disposition to alter, but no advance was made as the pains were as inefficient and infrequent as before. Remaining in the house, I retired to bed, to be called when the pain increased in severity. Six hours later, however, the uterine contractions had not improved and a *face* presentation again existed, with position as originally described. Chloroform was again administered and the operation of inducing flexion of the child's head was again successfully performed. The forceps were immediately applied and the head brought well down in the pelvis. The instrument was then removed and good uterine action occurring, the labor terminated naturally in less than half an hour. Mother and child did well.

CASE II.—Primipara, age fifteen, term labor. When the os had reached the size of a quarter of a dollar, the presentation and position were found to be face, with brow anterior and to mother's left side, the chin being situated to the right, posteriorly. The os having dilated, the patient was anesthetized and I succeeded in converting, with my hand, the face presentation into one of the first position of the vertex. The membranes ruptured when the hand was passed into the vagina. The case was now normal with respect to the presentation and position. Excellent labor pains followed and the head descended to the pelvic outlet. The escape of the head from the pelvis was opposed by an abrupt projection forward of the coccyx at the sacro-coccygeal articulation. No progress was made for some time during which, the patient being anesthetized, attempts were made to overcome the deformity by the use of all the strength that could be brought to bear by the fingers. The forceps were finally applied, and three-quarters of an hour was occupied in delivery by this instrument. The child was still-born, weighing eight pounds. Examinations made immediately after labor, and again three weeks later, revealed the fact that the deformity was not relieved by the pressure of the child's head during its extraction. Puerperal convalescence was normal.¹

CASE III.—Multipara, seen in consultation when the os was fully dilated and membranes ruptured. The physician in attendance stated that a complete face presentation had existed during the entire labor. He had been able to convert it into a vertex presentation, but in less than half an hour it became again one of the face. The head was still in the uterus and movable. Again giving chloroform, I also was able, without difficulty, to flex the head, by the manipulation described in the previous history, and in view of the tendency already shown, to a return to face presentation, the forceps were at once applied. While the instrument was applied, notwithstanding the reasonable use of traction,

¹ These cases were reported in the *New York Medical Journal*, March, '77, and occurred at the New York Infant Asylum.

with manual supra-pubic efforts to steady the head, the presentation again became face. Operative measures having been already somewhat prolonged, internal podalic version was performed with success. Convalescence was normal.

CASE IV.—Second pregnancy, age twenty-three, term labor. In the first labor, a large child was delivered by the forceps by a well-known physician, the presentation being vertex, first position. When in this, the second labor, the os was half dilated, the presentation was found to be face, the chin being posterior and to the mother's left, the brow anterior and to the right. When the cervix was dilated, chloroform having been given, with the hand in the vagina, and fingers within the cervix, I succeeded in altering the presentation to that of vertex, second position, the *membranes remaining unruptured* after the operation. With the next pain, I ruptured them; the head promptly engaged, and descent took place. At the pelvic outlet, considerable delay took place, and the head was finally lifted past the ischial tuberosities with the forceps. Mother and child did well, the latter weighing ten and a quarter pounds.

CASE V.—Primipara, age twenty-two, confined during my service at the Nursery and Child's Hospital, Dr. Nelson H. Henry, then resident physician, having charge of the case. The diagnosis of face presentation, chin posterior, was made early in labor. When the os was nearly dilated, chloroform was given, and Dr. Henry converted the presentation into that of vertex. This was accomplished by the introduction of the hand into the vagina, and the fingers in the uterus. The hand on the abdomen was unnecessary, and the membranes were not ruptured during the manipulation. The head commenced descent, and, twenty minutes later, the membranes were ruptured. Labor terminated successfully, the child weighing eight pounds.

It will be apparent from these histories that the feature in the treatment, to which I would call especial attention, is the change of an unnatural presentation to a normal one; and two questions arise in this connection. First, in view of the circumstances which commonly attend labor complicated by presentation of the child's face, is it desirable to effect a change from face to vertex presentation? Second, is the operation itself easy of performance and free from near or remote danger to the mother or child?

The prognosis in face presentation, as agreed upon by all obstetric writers, is that labor may be expected to be prolonged, with the usual risks attending delay, though, in the great majority of cases, the result is favorable. Lusk says: "Though spontaneous delivery is the rule in face presentations, the dangers to both mother and child are considerably greater

than in vertex presentations. The causes of the less favorable prognosis are to be looked for in the increased peripheral head measurements, which engage successively in the different planes of the obstetric canal, and consequently, from the increased reciprocal pressure exerted between the head and the soft parts, and partly from the compression of the veins of the neck by the anterior wall of the pelvis. Though the average length of labor does not much exceed that with normal presentations, the duration is more readily affected by minor disturbances, such as weak pains, moderately contracted pelves, and rigidity of the obstetric canal. At the same time, the prolongation of labor in these cases is attended by more disastrous consequences, and calls more frequently for the resources of art to complete the delivery." One child in every nine or ten is still born, and Winkel states that the mortality to the mother is six per cent. If the chin remains persistently posterior, death of the child is almost certain and delivery usually impossible, unless there is reduction in the size of the head by cephalotripsy. These are the opinions generally accepted. Remembering these statements, it may be noted that change in the presentation from face to vertex removes at once *all* the elements of danger enumerated, and if labor progresses without other complications, we have to deal with a normal condition of affairs. What need can there be for farther argument upon the *propriety* of the operation if it can be shown to be reasonably easy and safe?

Returning to a consideration of our five cases, we observe that *seven* attempts were made, by three different operators, to effect the desired alteration of presentation, and *all* were successful. Four times the operation was performed in *primipare*, and four times when the *liquor amnii* had previously *drained away*, yet no increased difficulty was thereby encountered.

The conditions *especially* favorable to the operation may be stated as follows: An os nearly or quite dilated; a face not engaged in or at least capable of being readily lifted from the pelvic brim; an unruptured bag of waters; a capacious vagina. In the majority of labors, a stage is reached when there are present these conditions. Chloroform to relax the structures of the parturient canal, to quiet the movements of the patient,

and to obviate pain which would attend the introduction of the hand into the vagina, is of primary importance. The manipulation requires the presence of the fingers only in the uterus, and does not involve any laceration of the cervix. Passing the palms of the fingers over the occipital bone, and pressing them firmly against it, traction downward should be made. In our endeavors, flexion of the head almost immediately commenced and quickly became complete. The other hand aided greatly by external manipulation. In two of the three instances in which the membranes were unruptured at the beginning of the operation, they remained unbroken at its completion, showing how simple the operation can be. It will be observed, however, that in two instances there was subsequent return to face presentation, after a change to vertex had been effected.

This is probably owing to a lack of tone in the muscles which should flex the head on the thorax, consequent upon the previous state of extreme extension of the head which had continued for some time, there being still in force, to a considerable extent, the factors which originally created the face presentation.

I would recommend, therefore, that watchful attention be given to every case until after engagement of the vertex in the pelvis takes place, and, if a tendency to a return of the original face presentation be observed, the forceps be employed simply for the purpose of *engaging* the head.

We met with but one instance in which it was impossible to maintain the normal, vertex presentation, after its substitution for the face.

I think the method of operating which I have described has decided advantages over those of Baudelocque, Clark, Hodge, and Schatz, all of whom aimed to alter, by various manipulations, face presentation to that of vertex.

Baudelocque advised an attempt to make such change *as soon as two fingers could be introduced into the os*. Such an attempt must be attended with great difficulty, and by much unnecessary, and perhaps dangerous, stretching and laceration of the cervix. Moreover we would be somewhat prone to meet with a return of the face presentation—if a change had been accomplished, because the factors originally causing the presentation of the face would still have their influence. If flexion was secured by his operation, a considerable time would elapse before

the completion of the first stage of labor, and engagement of the head in the pelvic brim. This period would afford sufficient time for catching of the occiput upon the structures superjacent to the inlet of the pelvis, and reproduction of face presentation if there was uterine obliquity or the dolichocephalic form of cranium.

Clark and Hodge hoped to change the face to the vertex by upward pressure on the malar bones, after the head, presenting by the face, *had descended* into the pelvis. No great success has attended such efforts in the hands of others, while the possible danger of establishing a brow presentation has deterred many from undertaking the procedure.

Schatz attempts to correct face presentation by external manipulation only. He recommends grasping the child through the uterine and abdominal walls. The fetus is to be lifted upward until the presenting part is liberated from contact with the pelvic brim, and the uterus straightened until its axis is in the median line of the woman's body. The child's breech is then pressed forward with one hand, while the other hand, placed against the upper and anterior part of the child's thorax, pushes this part of the body backward.

When semi-flexion of the head is believed to be gained by these manoeuvres, downward pressure upon the child will, he claims, completely restore the presentation to that of vertex. Theoretically viewed, this plan is ingenious and attractive, but I think there can be extremely few cases in which the fetus, surrounded by liquor amnii, can be thus manipulated through resisting uterine and abdominal parietes.

In conclusion then, I should say that the method employed in my cases is *easy*, is *reasonably sure of success*, and *free from danger*. It requires the introduction of the fingers only into the uterus—little more of a procedure than that often required in a thorough examination of a presenting part. If the manipulation is successful, there will be no occasion for tedious and uncertain endeavors recommended to assist in rotation of the chin forward, if it be directed posteriorly. We are spared the necessity, perhaps, for the use of the forceps, version, or even craniotomy. It must be remembered also that we cannot predetermine whether or not a chin, which enters the pelvis posteriorly, will rotate anteriorly during descent. If failure

attends our efforts, the case cannot have been complicated by them, for we have simply the original condition with which to deal.

A CASE OF SUCCESSFUL OPERATION FOR ATRESIA VAGINÆ.

BY

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IN January, 1882, I was requested to see Miss ———, age eighteen, apparently a perfectly developed woman, save in the fact that she had never menstruated, though for the last two or three months she would have periods of suffering, sometimes intense, with backache, bearing-down pains, and other symptoms of dysmenorrhea. Treatment directed to this end, in other hands, having afforded no relief, a local examination was requested and granted. This revealed perfect atresia vaginæ, implicating one-half of the canal, midway between the cul-de-sac and ostium vaginæ, about one joint of the index finger being admitted in front, and examination per rectum revealing a small dilatation at cul-de-sac, containing fluid. The condition being explained to the friends, they did not receive favorably the proposition of a radical operation, preferring first to gain temporary relief by a simple puncture.

Ether was given, and with two fingers in the rectum to guide, a trocar and canula was cautiously passed between rectum and bladder. The fluid, black and tarry, was too thick to run, but sufficient, one or two ounces, was passed by pressure to give relief.

She was comfortable until the next mornning, when her sufferings returned just the same, the puncture of course having closed. They then consented to an operation.

After the period was over, the patient under ether, a trocar and canula was passed the same as at first. The trocar was withdrawn and a grooved staff, belonging to a urethrotome, passed through the canula, the canula removed, and a fine saw passed through the puncture guided by the groove in the staff. Now, the parts being guarded well with the fingers in the rectum and sound in the bladder, the tissues first on one side then on the other were torn with the saw until a finger could pass. The breaking-down and tearing with saw and fingers was kept up until the canal was of a uniform calibre throughout and admitting easily a glass plug $1\frac{1}{4}$ inches in diameter. The os uteri was found normal but small, and admitting the probe easily for nearly two inches. The black, tarry contents amounted to probably two or

three ounces, which could not be accurately estimated as it came away during the operation.

The parts were well syringed out with warm water.

The glass plug, anointed with glycerin, was inserted the full length of the vagina and held in place by a T bandage. This was left in place until the third day, when suppuration was established, then removed and the parts washed out with warm carbolized water, and the plug replaced. This was repeated daily. She got well promptly, being up and around in less than three weeks, the vagina all healed and looking very much like natural mucous membrane; has been in good health ever since, menstruating regularly.

She wore the plug constantly for five months, then for a while only at night. Now, two years since the operation, she only inserts and removes it as she is going to bed.

One feature peculiar to this case is, that the patient uses a size larger plug now than at first, for the tendency is invariably for the vagina to grow smaller. And before the suggestions and experience of Dr. Emmet, this operation was always a failure. Closure would invariably take place when the attempt was made to maintain the parts patulous by plugging with lint or other compressible substance, or the occasional use of a bougie. His suggestions which have made the operation a success, both in his hands and others, were "to heal around glass, not to use cutting instruments, and to complete the operation at one sitting," and that wounds which are torn heal slower and there is not that tendency to contract, and when healed around glass partake more of the nature of mucous membrane.

In this case, aside from its perfect success, the main feature of interest to me, and all that I claim as original is the use of the saw, which served an admirable purpose. In the first case I had of this kind I simply used my finger-nail and the serrated ivory handle of a scalpel.

LOOMIS' FORCEPS FOR THE REMOVAL OF PLACENTA
AFTER ABORTION.

BY
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Girard, Ill.

HAVING realized the necessity of being able to secure prompt and thorough evacuation of the uterus as soon as indicated in the treatment of abortions, and having good reason to believe that the means commonly employed for this purpose frequently fail to produce the desired result, so that nature unassisted is left to relieve herself with the risk of hemorrhage, septicemia, etc., is my apology for now calling your attention to an instrument which I believe will be found the most practical of all that have been devised for this purpose, and in my experience has proved entirely satisfactory: Loomis' forceps, an instrument which is not by any means new, although it seems to be but little used by or known to the profession, and information regarding its history and use being very meagre, as all that I have been able to obtain comes through Dr. Henry A. Martin, of Boston, who described it in a paper read before the Norfolk Medical Society, Dec. 11th, 1877,¹ giving full instructions in detail as to its application, through whose statement of such satisfactory results from its use in his hands I was induced to purchase the instrument and give it a trial, and shall ever feel grateful to the doctor for the satisfaction and benefits resulting therefrom.

The instrument under consideration consists of a long, slender forceps $12\frac{3}{4}$ inches in length, having the joint near the middle and terminating at its distal extremity in short curved fenestrated blades $2\frac{1}{2}$ inches in length and $\frac{5}{8}$ of an inch in width. The proximal ends terminate the one in a ring, like the handle of scissors, the other in a hook, which fits around the ring when the hook or crotchet handle is rotated, rotation being provided for by a peculiar construction of the

¹ Miscarriage, and especially the use of Loomis' Forceps in the management of; by Henry A. Martin, p. 578, Chicago Med. Journ. and Examiner, June, 1879.

joint, whereby the blades become spooned together as it were as one blade. Fig. 1 shows the instrument in the compact form ready for introduction. Fig. 2 shows instrument with concavities of blades in opposition after introduction and rotation. The mode of operation is as follows: The patient being placed on either side with nates near the edge of the bed and knees well drawn up, the index finger of either hand, according to the position of the patient, is introduced into the vagina, that of the right hand being used if the patient is on the left side, and vice versa. The instrument, well oiled, is now to be introduced in the compact form, the concave surface of the blades passing along the guiding finger till the os uteri is reached, which it is made to enter with convex surface of the blades turned toward the posterior vaginal wall; the cervix is now gently lifted for-

CODMAN & SHURTLEFF,
BOSTON.



FIG. 1.



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BOSTON.

FIG. 2.

ward in order to bring the axis of the uterus on a line with the instrument, when, by a little manipulation, taking care to insinuate the blades between the presenting mass and the posterior uterine wall, it will readily pass to the fundus between the membranes and the uterine wall. The crotchet handle is now detached and gently rotated to the right through an arc of 45 degrees or one-quarter of the rotating space, the handles being now firmly held with both hands, slight pressure is made to the left and the blades gently expanded, rotation being kept up in the meantime till the crotchet handle has passed through an arc of ninety degrees, or one-half the rotating space; now firm pressure is made to the right, and expansion and rotation continued till rotation is checked by the stop fixed in the joint. The blades now having their concave surfaces in opposition with their convex surfaces touching the uterine cavity at opposite points, are to be closed, and will necessarily include between them

some portion of the uterine contents. The handles are now to be rotated a time or two together, to detach adhesions, and gently withdrawn, bringing away generally the entire contents en masse. The operation is to be repeated as often as deemed necessary, or as long as fragments of the placenta or membranes can be brought away, and in the early stages of pregnancy, repetition of the operation will be found more necessary on account of their greater friability, but as the introduction and operation of the instrument is quickly and easily done and attended with little or no pain, comparatively speaking, it is therefore a matter of little consequence if repetition becomes necessary.

It should be observed that the operation is to be performed with gentleness and care, and that in expanding the blades the operator should be governed by the sensation of the patient as to the amount of force to be used, which should be just enough to keep the convex surface of the blades in contact with the uterine surface. I wish here to call attention to the importance of following the instructions in detail, as already given, in regard to the operation, as highly necessary to the successful application of this instrument, and particularly to the necessity of expansion of the blades with pressure to the left and right, as already indicated; for no matter how well or how often the instrument may be introduced, simple rotation will not place the narrow blades on opposite sides of an elastic body whose diameter is equal to or greater than the width of the blades, and when it is remembered that in the majority of cases demanding instrumental interference the diameter of the object to be removed is not only generally greater than the width of the blades, but may only partly fill a cavity and be located at a point opposite the blades when introduced, it will be seen next to impossible to operate successfully without expanding the blades; yet Dr. Martin, in describing this operation, does not speak of expansion, but says, "when this is done," referring to rotation, "supposing the blades to have been fully introduced to the fundus, the contents of the uterus must necessarily be included between them." From which we are led to infer that there is nothing more to be done to make a successful application of the instrument after introduction than simply to make rotation. And yet I do not in the least doubt that with his

"ten years' experience" he has operated successfully in "hundreds of cases with invariable satisfaction and success," but I do believe he has evidently omitted to tell us all that is necessary to a successful operation. To illustrate further what has been stated, I submit the following cases from practice.

CASE I.—Jan. 10th, 1880. Mrs. McC., age thirty-seven, three and a half months pregnant. Fetus delivered an hour before my arrival. Placenta retained, which having failed to remove after the exhibition of full doses of ergot and by manipulation, I applied Loomis' forceps according to Dr. Martin, with high hopes of success, but to my utter dismay, after numerous attempts, failed completely. But after studying the cause of failure, and finding the unexpanded blades placed invariably on one side of the placenta after rotation, I attempted to bring the blades on opposite sides by combining expansion with rotation, and after repeated attempts succeeded the following day in bringing away the placenta and membranes entire, and the patient made a good recovery.

CASE II.—Mrs. Z., age thirty-eight, three months pregnant; aborted July 14th, 1880. Fetus delivered two hours before my arrival. Patient flooding profusely. After trying to remove placenta by ordinary means without success, applied the forceps, and after several unsuccessful attempts succeeded in bringing away placenta entire, and hemorrhage ceased immediately.

CASE III.—Mrs. McC., same patient as Case I., aborted at two months. After waiting an hour after delivery of fetus, the membranes retained and hemorrhage threatening, forceps applied, bringing them away in fragments after introducing the instrument three or four times.

CASE IV.—Mrs. W., age twenty-six. October 11th, 1881, flooding six hours after an abortion at two and a half months. Forceps applied, and some fragments of after-birth brought away, and hemorrhage ceased immediately.

CASE V.—Mrs. M. C., age twenty-nine. February 6th, 1882, delivered of a two-and-a-half months' fetus ten days previous by her family physician, who not being able to secure after-birth, put her upon ergot and left the case principally to nature, as is the common practice. Being called in the absence of the attending physician, found patient, who had apparently been getting along well and able to go about, now flowing profusely and the membranes just in reach of the fingers. Forceps applied three or four times, bringing the after-birth in fragments, some portions of which being still firmly attached, and hemorrhage ceased immediately.

CASE VI.—Mrs. V., age thirty. September 6th, 1882, flooding after an abortion at three months, two hours after delivery. Forceps applied and some small portions of placenta brought away, and hemorrhage ceased.

CASE VII.—Mrs. B., age thirty-eight. September 10th, 1882,

flowing profusely after an abortion at three and a half months, four hours after delivery of fetus. Forceps applied, and a fragment of placenta, which was not detected on digital examination, brought away, and hemorrhage immediately ceased.

CASE VIII.—Mrs. S., age twenty-five, aborted March 25th, 1883. Waiting an hour after the delivery of a three months' fetus, applied forceps, bringing away placenta entire, which was still beyond the reach of ordinary means.

It is worthy of remark that in the foregoing cases convalescence was rapid and recovery complete, and there was in no instance any evidence of any injurious effect from the use of the instrument, and the results were certainly as good as could be wished for.

The manner of operating that I have adopted and described is substantially the same as given by Dr. Martin, with the exception of those points that I have already called attention to, which I found by experience and experiment to be essential. With my limited experience I feel safe in asserting that the instrument will do all that it is designed for, viz., remove the uterine contents in abortions, provided the os is sufficiently patulous, and that it will be found simple, safe, efficient, and complete, being operated without the aid of other instruments.

The view of assisting, if possible, in placing this instrument before the profession in its most practical light, is my apology for offering these remarks, and should I succeed in eliciting, to some extent at least, further investigation of this subject, I shall consider that this effort has not been in vain.

AN INQUIRY CONCERNING THE RELATIVE INFLUENCE OF THE SEX OF THE FETUS IN UTERO, ON THE MENTAL, PHYSICAL, PHYSIOLOGICAL, PATHOLOGICAL, AND DEVELOPMENTAL CONDITION OF THE MOTHER DURING GESTATION, LACTATION, AND SUBSEQUENTLY.

BY

JOHN STOCKTON-HOUGH. A.M., M.D., Mag. Chem.

(Concluded from p. 517.)

16. *Influence of the Sex of the Fetus on Arresting the Growth, Development, and Nutrition of the Mother.*

THE function of reproduction, when active (that is, during gestation), is antagonistic to growth, development, and nutri-

tion, severally and combinedly. No one insists more strenuously upon the truth of this proposition than Herbert Spencer. He says: "It is a general physiological truth, that while the building-up of the individual is going on rapidly, the reproductive organs remain imperfectly developed and inactive; and that the commencement of reproduction at once indicates a declining rate of growth, and becomes a cause of arresting growth." Not only does reproduction (gestation) arrest growth and development during the *time of gestation and lactation*, but the arrest of development at the time of the first gestation precludes, in a great degree, if not absolutely, any further development subsequently.

Every one who has had any experience in the raising of cattle knows that the heifer that is permitted to breed in her second year, or before she is at least three years old, is "stunted," and retains more in size and appearance her condition as a calf. After her first gestation she neither develops nor grows. Females then should not be allowed to breed until they have reached maturity, or full development, not only for the sake of the quantity and quality of the product, but for the future welfare of the parent, and her subsequent progeny.

Women do not bear their heaviest children (among which there is a larger proportion of boys than at other periods) until they have reached maturity (ceased growing) and the menstrual function has been for a length of time well established, and the organs of reproduction have already borne fruit a few times. So great is this antagonism between reproduction and growth and development that women who menstruate early have heavier children in their first pregnancy than those who menstruate late, as may be seen from the following table of Wernich.

NO. OBS.	BOYS.	GIRLS.	MEAN
	GRAMS.	GRAMS.	BOTH SEXES.
642 1st born in general.....	3236	3117.70	3174.64.
69 1st born, mothers menstruated			
after nineteenth year.....	3166.84	3109.65	3138.24.
63 1st born, mothers menstruated			
before thirteenth year.....	3375.75	3193.29	3284.52.

From this table, it is clearly shown that boys suffer more than girls, in being the first born of mothers who have men-

struated late, there being a difference of 209 grams among the former class (boys) and only $83\frac{1}{2}$ among the latter class (girls). This fact is a further confirmation that very young women, and women who have menstruated late, have a larger proportion of girls among their first children; the production of boys being a more difficult rôle to them, the proportion increasing up to the twenty-sixth year of the mother, at which time she reaches her maximum of fecundity, according to Routh, and up to her twenty-ninth, according to Duncan. As a further explanation and proof of the greater exhaustion of the mother by a female gestation and lactation, we have elaborated from the records of the Philadelphia Hospital the following table based on 579 observations, in which the *length of body* (vertex to perineum) of the new-born child is compared with its entire length. We know of no statistics in which this point has been noted.

Table showing the numerical relation between the entire length and the length of body of the new-born child; also illustrating the fact of greater proportional length of body of female children over male, as first brought out by the author.

Average age of Mother.	Number of cases observed	Mother, single or married.	No. pregnancy.	Sex of Child.	Actual Length in Inches.		Proportion.	Weight.	
					Entire.	Body.		Lbs.	Oz.
21.92	168	S	1	F	18.95	12.43	100 : 65.59	6	13
20.90	153	S	1	M	19.42	12.59	100 : 64.83	7	7
					.47	.16	+.76		-10
22.88	48	M	1	F	18.56	12.27	100 : 66.11	6	7
23.35	54	M	1	M	19.15	12.50	100 : 65.27	6	14
					.59	.23	+.84		-7
26.26	51	S	2	F	19.30	12.54	100 : 65.07	7	9
23.70	17	S	2	M	19.60	12.66	100 : 64.59	7	12
					.30	.12	+.45		-3
26.06	44	M	2	F	19.26	12.66	100 : 65.79	7	7
23.38	44	M	2	M	19.66	12.95	100 : 65.87	7	12
	579				.40	.29	-.08		-5

There is probably an element of error in the above table, which, if eliminated, would still further increase the excess in the length of the female *trunk alone* over that of the male—1

refer to the probable greater length of head and neck (top of head to level of shoulders) in the male than in the female.

It would be more desirable to have the length of the trunk alone to compare with the entire length than the present method of taking the measurement from the crown of the head to the level of the perineum, and I would therefore suggest that an instrument be used adapted to taking this measure in obstetrical wards. A measure might be used, consisting of a Y-shaped rod, the forked end straddling the infant's neck, with projecting pieces at right angles touching the tops of the shoulders, and a sliding piece at the distal end, after the manner of a shoemaker's measure.

We read in our table that 168 unmarried woman of an average age of about twenty-one years in their first pregnancy gave birth to female children, in whom the entire length from vertex to sole of feet was to the length of their bodies (vertex to level of perineum) as 100:65.59 inches, whereas among the 153 male children born under similar circumstance, the proportion was as 100 to 64.83 only, and so on for each class, showing that in each instance *the female fetus has a longer trunk in proportion to its entire length (stature) than the male, consequently the vital organs presiding over digestion, assimilation, and nutrition have a relatively larger space allotted to them in the female than in the male.* The supply of vitality being greater, the demands of the system must be greater, increased, perhaps, by the greater demands of the reproductive system in woman. Life is longer, vitality is more tenacious among women than among men, and the same may be said of many of the lower animals.

Now we come to examine the relative influence of female fetuses as compared with males, in producing this arrest of growth, development, and nutrition of the mother. From an examination of the facts brought together in this paper, we are persuaded that the influence of the female fetus is greater than that of the male, and this opinion is based upon the following facts, viz.:

1. The probable greater and more active nutrition required by the female fetus during gestation and lactation.

2. The greater weight and surface of attachment of the placenta when a female.

3. The larger trunk of the female fetus, the vital organs consequently greater in total bulk. Greater number of females who die before labor begins. Twins of different sexes, the females sometimes sterile among cattle.

4. Longer duration of lochial discharge with females.

5. The experiments of Martegoute (see 15th proposition) showing that nursing female lambs caused a greater loss of weight than the nursing of males.

All these discriminations against the female indicate that gestations with and suckling this sex call for more nutrition from the mother than males do, and whichever requires most nutrition arrests most the growth and development of the mother. Hence, those women not yet fully mature have their development most interfered with or arrested by bearing first or more frequently girls than boys, and yet it is a well-ascertained fact that the more immature the woman is, the greater the probability of her bearing a girl, as we know the proportion of girls in such births is always larger than among women who are already the mothers of children, or have reached maturity. Why is it then that nature sets her hardest task first, and having set it, repeats this task sooner after the birth of a girl than after the birth of a boy, with the probability, however, that it will be an easier one, viz.: a boy?

We may attempt to answer this apparent anomaly by observing that the production of ova in such a vital or developmental condition as is best suited to produce males is a more difficult rôle on the part of the mother, than the production of eggs which will produce females when fecundated, as is attested by the fact that very young women, women who have menstruated late in life (in their first pregnancy), women subject to prevailing epidemics such as cholera, etc., women living in great ease and luxury in cities, all these have a larger proportion of female children than those who are older, who have menstruated early in life, or who live in a condition more allied to nature.

The only answer we can give to this question is, that changes must necessarily take place gradually, it would be impossible to have the female incapable of reproduction until the same fixed year in every instance. The fault is in making use of this function until the individual has reached maturity, at which

time nature provides that she should begin with males rather than females (that is, the proportion of males is largest at this time, hence the probability of having a male is greater than that of having a female).

The immature female is less able to produce eggs which will give males, the latter being the *higher rôle on the part of the female*. Male eggs, or eggs from which males are formed, have always been credited with being more highly developed than those from which females emerged, following the relative condition of the resultant product. Hence, the following couplet, adapted from one of the Latin poets :

In eating eggs, always choose the long,
Cock eggs are they, more nourishing and strong.

17. *The Influence of the Sex of the Child of a Preceding Pregnancy on Determining the Sex and Weight of the Child of the Next Succeeding Pregnancy.*

The following propositions have already been the subject of investigation by various authors, viz. :

1. Does the same proportion exist among the sexes in births of different pregnancies ? The answer has always been that : First (early) and later pregnancies furnish a larger proportion of girls than intermediate ones. Illegitimate births, among which a large proportion are first children, also furnish a larger proportion of girls than the proportion in the general total of legitimate births.

2. What influence does a child of one sex have on the weight of the next succeeding child when of a different sex, and when of the same sex ? This question has been answered by Wernich, from whose tables we have taken the trouble to elaborate others, in order to show the various effects of this influence.

It remains for us to show what influence the sex of the child of a preceding pregnancy has on the sex of the product of conception immediately following.

Table showing the influence of the sex of the child of a preceding pregnancy on the weight of the child of the pregnancy next succeeding, when a different sex and when of the same sex, arranged according to the number of the pregnancy. Elaborated from Pinard,¹ out of Wernich.²

Number of cases.	Variations of sex with mean weight (in grams).				Difference according to sex.
102.	1st a girl, 3073,	followed by a boy in 2d pregnancy,	3309.	+	296 gm.
105.	1st a boy, 3326,	" " a girl in 2d "	3285.	-	41 "
133.	1st a boy, 3375,	" " a boy in 2d "	3406.	+	31 "
80.	1st a girl, 3218,	" " a girl in 2d "	3263.	+	45 "
73.	2d a girl, 3257,	followed by a boy in 3d pregnancy,	3382.	+	125 gm.
87.	2d a boy, 3456,	" " a girl in 3d "	3359.	-	97 "
105.	2d a boy, 3406,	" " a boy in 3d "	3411.	+	5 "
68.	2d a girl, 3292,	" " a girl in 3d "	3289.	-	3 "
37.	3d a girl, 3271,	followed by a boy in 4th pregnancy,	3457.	+	186 gm.
56.	3d a boy, 3335,	" " a girl in 4th "	3115.	-	220 "
40.	3d a boy, 3491,	" " a boy in 4th "	3475.	-	16 "
49.	3d a girl, 3289,	" " a girl in 4th "	3263.	-	26 "

Condensation of the above table, bringing the three classes (pregnancies) together :

	1st, 2d, and 3d pregnancies; mean weight, grams.	2d, 3d, and 4th pregnancies; mean weight, grams.	Mean difference; grams.
In 210 cases, girl of 3,200,	followed by a boy of 3,383.		Excess + 183.
" 248 " boy " 3,373,	" " girl " 3,253.		Loss - 119.
" 278 " " 3,424,	" " boy " 3,430.		Excess + 6.
" 197 " girl " 3,266,	" " girl " 3,272.		Excess + 6.

Average weight of :

Boys, 3,377 gms. where the *preced'g* & *succeed'g* child'n were of a *dif. sex*
 Girls, 3,327 " " " " " " " " " " " "

150 " " " " " " " " " " " "
 Boys, 3,427 " " " " " " " " " " " "
 Girls, 3,269 " " " " " " " " " " " "

158 " " " " " " " " " " " "
 Average weight, all boys, both classes, 3,414 grams.
 " " " girls, " " " 3,246 "

168 "

From an examination of these tables, we discover the uniform rule, viz., a girl who precedes a boy is lighter in weight than a girl who precedes a girl—hence we deduce the following proposition :

¹ Art. Fetus. Dict. Encycloped. des Sci. Med., 1878, p. 486.

² Beiträge, 2; Geburtshülfe, I., 3-16, 1871.

The greater the weight of the girl, the greater the probability of her being followed in the next succeeding pregnancy by a child of the same sex, and *vice versa*, as may be seen by an examination of the following table:

								Mean of girls.
102	girls in a 1st pregn'y who	<i>preceded</i>	a boy	weighed	3,073	gms.		
80	" " 1st " " " "	"	a girl	"	3,218	"		
								<hr/>
73	" " 2d " " " "	"	a boy	"	3,257	"	+ 145	"
68	" " 2d " " " "	"	a girl	"	3,292	"		
								<hr/>
57	" " 3d " " " "	"	a boy	"	3,271	"	+ 35	"
49	" " 3d " " " "	"	a girl	"	3,289	"		
								<hr/>
								+ 18 "

Making a total for all pregnancies of:

210	gl's in 1st, 2d, & 3d pr. who	<i>preceded</i>	a b'y	weig'd	3,200	gms.	
197	" " " " " " " "	"	a girl	"	3,266	"	
							<hr/>
							+ 66 "

Hence, girls who precede boys weigh a mean of 66 grams less than girls who precede girls, and from the following we learn that boys who precede boys weigh on an average 52 grams less than boys who precede girls:

248	b'ys in 1st, 2d, & 3d pr. who	<i>preceded</i>	a g'l	weig'd	3,372	gms.	
278	" " " " " " " "	"	a b'y	"	3,424	"	
							<hr/>
							Mean of boys, + 52 "
210	" " " " " " " "	<i>followed</i>	a g'l	weig'd	3,383	"	
278	" " " " " " " "	"	a b'y	"	3,430	"	
							<hr/>
							+ 47 "
248	gl's " " " " " " " "	"	a b'y	"	3,253	"	
197	" " " " " " " "	"	a g'l	"	3,272	"	
							<hr/>
							+ 19 "

Hence:—

A boy who *follows* a girl weighs 47 grams less than

A boy who *follows* a boy.

A girl who *follows* a boy weighs 19 grams less than

A girl who *follows* a girl.

A boy who *follows* a boy weighs 158 grams more than

A girl who *follows* a girl.

A boy who *precedes* a girl weighs 52 grams less than
 A boy who *precedes* a boy.
 A girl who *precedes* a boy weighs 66 grams less than
 A girl who *precedes* a girl.
 A girl who *precedes* a girl weighs 158 grams less than
 A boy who *precedes* a boy.

Table showing the order by sex of the births most favorable to the weight of the fetus.

Grams.			Grams.		
3,430.	Boy follows	a boy.	3,424.	Boy <i>precedes</i>	a boy.
3,383.	" "	a girl.	3,372.	" "	a girl.
3,272.	Girl "	a "	3,266.	Girl "	a "
3,253.	" "	a boy.	3,200.	" "	a boy.

The weight of the fetus depends to some extent on the length of the intergestation period which has immediately preceded it, as we have shown that boys who follow boys are heaviest, and are conceived a greater length of time after the previous birth than any other combination between the sexes, thus verifying the dictum of Wernich, who declares that increase of interval between births acts more beneficially than diminution.

18. *Influence of Sex of Fetus on the Length of the Intergestation Period.*

It is a well-known fact that the period between the birth of successive children in women who are not relatively sterile, is on an average eighteen months among the middle classes generally, and twenty-odd months among the upper classes, particularly when living in cities. The number of the pregnancy, especially after the seventh, seems to have a perceptible influence in shortening the period in each successive birth, until it has dropped from twenty-one months in the seventh to fifteen months in the eighteenth, as may be seen from an examination of the following table, by Ansell, based on 25,000 observations.¹

¹ Duncan: Sterility in Woman; London Lancet, Feb. 24th *et seq.*, 1883.

Table showing the Mean Time after Marriage of successive Births, and the average interval between them.

ORDER OF BIRTH	MEAN TIME OF BIRTH AFTER MARRIAGE.	AVERAGE INTERVAL BET. SUCES. BIRTHS.	ORDER OF BIRTH	MEAN TIME OF BIRTH AFTER MARRIAGE.	AVERAGE INTERVAL BET. SUCES. BIRTHS.
1st Child	1.32 years.	—	10th Child	16.33 years.	20.0 months.
2d "	3.02 "	18.0 months.	11th "	17.65 "	19.0 "
3d "	4.83 "	19.0 "	12th "	18.85 "	19.0 "
4th "	6.69 "	20.0 "	13th "	19.87 "	18.0 "
5th "	8.53 "	20.0 "	14th "	20.71 "	18.0 "
6th "	10.28 "	20.0 "	15th "	21.41 "	17.0 "
7th "	11.92 "	21.0 "	16th "	22.01 "	16.5 "
8th "	13.47 "	20.0 "	17th "	22.54 "	16.0 "
9th "	14.93 "	20.0 "	18th "	23.02 "	15.0 "

In another table, based on 6,035 observations corrected for still-births and twins, Ansell has shown that the mean interval between marriage and the birth of the first child is nearly sixteen months; 3,159 women having borne their first child before the first year of married life had elapsed, and nearly seven-eighths of the whole number (6,035) before the expiration of the second year.

Having now determined the mean time between the birth of children of different pregnancies, *without regard to sex*, it remains for us to show *the influence of the sex of the preceding fetus on the length of the intergestation period; that is, from a birth to the next succeeding conception.* We have just shown that the sex of a preceding fetus has a perceptible influence in determining the sex and weight of the fetus next succeeding it, and now we shall show that the length of the intergestation period is influenced by the sex of the fetus at each end of this period.

We have not been able to discover the expression of any opinion on this point to guide us in our researches, and have consequently been obliged to rely upon such data as were afforded by genealogical tables. In genealogies it is usual to give the date of the marriage and the date of birth, and name of each child born, and from these facts we have elaborated a table in which are clearly shown the points we wish to elucidate. Such families were selected as offered a fair proportion of the two sexes, and any period of three years or more between the births of two children was thrown out as showing a certain amount of relative sterility, or indicating a disturbing influ-

ence, such as disease, temporary suspension of intercourse, etc., etc.

The questions to be solved, then, are as follows:

1. What is the mean time between marriage and the birth of a boy?
2. What is the mean time between marriage and the birth of a girl?
3. What is the mean time between births when a boy follows a girl?
4. What is the mean time between births when a girl follows a boy?
5. What is the mean time between births when a boy follows a boy?
6. What is the mean time between births when a girl follows a girl?

(Pregnancies of the same number only to be compared.)

Plan for a Table showing the Mean Time between Births, also the Mean Time between a Birth and the next succeeding Conception, according to the Sex of the preceding and succeeding Fetus, and the Number of the Pregnancy.

ORDER OF SEXUAL DIFFERENTIATION.	FROM BIRTH OF GIRL TO BIRTH OF BOY		FROM BIRTH OF GIRL TO BIRTH OF GIRL		FROM BIRTH OF BOY TO BIRTH OF GIRL		FROM BIRTH OF BOY TO BIRTH OF BOY	
Order & No. Preg.	M'ths.	Days.	M'ths.	Days.	M'ths.	Days.	M'ths.	Days.
From 1st to 2d b'th								
“ 2d to 3d “								
“ 3d to 4th “								
“ 4th to 5th “								
“ 5th to 6th “								
Continuing in the same manner to say the 20th Preg.								
Total all Pregnan.	22	20	23	19	25	03	38	26
Mean <i>intergesta-</i> <i>tion</i> period	13	20	14	19	16	03	19	26
[Found by subtrac- ting period of ges- tation. ¹]								
Total No. Observa- tions each category	15		17		19		18	
69 Observations.								

¹ A week less should be subtracted from gestations with girls than for boys.

Mean time from *marriage* to birth of *first child* (within 3 years) when a *girl*, — mos. — days.

Mean time from *marriage* to birth of *first child* (within 3 years) when a *boy*, — mos. — days.

I regret very much that I have not had the time nor the patience to collect and tabulate the intergestation period in a larger number of instances, involving as it does much tedious calculation, but the few observations I have tabulated in the following table, encourages me to believe that, as I predicted, the sex of the fetus has a very decided influence in determining the length of the intergestation period.

By subtracting nine months from the interval between births we get the

Mean Time between Birth and the following Conception, which we found to be as follows:

	NO. OBS.	BIRTH OF GIRL TO CON- CEP. OF BOY.	BIRTH OF GIRL TO CON- CEP. OF GIRL.	BIRTH OF BOY TO CON- CEP. OF GIRL.	BIRTH OF BOY TO CON- CEP. OF BOY.
British Peer- age.	27	7 mo. 23 d. 233 d.	5 mo. 23 d. 173 d.	10 mo. 27 d. 327 d.	14 mo. 23 d. 443 d.
Hyde Gene- alogy.	69	13 mo. 20 d. 410 d.	14 mo. 19 d. 439 d.	16 mo. 3 d. 483 d.	19 mo. 26 d. 596 d.

On examination of the above table, we discover that a woman conceives after the birth of a girl in 6 months 23 days to 14 months 4 days, according to the sex of this conception, and in 12 months 25 days to 17 months after the birth of a boy.

This difference in the length of the intergestation period is further corroborated by an examination of Ansell's table just cited, where the longest intergestation period is shown to be between the birth of the sixth and conception of the seventh child, or $21 - 9 = 12$ months, showing a gradual increase from marriage to this period, and a gradual decline after it. Now it is known that, in general, women who marry at the usual age of eighteen to twenty-five, and live in wedlock until the climacteric, have a larger proportion of girls among the early and later children than among those born in the middle of the child-bearing period, thus confirming the results of our tables, though we have dealt with such a small number of cases.

Hence we conclude that women generally who conceive within two years after marriage, of a girl in their first preg-

nancy, will have a greater number of children and at shorter intervals, among which there will be a larger proportion of girls, than the woman who conceives under similar circumstances of a boy.

The sex, then, of the first child, is an index of the sex of the majority of the children the woman will bear, as well as an indication of her prospective fecundity, and comparative length of the intergestation period.

The woman who bears boys exclusively will have fewer children, and longer intergestation periods than the woman who has children of both sexes, or girls only.

Mr. Robertson,¹ in "An Attempt to Demonstrate the Law which Regulates the Intervals of Conception in the Human Female," says that the she-ass goes ten months and receives the male the seventh day after delivery, and the mare goes eleven months, and receives the stallion on the ninth day after delivery. In the cow, gestation lasts nine months, and she conceives three months after calving. The sheep and goat go five months, conceiving seven months after delivery—while the human female goes nine months and conceives nine months after delivery, and nurses a year to fifteen months.

Among the lower animals that depend upon grazing, when in a wild state, there is a tendency to bring forth the young in the spring when nourishment is more plentiful. The longer the period of gestation, the shorter the interval between birth and the following conception. The mare goes eleven months and must conceive within the month following, or she would bring forth later and later each successive year.

Among savage and barbarous peoples, it is not unusual to suckle children from three to five years, or even indeed until the mother becomes pregnant again; and, in some cases, going so far as to have the infants of two pregnancies at the breast at once.

Mr. Robertson, believing that suckling had some influence on the length of the intergestation period, questioned one hundred and sixty women, of an average age of thirty and one-half years, having given birth to six hundred and eighty-six children, or four and one-half each, five hundred and twelve of whom lived to be weaned, or three and one-half for each. Average

¹ Edinburgh Med. Jour., 1832, pp. 1-11.

age at weaning was fifteen and one-half months. Of these women, eighty-one became pregnant once or oftener during lactation, and seventy-nine had never conceived during lactation. Of the eighty-one who became pregnant during lactation, twenty-seven, who had produced one hundred and one children, had conceived on thirty-eight times during lactation. That forty-two who had always conceived during lactation, did not, on an average, conceive until they had suckled nineteen and one-third months nearly; and that twenty-two, the remainder of the eighty-one, had been in the habit of conceiving during lactation, and soon after parturition. The average age for weaning was different in the two classes of women—in those who had not conceived during suckling, and those who had conceived once or oftener during suckling. For the seventy-nine women composing the former class, it was fourteen and one-fourth months; and for the eighty-one women of the latter class, it was fifteen and two-thirds months.

“The *first* corollary,” says Robertson, “which I would draw from the foregoing fact is, that in seven out of eight women who suckle for as long a period as the working classes in this country (Scotland) are in the habit of doing, there will *elapse an interval of fifteen months* from parturition to the commencement of the subsequent pregnancy.

“*Second*, That, in a majority of cases, when suckling is prolonged to even nineteen or twenty months, pregnancy does not take place until after weaning.

“*Third*, That lactation has an influence on the generative function; that, up to a late period in suckling, probably in this country about fifteen months, conception does not, in general, occur; and that hence we are warranted in regarding the secretion of milk as the *cause* which regulates the periods of conception in mankind, as instinct operates to the same end in graminivorous quadrupeds, and probably in all other animals.”

The tables to be found under Proposition 18, though founded on too few observations to be infallible, are yet interesting as showing that, among the peeresses of England, the intervals between birth and the next succeeding conception, though they have fewer children, was $s \times$ months, or nearly one-third shorter among the mothers of New England—a difference which we are inclined to ascribe to the probable fact

that a much larger proportion of New England mothers suckle their children (at least at the time of the births from which these tables were made, viz., one hundred years ago). As we know that peeresses have fewer children, and from the above, as it appears, at shorter intervals, we are forced to conclude that among them the entire child-bearing period must be much shorter than among the women of New England, one hundred years ago.

In conclusion, the following questions present themselves, viz.:

Does suckling a female exhaust the mother more than suckling a male?

If so, what effect has this greater exhaustion in accelerating or retarding the next succeeding conception, that is, lengthening or shortening the intergestation period?

In answer to the first, we have only to repeat Prof. Martegoute's conclusion from his experiments with sheep, in which he found that female lambs exhausted the mother more than the suckling of males. Notwithstanding this greater exhaustion from suckling females, from our tables we learn that a woman conceives from three to six months sooner after the birth of a girl than after the birth of a boy, from which we must conclude that this exhaustion, or rather the causes which bring it about, determine in the woman a genetic tendency which may be defined to be a disposition to reproduction rather than to nourishment. This corresponds with what writers on population tell us, viz., that years of famine (necessarily attended by a certain amount of physical exhaustion from lack of food) are followed by increase in births and in the proportion of male children, thus confirming the law that reproduction and growth are antagonistic.

19. *Influence of the Sex of the Fetus on the Mind of the Mother.*

Thus far, we have omitted to discuss the influence of gestation and lactation on the mind of the child-bearing woman. Puerperal mania seldom appears, if indeed at all, before labor sets in. And from Esquirol we learn that out of every twelve women who are insane, one owes the origin of her malady to child-bearing. Of those women who have puerperal mania, two-

fifths were attacked within the first fifteen days after labor ; one fifth from the fifteenth day to the end of the second month ; another one-fifth after the third month ; and the last one-fifth immediately after weaning. It remains for us to inquire what influence nursing has on producing or aggravating this malady, and finally, whether those women who have given birth to and nurse girls are not more frequently attacked by this form of insanity than those who bear and nurse boys, as the testimony of the facts brought forward in this paper would seem to indicate, particularly if there be, as we believe, a traceable relationship between this malady and the nervous and physical exhaustion of the mother.

20. *Influence of Co-Twins of Different Sexes on the Mother and on Each Other.*

That the opinion that one of a pair of twins is sterile is not a modern discovery may be gathered from the following, out of Joubert:¹ S'il est vray que de gemeaux l'un est inepte à engendrer ; et semblablement des gemelles, l'une est inepte à concevoir : et si les gemeaux ne peuvent faire d'autres.

Free-martin is a name given to a female calf born co-twin with a male, and almost always sterile from this fact, while the male co-twin is able to engender.

This fact seems to indicate that where a male and a female fetus are exposed to exactly the same conditions during gestation, it is the female which fails to come to perfection, possibly from lack of nourishment, or some influence of the male co-twin on the mother, reacting on the female. There may be a developmental prepotency on the part of the male twin.

I know of no statistics in which the relative mortality before birth of co-twins is mentioned, but we have shown in this paper that the number of females (in single births) who die during gestation (before labor) exceeds the number of males, and we believe that this excess is still greater in twin births of females than with males. The greater exuberance of vitality of ovum destined to produce a female is further shown in the fact that double monsters are four times as frequently females as male. Twins, of whatever combination of sexes, suffer from this fact alone, it being impossible to produce two crea-

¹ Erreurs populaires, etc. 8vo. Paris, 1586. 2d pte., p. 141.

tures as perfect and vigorous as if only one had been born. This view is strengthened by the observations of Arthur Mitchell,¹ who "shows that twins are peculiarly liable to be imbeciles or idiots." The conclusions of Mitchell's paper are so pertinent to the present subject that I quote them here at length. 1. "Among the imbeciles and idiots a much larger proportion is usually found to be twin-born than among the general community. 2. Among the relatives of imbeciles and idiots twinning is also found to be very frequent. 3. In families, when twinning is frequent, bodily deformities (of defect and of excess) likewise occur with frequency. 4. The whole history of twin births is exceptional, indicates imperfect development and feeble organization in the product, and leads us to regard twinning in the human species as a departure from the physiological rule, and therefore injurious to all concerned. 5. When we pass from twins to triplets and quadruplets, everything we know regarding these latter gives support to the general conclusions in question."²

In conclusion, I cannot too strongly urge the importance in all statistical tables of separating the sexes, and in individual cases to state the sex of the child. In all statistical work, separate tables should be made for each sex, so that inquirers of all classes may be able to make a selection of such facts as they may need to illustrate any subject from the point of view from which they may wish to regard it. In all physiological inquiries, the results should be tabulated by sexes. In cases of placenta previa, extrauterine gestation, and *phlegmasia alba dolens*, the sex of the child is seldom stated in the reports of cases. Dr. Parry was only able to find the sex stated in eighty cases of extrauterine gestation, and I have only been able to discover the same number of cases of placenta previa wherein the sex was stated.

There is an interesting field of investigation open in the study of the *placenta*, regarding its *site*, *weight*, and *shape*, by sex of fetus. Who will prove what we conjecture, viz., that the placenta of the female fetus has a larger surface of attachment to the womb than that of the male?

¹ Medical Times and Gazette, November 15th, 1862.

² Duncan: London Lancet, March 24th, 1883, p. 492.

OBSERVATIONS REGARDING THE EFFECTS OF TRACHELORRHAPHY ON FERTILITY AND PARTURITION (CONTINUED).

BY

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IN the number of this JOURNAL for January, 1883, I promised to continue my observations regarding the effects of trachelorrhaphy on fertility and parturition, and, having waited to hear what other clinicians had to say upon that subject, it may not, I hope, be irksome to the readers of the JOURNAL OF OBSTETRICS to take up the subject where it was interrupted, and continue it to the end. If I have gained nothing else, I certainly must take to myself, egotistical though it may seem, the credit of awakening among gynecologists some zeal in the investigation of this important matter. The object of this communication is not so much to play the part of a whetstone, as to put myself in such a position before the profession as my ideas inculcate. My paper certainly could not carry the impression that I am opposed to Emmet's operation for the repair of an injury that, as every one must admit, is imperative, and which cannot be relieved without such treatment. "Emmet's operation is doubtless the treatment in extensive laceration of the neck of the womb," as I stated before, and if gentlemen will only keep that factor in their minds while discussing this subject, we will save much idle talk, and arrive at just conclusions. I am not considering the question as to the feasibility of the operation, its necessity, and its success in relieving certain morbid conditions. That subject has been fully and ably settled in the columns of this JOURNAL. The same subject is discussed in the journal of the American Medical Association, Vol. II., No. 8. "His (my) arguments, which appeared to me at the time erroneous, have been so completely refuted by the statements to the contrary published in the same journal and in many personal communications from prominent operators, that I only refer to the subject in passing, and will now proceed to the narration of the cases which form the basis of this paper," says

Dr. Joseph Taber Johnson, of Washington. The narration of five cases ensues, but not a word about fertility and parturition, the only questions at issue; not an examination recorded as being made subsequent to the operation. Only the brilliant success is a matter of history. Did stenosis of the cervical canal follow any of these cases reported? By what mental process is the conclusion arrived at that a woman's condition is rendered worse by the operation should sterility ensue or a subsequent laceration occur? It is admitted by competent authority (Baer) that the condition existing prior to the operation predisposes to sterility. Therefore sterility ensuing cannot make matters any worse. If laceration should recur, would that militate against a second operation? After the operation for restoration of the perineum, it frequently happens that relaceration accompanies delivery. This certainly ought not to debar a woman from being relieved by a second operation of a train of symptoms following this injury.

It is an admitted fact that under almost every conceivable condition of the cervix pregnancy may follow, and hence there is no legitimate argument to use except the *post hoc ergo propter hoc*—the operation cured the sterility. It is also conceded that lacerations, more or less extensive, occur in thirty per cent of women delivered at full term, and my experience is that, just as in lacerations of the perineum, there are few that escape without injury. It is true that the injury may be so slight as to cause no trouble, and it may never call for surgical interference. When the rent is extensive, and the operation is demanded, there exist, usually, such pathological changes in the surrounding structures as tend to sterility, and no matter how careful the dissection and the apposition of the parts, it is hardly possible to conceive why the cervical canal should not be narrowed and partial occlusion result. For this reason, if all the cicatricial tissue be removed, no matter how large a space you may leave for your *externum os uteri*, the coaptation must necessarily bring the edges of the entire rent together, and, if union follow, the entire denuded surface must unite. If you leave a portion undenuded, you are only partially performing the operation. I have seen stenosis follow operations of this kind even in the hands of the most skilled gynecologists. The new tissue is inelastic. If pregnancy

follow, there will be subsequent laceration, owing to the non-yielding of this tissue. I am not now speaking of the superficial rents due to a rigid cervix, where either the head, or more probably, the shoulders, exercise unequal pressure in certain directions in their transit through the maternal outlet. I only wish to speak of the deeper lesion, where the tissue is so separated as to produce eversion of the lips and consequent subinvolution and its concomitants. These pathological changes give rise to a degree of induration and hyperplasia that the operation cannot remedy, though by restoring the impeded circulation it can correct the malnutrition pre-existing. It is hardly possible to conceive that should pregnancy occur following an operation in this class of cases, that even with the softening of the cervix which accompanies gestation there will not be more or less dense tissue which will impede labor to a greater or less extent and, by its unyielding nature, predispose to relaceration. This argument in itself would be sufficient proof why parturition following repair of the cervix in the cases indicated should be prolonged and difficult. Dr. Goodell, whose authority, I think, demands equal consideration with that of the gentlemen from Syracuse and Washington, says that he has operated one hundred and sixty-nine times, and believes that the influence of the operation was to cause sterility. Out of these one hundred and sixty-nine cases, he only knew of seven (7) to become pregnant. Of these seven, two sustained a second laceration, and demanded a second operation. If such testimony be true, and I think that no one who knows Dr. Goodell would be likely to gainsay it, my thesis is proven, and this, too, from statistical evidence, as I have neither the presumption nor the desire to put my own preconceived ideas in opposition to authority. It is a source of daily pride and congratulation that, in common with the entire medical profession, the library of the Surgeon-General's office is at my disposal. Those who glean extensively from individual collections can never appreciate or understand its value. But let us pursue the argument a little further. In the *Medical News*, February 24th, 1883, Dr. Baer says: There is no doubt of the truth of this statement, "that repair of laceration of the cervix uteri is usually followed by sterility," and he accounts for it on the ground that a persisting cause for sterility

following the operation was found before the operation was accomplished. I agree fully with him, that the longer the operation is delayed, *cæteris paribus*, the much less likelihood is there of subsequent pregnancy. I regret that he should suppose that I am opposed to Emmet's operation, *e contra*, as hereinbefore stated. "Of the twenty-seven cases in which I have made the operation," says Dr. Baer, "six were either widows or had reached the menopause, and they must, therefore, be excluded from the analysis. This leaves twenty-one cases to be reported upon in this inquiry. Of these twenty-one cases, thirteen had been sterile from five to sixteen years prior to the operation, and I think, for reasons which will be stated further on, that they ought also to be classed as beyond the probability of becoming pregnant. In the remaining eight cases, pregnancy had occurred within five years, but had resulted in abortion in five. In twelve of the twenty-one cases, from one to five abortions had occurred, in each, subsequently to the occurrence of the laceration." This gives abundant proof of the ill effects of the lesion and its results, subinvolution, hypertrophy, cellulitis, oöphoritis, etc., on fertility. Then follows the report of four cases when pregnancy supervened, and when delivery was normal. These facts speak for themselves. The gentleman from Syracuse who accuses me of running to the library of the Surgeon-General's office for information, and arriving at break-neck speed conclusions, reminds me very forcibly of another distinguished personage from a city of the same name. "I make the operation," he says, "to cure sterility. I am able to show that my results are excellent. I think this is due to the restoration of the canal of the cervix as a cavity of the uterus." How a canal of the cervix can be such and at the same time a cavity of the uterus I leave to the reader to interpret.

The following is Table V., to which he refers me for information, *vide* AMERICAN JOURNAL OF OBSTETRICS, July, 1883, p. 688.

Table V.—Relative Fertility Before and After Operation.

Number.	Duration of laceration. Years.	Number of children during laceration.	Time since operation. Years.	Number of labors since operation.	Number of abortions since operation.	Remarks.
2	5	1	$\frac{1}{2}$..	1	Aborted at 2 months.
14	10	1	1	1
18	6	2	1	1
20	5	..	$1\frac{1}{2}$	1
21	8	1	$1\frac{1}{2}$	1
22	1	..	1	Aborted at 4 months.
25	7	..	1	1
26	13	..	4	1
30	1	..	1	1

Why not give the histories of these cases? In all but one case the rent existed for over five years, but as the doctor has a way of operating peculiar to himself, I suppose fecundity might ensue even after the menopause. "I am not able to adduce as marked testimony concerning the renewal of the laceration as I could wish. Of the four patients whom I examined after labor, two suffered renewal of the laceration, and two showed the parts intact. The forces that ruptured the cervix in the first instance may also be present with equal force at the first labor subsequent to the repair, and renew the injury. From what I have seen and heard of the operation, I should say that the cervix ruptured in about half the cases at the subsequent labors. At any rate, the marked benefit from the operation in suppressing reflex neuralgia and restoring checked nutrition, is so great that the behavior of the cervix at subsequent labors has but little to do with the question of its expediency." This should be read with interest, as the antithesis is a marked one, between the autocracy of a previous absolute statement and this negative peroration. I am obliged to the doctor for so completely bearing me out in these, his after-considerations.

My assistant, Dr. H. L. E. Johnson, has taken the following statistics from the records of the Columbia Hospital, from May 15th, 1882, to March 20th, 1884. Out of 175 lying-in patients examined after normal labor, about three weeks following delivery, 84 had the cervix uteri intact, and 91 had well-marked cervical laceration, more or less extensive. I also beg leave to append some excerpts from special literature bearing upon the

question at issue. I think that any argument based upon a resultant sterility will fall to the ground through lack of cohesion, and the moral hesitancy of the gentleman from Washington to operate upon any case in which the possibility of sterility might present itself, becomes simple and idle prejudice, or the avowal of a singular forgetfulness of circumstances. The woman is already sterile, and yet he would consider the operation criminal which maintained the same state of affairs. A year's consideration of the objections raised by me, together with the digest of current literature, only strengthen the conviction which has created so much earnest discussion.

Thirteen years of hospital life has taught me to go slow, and not mistake a hypothesis based upon any man's dictum for a conclusion founded upon fact. Such an important consideration of the operation of trachelorrhaphy, one of the most serious questions among the many which go to make up the compact history, cannot be dismissed in a few lines of empty criticism. Neither will the incomplete statistical tables of an individual operator be accepted as in any way conclusive. "Time at last sets all things even."

Dr. Cleveland, speaking before the Obstetrical Society of Cincinnati, regarding the operation of trachelorrhaphy in general, remarked: The operation, as reported, recommends itself, and what he had to say would be on the general subject of trachelorrhaphy. He spoke with diffidence in discussing the subject, because he had not yet performed the operation, but he thought it was quite probable that it would follow the same evolution that the section of the cervix had, undeservedly popular at first, then fall into disrepute, after a while finding its true level, viz., a valuable measure in a few properly selected cases. That the operation of trachelorrhaphy had been abused can hardly be denied, nor will any one deny but that in some cases it relieves and even cures the patient of the chain of symptoms for which the operation is performed; but it is hard for one who is in the habit of observing the condition of the womb in child-bearing women to believe that this operation is required as frequently as most of the specialists would lead us to believe.

How often do we see women with badly torn cervices that pass life apparently without inconvenience and are fruitful; in fact, laceration is very frequent, and it is not often you see the cervix of a multiparous woman that is not more or less torn. Among the women that I see it is seldom that I meet a case that I think calls for an operation. An incident bearing on this subject, which I will relate, illustrates how easy it might be even in this operation to mistake the *post* for the *propter*. A patient of mine, the

mother of one child, had a badly lacerated cervix, her child was a year old, and she had never been restored to her usual health after the birth of her child. She had leucorrhea, uterine enlargement, tenderness over the ovaries, and pelvic neuralgia. Here, I thought, was a suitable case for operation, and I prepared to operate, but when the time approached, the patient declined to be operated upon. She was put upon tonic treatment, and electricity was used two sittings a week of the constant current. She began to improve, and entirely recovered, in spite of the torn cervix. Now, if she had been operated upon, she would probably have recovered all the same, and the case would have been recorded as a case cured by trachelorrhaphy. I have no doubt but that in many cases reported as cures, the operation is really of secondary importance, if not positively harmful, while the improved management and careful treatment of the patient while she is under the charge of the physician, is the real agent of cure.

DR. FORD reports a case operated on by him, woman, aged thirty-two, laceration in labor; operation; pregnancy; delivery; *no laceration*.—*Obstetric Gazette*, vi., 1883, p. 354; *St. Louis Obstet. and Gynec. Society*; *St. Louis Courier of Med.*, ix., 1883, p. 542.

DR. B. F. BAER looks on the sterility following Emmet's operation as not due to the operation itself, but to the pathological conditions which existed with the laceration and which were frequently not relieved. If five years or more expire after the laceration before the operation is performed, sterility is likely to remain.

He reports four cases of pregnancy afterwards, but does not say whether or not the labor was attended with a relaceration.—*Med. News* (Philadelphia), xlii., 1883; *Trans. Obstet. Soc., Philadelphia*, p. 724.

DR. W. BALLS, Headley, says, p. 492: "The one objection that may be raised to the operation is the possibility of a recurrent laceration in subsequent labor. My experience extends to only one case, though others are pregnant; and in this case the os was so lacerated in the ensuing confinement. She was one of my first cases, and, perhaps, less satisfactory than most of the later ones.

"Mrs. B., aged twenty-eight, has had three children, the last two years ago, at the seventh month. Her confinement lasted three days. She had great loss of blood, but a good recovery. The cervix was lacerated to the vaginal junction. The lips were everted and raw-beef looking, with chronic metritis. She could not walk, and lay on her bed almost constantly. Pulse 120, vaginal temperature 100°. After some alleviating treatment, which did not much improve her general condition, I operated. She had some metritis, but ultimately did well. Three months afterwards I noticed that the os was of a healthy size, very slightly raw, but healing. There was slight laceration at the left side, but very fairly healed. The uterus was anteverted and the walls were thick.

She was not on good terms with her husband, and after a solitary coition in the following month became pregnant and was confined of a fine child. I was absent and she had a good and easy labor, but the old wound reopened and there is now some low metritis which does not prevent her from walking.

It appears to me that the possibility of a recurrence of a rupture should not be deterrent of the operation, for the following reasons:

1. The patient is so suffering that her condition should be relieved.

2. She may not become pregnant.

3. Should she become pregnant the cicatrix may not yield.

4. Should it yield, the condition is no worse than before the operation, but probably better, by the absence of a yet larger split.

5. The operation is something similar to that for cancer which may be repeated if the abnormal condition be recurrent.

6. The trouble of the operation is incomparably less than that of being locally treated for an unlimited time in the ordinary method.

7. The danger of the operation, carefully performed, is less than that of the original condition, abortions from metritis, etc., or from subsequent cancer, etc."—*Australian Med. Journ.*, 1882, iv., p. 486.

DR. E. VAN DE WARKER reports a case (Case XVIII.), woman age thirty, three children; laceration; with first and lasted six years; operation; pregnancy; delivery; *laceration* nearly complete on left side.

Another case (Case XX.), woman age thirty-five, two children; laceration with second and lasted five years; operation; pregnancy; labor; *no laceration*.

Another case (Case XXV.), woman, aged thirty, one child, lacerated, labor instrumental, laceration lasted seven years. Operation; pregnancy; delivery; *no laceration*. Dr. Van de Warker says of his own experience that of four patients, on whom operation had been performed, and he attended in labor, two had a new laceration and two had not.—*AMER. JOUR. OBSTET.*, xvi., 1883, p. 684.

DR. AGATHONOFF reports results of the Gynecological clinique of Prof. Slavjansky of the Academy of Medicine of St. Petersburg.

Case of woman who had not conceived for some years, in consequence of laceration of neck due to labor. Operation. One year afterwards she was pregnant again and had a happy delivery. But there was a *new laceration*.—*Annales de Gynécologie*, Paris, xx., 1883, p. 16.

DR. B. F. BAER reports cases of laceration of cervix with operation and subsequent pregnancy.

CASE I.—Woman, aged thirty, one child; laceration; subsequently several abortions; operation; pregnancy; normal delivery; *no laceration*.

CASE II.—Woman, aged twenty-one, one child; laceration; operation; pregnancy; spontaneous delivery; two months afterwards there was found a very slight fissure on left side.

CASE III.—Woman, aged thirty-four, eight children; laceration; operation; pregnancy; natural labor; no laceration.

CASE IV.—Woman, thirty-five years, seven children; laceration; operation; pregnancy; history not completed.

DR. W. H. H. GITHENS, at same meeting, reported a case of pregnancy after operation for laceration of cervix. Easy delivery and *no laceration*.

DR. E. E. MONTGOMERY remarked that one-sixth of the patients he had operated upon within four years had since become pregnant.

He reported a case similar to that of Dr. W. H. H. Githens. He remarked that, as regarded the question of sterility resulting as a consequence of the restoration of a lacerated cervix, he had been operating since 1879, and five of the patients he had operated upon had since become pregnant. The first patient upon whom he operated became pregnant lately, but aborted; as she had desired not to become pregnant, and was anxious that an abortion should occur, he believed that it had been artificially produced. Another patient operated upon in 1880 had been delivered in January, '83, *without accident*. A patient operated on in '79, was now four months advanced in pregnancy. Before the operation she had aborted at the third month; this accident was apparently consequent on the existence of the laceration.

DR. ALBERT H. SMITH at same meeting, reported that there had been no tendency to relaceration in the same position. He used inhalations of chloroform, and hot water douches in such cases and did not rupture the membranes early; he also prevented the patient from bearing down, and so secured a slow and safe labor. Dr. Wm. Goodell, at same meeting, said that he believed that the influence of the operation was to cause sterility. He had operated one hundred and sixty-nine times, and had only known of seven patients who had since become pregnant. In two of the seven cases a second operation was required, but it was slight. In one case not the slightest change occurred in the form of the os. He believed that the laceration predisposed to cancer. Dr. Smith was not prepared to accept the statement as to the laceration being a predisposing cause to cancer; his experience was different.—*New York Medical Journal*, Vol., xxxviii., 1883, pp. 48 and 76, *Obstetrical Soc. Philadelphia*.

DR. GEO. H. GREEN (*Detroit Lancet*, vi., 1882-3, p. 489): "Dr. Martin sewed up a lacerated cervix where the woman was three weeks advanced in pregnancy, and she went to full term and was delivered all right."

DR. WHARTON SINKLER had three patients who had been operated upon for laceration of cervix; one of them by Dr. Goodell. All of them had since become pregnant.

DR. J. B. HUNTER said he delivered a woman in June last, on whom he had formerly performed an operation for a severe laceration of cervix, and also for a complete laceration of perineum.

Child, born at full term, weighed seven pounds. Neither the cervix nor the perineum gave way, although the latter had a narrow escape. He mentioned the case, because so many physicians feared a reproduction of the injury in delivery after the operation. This was only one of several which he had seen, in which no injury was done to the repaired laceration in subsequent labors.—*AMERICAN JOURNAL OF OBSTETRICS*, January, 1883. Transactions of N. Y. Obstet. Soc. Meeting Oct. 3d, '82.

Same Meeting. DR. A. J. C. SKENE. He had seen several cases of successful delivery *without further injury*, after the operation for laceration of the cervix and perineum, and regretted not having notes of them with him. He had seen several cases of delivery after restoration of the cervix, and had seen partial laceration follow, but never a complete bilateral laceration. He could readily understand why this should be so; for with the development of the uterus during pregnancy the compensation of normal tissue would be so great, and scar tissue so insignificant that there could be no more reason why laceration should occur, than in a case in which it had not happened before.

Same Meeting. DR. A. S. CLARK (with reference to the existence of cicatricial tissue some time after restoring a lacerated cervix) remarked that, about five years ago, he assisted Dr. Skene in restoring a cervix badly lacerated bilaterally, and he was sent for in June last, to deliver the same woman, but, when he arrived, the child was born, labor having been very rapid. The child weighed ten pounds. There was no laceration. He examined the woman again lately, and no trace of a laceration could be found. He thought that if any cicatricial tissue from the old laceration had been present at this rapid dilatation and delivery, it would certainly have given way.

Same Meeting. DR. H. T. HANKS said that in one of the first cases he ever operated upon for laceration of cervix uteri, he delivered the patient of a child about eighteen months afterwards without any injury to the cervix. Since then he had had a similar experience in several cases, one of which he remembered reporting when Dr. Emmet read his second paper on the operation.

Same Meeting. DR. LEE, two years ago, performed an operation in the Woman's Hospital on a patient who had a very extensive double laceration of the cervix, so that very little of the true cervical tissue remained after the repair. An excellent result was obtained. He was particularly interested in the case, as its laceration had been so extensive, and she was a young woman and expected to bear more children. He was engaged to attend her in confinement last summer, as she feared a recurrence of the laceration. Being unable to attend her himself, his associate, Dr. Swasey, was present and reported that no laceration had occurred. Dr. Lee examined her very carefully afterwards himself, drawing down the cervix with a tenaculum, but was unable to find any laceration.

Same Meeting. DR. M. A. PALLAN. With regard to subsequent delivery without injury after operation on the cervix, he

had met with several such cases—at least half a dozen—in his own experience. Some patients he attended at *two* subsequent labors, and no laceration took place. Last February he closed a double laceration of the cervix, and in July last attended the patient in labor. No laceration occurred either of the cervix or perineum, both of which he had operated upon for laceration.

REPORT OF THE PROGRESS OF GYNECOLOGY AND OBSTETRICS IN FRANCE.

BY

A. AUVARD, M.D.,

Chief of the Histological Laboratory at the Clinique D'Accouchements, Paris, France.

I.—LITERATURE. Dr. Eustache, of Lille, has published an essay on the lochia and micro-organisms.¹ From this lengthy monograph I would only notice what concerns the microbes. He finds them in both normal and pathological lochia, and thence draws the conclusion that they are not pathogenetic factors. He then pronounces himself in accord with Béchamp's theory of microzymes, and grants that these microbes originate from the organism itself, that their birth-place is the uterus. In a word, M. Eustache returns to old theories, abandons the doctrine of hetero-infection, and believes only in auto-infection. In an excellent answer, Dr. Doléris,² who has specially studied the microbes of puerperal septicemia, has refuted without difficulty the various arguments of Dr. Eustache. He has shown that what the Lille accoucheur took for microbes are simply fatty granulations, an error into which he would not have fallen had he, in addition to the microscope, employed culture tests, which alone are capable of giving definite information in regard to the inferior organisms.

It is evident from this discussion that there can exist no serious argument against the opinions generally in vogue to-day concerning puerperal septicemia, opinions on which the antiseptic system is based. The backward step which Dr. Eustache has endeavored to take will not be imitated in France, where all accoucheurs are confirmed antisepticians.

A discussion on the subject of Battey's operation was provoked at a meeting of the Surgical Society (March 5th, 1884) by M.

¹ Archives de Tocologie, 1883.

² Annales de Gynécol., February and March, 1884.

Pozzi. This operation has been but seldom performed in France, and therefore it may prove of interest to note the opinions of our surgeons on this subject. The case related by M. Pozzi concerned a patient with swelling of the ovary and, at the same time, hemianesthesia and hemiplegia. He removed the ovary, and the symptoms disappeared. M. Ferrier said that he believed in the operation under certain conditions, although he had recently removed an ovarian cyst, and after the operation the patient had had severe hysterical attacks, a proof that the operation may occasionally lead to a different result from the one intended. Mm. Reclus and Polaillon also pronounced themselves in favor of the operation; M. Gillette alone showed little confidence in its possible results.

Dr. Siredey, physician to the Lariboisière, and for a long time in charge of its obstetric service, has just published, under the name of "*Puerperal Diseases*," a treatise on puerperal septicemia. The work is nothing more than an excellent exposé of the opinions generally accepted; the old ideas of epidemicity, of contagion through the atmosphere, of puerperal poison, being replaced by the existing theory of microbes implanted by direct contact on the genitals of the lying-in woman.

On the strength of a woman with practically normal external organs of generation and absence of internal, M. Pozzi has formulated a new hypothesis concerning the formation of the hymen. The majority of embryologists consider this membrane as formed from the inferior portion of the vagina, being nothing less than the narrowed vaginal canal projecting towards the vulva. Pozzi's patient had no vagina, and yet a membrane analogous to the hymen. The hymen existed, and thence the author concludes that these two parts (hymen and vagina) of the genital apparatus are independent. He makes the hymen the analogue of the corpus spongiosum of the man. To demonstrate the truth of this assertion, it must be shown that Pozzi's patient had a true hymen and not a simple pathological band; and, in the second place, it seems to me rather hazardous to draw the analogy in view of the different formation of the hymen and corpus spongiosum, and in view also of the fact that woman already possesses a true corpus spongiosum.

II.—CLINICAL OBSERVATIONS. At the different Paris maternities the bichloride of mercury (1-1,000 to 1-2,000) still remains the favorite antiseptic. Having noticed that this solution frequently gave rise to an erythema, M. Pinard has substituted in his service the following which, while equally antiseptic, is less irritating:

R	Biniiodide of mercury	5 grams.
	Iodide of potass.	5 “
	Water	10 litres.
	Fuchsine, sufficient to color.	

Dr. Charpentier (Acad. of Med., March 4th, 1884) prefers as an antiseptic the sulphate of copper. Whilst substituting for two months at the *Clinique d'Accouchements*, he used it in a solution of 1 to 100. It is perfectly harmless, absolutely antiseptic, and is said to be equally efficacious for vaginal and uterine douches as well as for washing the vulva.

During the past three months three new cases of rupture of the uterus have been noted at the *Clinique d'Accouchements* (service of Prof. Pajot) and at the *Maternité* (service of Prof. Tarnier). Of these three cases two recovered.

In the fatal case the rupture implicated, not only the anterior wall of the uterus, but also the neighboring portion of the bladder, so that the urine passed directly into the peritoneal cavity. It was proposed to do laparotomy, the woman's only chance of cure, and suture the vesical rent, but she was in such a low state that all surgical interference was contraindicated.

Of the two other cases, the rupture was complete in one, and apparently incomplete in the other, although no exact diagnosis was made; the tear was lateral, and seemed to extend into the broad ligaments without implicating the peritoneal cavity. In neither of these three cases was drainage attempted. The treatment for the first day simply consisted in uterine irrigation with Van Swieten's solution (sol. hydrarg. bichlor., $\frac{1}{1000}$); afterwards the injections were vaginal. At the *Maternité* these injections were given very frequently, at least every hour, and during the first day nearly every half-hour. At the *Clinique* the injections were given less frequently; twice a day, at times only once. The further treatment consisted in ice over the abdomen, quinine, and alcohol.

Adding to these cases the two seen at the *Maternité* during 1883, we have a total of five cases with four recoveries. And this, thanks to careful antisepsis by means of corrosive sublimate and without peritoneal drainage.

Dr. Pinard reported before the Academy (February 19th, 1884) the following case: A woman thirty-one years of age conceived for the third time in November, 1882. At term a false labor set in; the diagnosis of extrauterine pregnancy was made. Two months thereafter rapid swelling of the abdomen. M. Pinard made an incision into the posterior vaginal cul-de-sac, where the tumor presented, and extracted the fetus with ease. The pla-

centa was passed spontaneously a few days after. Thanks to daily injections of sublimate ($\frac{1}{2000}$) there was no septicemia. One month after the operation the patient was entirely well. I have deemed it advisable to report this case here on account of the special interest pertaining to it.

III. ITEMS.—In accordance with the prediction in our last report, M. Tarnier has been appointed Professor of Obstetrics at the Faculty of Medicine, of Paris, and it was amidst the congratulations of the entire younger French school that he began his lectures on Wednesday, the twenty-eighth day of March. In his opening lecture, M. Tarnier spoke of the recent progress of obstetrics, particularly in connection with the antiseptic system. He clearly showed the admirable results obtained through it at the *Maternité* of Paris. He then outlined the method in use in his service—the solution of bichloride of mercury $\frac{1}{1000}$ to $\frac{1}{2000}$ is almost exclusively employed, and, in connection with this, he gave, in a few words, the history of this antiseptic, first used by Petit in his laboratory experiments in 1872 and by Davaine in 1874, and first used clinically by M. Tarnier in 1881 (see his communication before the Congress at London), and yet the Germans, in their writings, give Koch the credit of having used it first in his laboratory in 1881, and Schede, clinically, in 1882.

CORRESPONDENCE.

LISTERISM IN ABDOMINAL SURGERY.

TO THE EDITOR.

SIR:—I was under the impression that the discussion of this threadbare subject had ended, but I see that in Dr. Bigelow's paper on gastrotomy for myofibromata of the uterus it is reopened by an extract from a letter written by Mr. Knowsley Thornton. The words of that letter which appertain to my present purpose are: "I attribute my large mortality in hysterectomy . . . as entirely due to the fact that, in the majority of these operations, it is impossible to carry out strict antiseptic or aseptic surgery. . . . I am also prepared to maintain that, while strict Listerism will give us perfect results in the removal of the appendages, it is to the perfecting of our antiseptic precautions that we must look for improved results in hysterectomy."

It is difficult to believe that Mr. Thornton could make such a statement as this, knowing, as he well does, the real facts of the

case as they have occurred at his own hospital. He uses Listerism as strictly as he knows how to do it. His colleague, Dr. Bantock, does not use it at all. In the *British Medical Journal* for October 13th, 1883, and in the last volume of the Transactions of the Obstetrical Society, the figures of the two surgeons of the Samaritan Hospital are given, and they are as follows:

Bantock, 22 cases, with 2 deaths.

Thornton, 12 cases, with 5 deaths.

It must be perfectly clear that there is some other reason than Listerism or the want of it to account for Dr. Bantock's splendid results and Mr. Thornton's great mortality.

I do not think the reason is far to seek, for every one knows that hysterectomy is a very difficult operation, and that the more skilful the surgeon the more likely he is to have good results.

I am, etc., LAWSON TAIT.

BIRMINGHAM, April 26th, 1884.

THE "BALLOON" OR "SQUATTY UTERUS."

TO THE EDITOR.

SIR:—In your last issue of May, in the Transactions of the Obstetrical Society of New York, Dr. Robert Watts, in his remarks on Dr. Perry's report of a case of persistent flaccidity of the uterus after delivery, referring to two cases which he had seen at the Charity Hospital, showing the great length that the sound could be introduced into the uterus without entering the Fallopian tubes or piercing the fundus of the uterus through the peritoneum, states "that Dr. Isaac E. Taylor called those cases 'balloon uteri.'" This is the first time such cases have been accredited to me as "balloon uteri," and I should never have supposed they could be entitled to such a name. The title I gave was the "squatty," as well as "ductile" uterus.

By reference to my monograph on "Amputation of the Cervix Uteri in Certain Forms of Procidencia and Complete Eversion of the Cervix Uteri," published in 1869, as well as in a monograph on the "Physiological Lengthening of the Cervix Uteri Before, During, and After Delivery," in 1874, will be found extensive remarks on the subject from the recognition of the affection, over twenty-five years ago. Your publication of the rectification of the error Dr. Watts has fallen into, and justice to the views I entertain, will much oblige, Mr. Editor, Yours very truly,

I. E. TAYLOR.

NEW YORK, May 12th, 1884.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF NEW YORK.

Meeting, December 4th, 1883.

INSTRUMENT FOR OCCLUDING THE URETER.

DR. J. B. HUNTER presented an instrument which was devised by Dr. Silbermann, of Breslau. He was indebted for it to Dr. W. T. Bull, of this city. Dr. Polk had made a suggestion about a year ago that, in cases of disease of one kidney, by pressure one ureter could be occluded for diagnostic purposes. The instrument presented was intended to accomplish this object, and it could be used in either the male or the female bladder. Dr. Silbermann had been led to make the device by having a case of tumor of the bladder in which the growth so pressed upon one ureter as to produce occlusion. The instrument consisted of a catheter with a fenestra near the end, on the right or left side, according to whether the right or left ureter was to be occluded. At the fenestra was a rubber balloon, covered during the introduction of the instrument by a slide, and when in position distended by quicksilver introduced through the catheter by means of a syringe. Thus a heavy artificial tumor, as large as a small egg, was made to shut off all the urine coming from one kidney by pressure exerted upon the ureter where it entered the bladder. Experiments upon living dogs, which were afterward killed, showed that the ureter was thus completely occluded. In the human subject the quantity of urine secreted for a given time was measured, and was found to be twice as great when the instrument was not used as when it was used.

DR. H. J. GARRIGUES thought it more likely that the urine was shut off by pressure of the artificial tumor upon the ureter along its course in the walls of the bladder than by occluding its vesical orifice.

DR. POLK was much interested in this subject, and had been experimenting for about six months in making an instrument for occlusion of the ureter, either in the male or the female, and he at present had hopes of success. The instrument consisted of a double sigmoid catheter for introduction into the bladder, between which and a rod with a bulb, passed up the vagina or rectum, the ureter could be closed so perfectly that, in experiments on the cadaver, it had been dilated to five times its natural diameter with injections of water without overcoming the occlusion. The distended ureter could be distinctly felt through the rectum above the point of compression. He at present had a case of bloody urine, evidently due to a lesion of one or other of the kidneys, and he expected by means of this instrument to be able to determine from which organ the hemorrhage proceeded. Bloody urine had been passed constantly during six months.

DR. A. JACOBI thought it would be justifiable to introduce the finger into the female bladder and make pressure upon the ureter

sufficiently long to collect urine from the opposite kidney, and thus determine from which organ the blood came.

DR. POLK said that a method which required dilatation of the female urethra was objectionable because of incontinence following, perhaps requiring an operation for its relief. It seemed to him that a vesico-vaginal fistula might be made, and a catheter introduced into the ureter, collecting the urine first from one kidney then from the other, and that this procedure was always justifiable for purposes of positive diagnosis before undertaking so severe an operation as extirpation of a kidney for purely diagnostic purposes. In performing lithotomy, Sir Henry Thompson had made a sufficiently large opening into the male bladder to admit the finger. This had been done, as he understood, in about twelve cases, and without serious results. Such being the fact, surely the same procedure might be resorted to in order to catheterize the male ureters, especially in view of the inefficient methods now at our disposal for making a diagnosis in cases of unilateral renal disease.

A SECOND CASE OF VAGINAL HYSTERECTOMY.

DR. P. F. MUNDÉ said that about six weeks ago he presented to the Society a uterus which he had removed through the vagina for cancer of the cervix. The patient at that time was doing well, and she had continued to do well since, leaving the bed on the fourteenth day and the hospital about the sixth week. She considered herself well three weeks after the operation. Since then he had done a similar operation on another patient, but not with such fortunate results. The case presented some points of difference from the first, to which he would briefly allude. The patient was thirty-one years of age, and had had one child several years before. She was of small stature and had a narrow vagina. The uterus was movable and could be drawn down to the vulva, but it was not so freely movable as in the first case. The disease was a carcinoma of the cervix, which had very slightly encroached upon the wall of the vagina. The physician who had had the kindness to send the patient to him said that he saw her six months before, when there was a laceration of the cervix, with granular degeneration of the anterior lip, which, however, was not believed to be malignant in character. When Dr. Mundé saw the patient, there was no question as to the nature of the disease. The degeneration had extended as high as the internal os, but the body of the organ was not believed to be involved. At first the case seemed to be a simpler one than the former, and it was his intention only to remove the cervix; but, on the strength of the other case, which had given such brilliant results, he put the question to the patient and her husband as to whether they were willing to take the chances of removal of the entire uterus, should it be deemed advisable at the time of the operation. They gave their consent. He was assisted in the operation by Dr. Garrigues, Dr. Chamberlain, Dr. Lee, Dr. Gerster, and the house staff. The operation proved much more difficult than in the first case, and for two reasons, namely, narrowness of the vagina, and less mobility of the uterus, the liga-

ments of which were tense and inelastic. The ligature was first applied to the left broad ligament by Dr. Gerster, and, as was thought, was tied very securely; but, when the ligament was divided, very slight traction upon the ligature caused it to slip. No hemorrhage, however, followed, and he proceeded to apply the ligature to the right broad ligament in sections, after which the left was also secured and ligated in sections. The uterus and ovaries were then removed. Perhaps an hour had elapsed, and it was then noticed that considerable oozing had taken place into the peritoneal cavity, more than had been appreciated, and the question arose as to where it came from. As he had just remarked, hemorrhage had not occurred from the ligaments. The Sims speculum, which had been used, was removed, and immediately there spurted up two streams coming from arteries of the size of a darning needle which lay in the gaping incision in the posterior vaginal wall. The oozing from the raw surface between the vaginal wall and the peritoneum, as much as three inches in diameter, must have been considerable during the operation, but it was impeded by the pressure of the speculum, and was also hidden from view thereby.

The two bleeding points could be secured only with difficulty, and the ligatures included considerable of the tissues surrounding the arteries. He brought the peritoneal and vaginal edges together with sutures, a flanged drainage tube was inserted, and cotton packed lightly into the vagina. The pulse had flagged considerably, but hypodermic injections of whiskey were given, and the patient reacted well, but died at the end of forty hours. The temperature rose just before death to 104° F. It was believed that the patient died from the shock and loss of blood during the operation, the greater portion of the hemorrhage being from the two arteries concealed, and partially compressed by the speculum. There were certain points in the case from which we might derive instruction in vaginal hysterectomy. First, in cases in which the vagina was of small size, the patient not having borne children, or from other cause, it should be dilated gradually by means of the colpeurynter before attempting the operation; and, secondly, the uterus should be rendered freely movable several days or more in advance by gently dragging it down with a vulsella. Heretofore he had employed the sharp needle in passing the ligatures through the broad ligaments, with the result of badly pricking the finger used as a guide. Hereafter he would employ the aneurism needle. It was possible that the dorsal position, with or without the use of perineal and vaginal retractors, would prove the better one for the operation, if the intestines did not fall too much into the posterior cul-de-sac.

DR. GARRIGUES inquired of Dr. Mundé why he regarded the case as one of epithelioma of the cervix.

DR. MUNDÉ admitted that a microscopical examination had not yet been made, but he had seen so many cases of carcinoma of this part, while there had been but few cases of sarcoma of the cervix

recorded, that he had no hesitancy in pronouncing the disease carcinoma in the present instance.

DR. GARRIGUES said that at the last meeting of the Society he referred to the fact of having himself been cognizant of three cases of sarcoma of the uterine cervix occurring within a year, and it was probable that more cases would be found if a microscopic examination were always made in so-called cases of epithelioma.

DR. C. C. LEE desired to know upon what ground Dr. Mundé proposed to pursue hysterectomy in these cases, assuming that the disease was not sarcoma. He had had an opportunity to see the operation done in the present instance. It was done well, and he did not mean to express doubts as to the propriety of it in this particular case, but he wished to be more convinced than he now was of the value of an operation which at best was exceedingly difficult to perform, was extremely dangerous to life, and, if it did not prove immediately fatal, was done for the removal of a disease that was almost certain to return and destroy life in a comparatively short time.

DR. MUNDÉ replied that the question was one which was at present agitating the surgical gynecological world: As to whether we were justified in extirpating the cancerous uterus under any circumstances, even with the prospect of the patient's recovering, for in most of the cases the disease would recur. But, inasmuch as permanent relief was thus sometimes given from cancer, grant that the operation was a justifiable one, in what class of cases should it be done? With regard to the two cases related, in the first the disease was not found to extend above the cervix, and he must admit that excision of the cervix might have been sufficient. In the second case, he had not examined the specimen to know whether the disease had invaded the body of the organ, but this point could not be decided during life, and, in view of the success obtained in the previous case, he had felt justified in giving the patient the benefit of the doubt by removing the entire organ. In future he would limit the operation to cases in which the disease had extended up as high as the internal os, and in such cases he believed that Schroeder's method of wedge-shaped excision of the cervix should not be undertaken, for, as shown by Schroeder's own statistics, the disease almost always returned within two years.

Dr. Mundé said that if the malignant disease did not involve the vaginal walls, and extended as high up as the internal os, he would, as the question at present stood, continue to do vaginal hysterectomy, but he would emphasize the importance of mobility of the uterus and a well-distended vaginal canal.

DR. LEE said that inasmuch as in all probability, in ninety-nine cases in a hundred, the disease would return, and since removal of the cervix alone would often tide on the case for a number of years, he had thought it important to know exactly under what circumstances Dr. Mundé would prefer to do hysterectomy. Several cases had been reported to the Society in which removal of the cervix by means of excision and the actual cautery, and also by other means, had prolonged life several years; one had been reported by Dr. Noeggerath of a patient who was living six or eight years after the operation. The disease has extended up to the internal os.

DR. MUNDÉ remarked that we were all familiar with the operations of Sims, Simon, and others, but the cases in which these operations for partial removal of the cervix were suitable seldom

came under notice early enough for the radical operation, and there would thus still be some cases left for vaginal hysterectomy when the disease was detected in its beginning.

DR. LEE remarked that, if Dr. Mundé could differentiate sarcoma from carcinoma positively, he would admit that he would have an indication for hysterectomy.

DR. POLK asked Dr. Mundé if, supposing he had a case exactly in the condition which he had described, in which all the conditions surrounding the uterus were exactly such as he had pointed out as allowing of total extirpation of the organ, he did not think that just as good results might be obtained by dissecting up the peritoneum anteriorly and posteriorly on the body of the uterus to about midway between the internal os and the Fallopian tubes, and amputating at that point. By this method the whole of the cervix and half the body of the uterus would be removed.

DR. MUNDÉ said that he could not speak of the results of the proposed operation from personal experience. Schroeder latterly had done essentially the operation suggested by Dr. Polk, and the percentage of mortality had been very small, but there had almost invariably been a recurrence.

DR. POLK said that, assuming that when the disease spread from the cervix, following its usual course through the lymphatics in the base of the broad ligaments, to the lateral pelvic glands, it always manifested itself by rigidity of the tissues in this region, the conditions pointed out by Dr. Mundé as justifying extirpation—viz.: a uterus movable, capable of being drawn down to the ostium vaginae, with no infiltration around the cervix or vagina—showed, of course, that the disease in such cases was confined to the cervix. But, granting this, it seemed to him that the operation he had suggested as a substitute would as effectually relieve a patient as removal of the entire organ. He did not mean to say that the disease could not involve the glands in the side of the pelvis without perceptibly thickening them, but he was simply presenting the case in the most favorable light for hysterectomy. If it were not taken for granted that the lateral pelvic glands were free from infiltration, the operation which Dr. Mundé proposed to continue to do would be useless.

DR. MUNDÉ would challenge the statement that carcinomatous infiltration of the lymphatics around the cervix necessarily produced rigidity.

DR. HUNTER believed that any infiltration which fixed the uterus contraindicated its removal. He believed it would be impossible, were that body fixed, to remove it safely. He had records of a number of cases in which removal of the lower half had not yet been followed by a return of the disease. The longest period was eight years, the next six years. Statistics of German operators went to show that out of a large number of cases of hysterectomy recurrence took place about as certainly and as rapidly as after the removal of the cervix only.

INTRAUTERINE FIBROID TUMOR; REMOVAL BY THOMAS' SPOON-SAW.

DR. LEE related a case as follows: The patient was single, aged forty-three years; had been sick fourteen years. Her catamenia appeared at thirteen years of age. She had two normal periods, and afterward was always irregular. Eleven years ago she had hemorrhage almost constantly, which confined her to bed. The

flow was very profuse. Occasional hemorrhages continued until the autumn of 1882, when the bleeding again became constant and profuse. During January and February, 1883, she had no hemorrhage, but in March it recurred with great violence, the patient being confined to her bed until August. During July she had pelvic peritonitis. She was admitted to the hospital September 15th, 1883. When, at this time, she came under Dr. Lee's observation, she was too weak to sit up. Bleeding occurred with every change of position. On careful examination, evident signs were found of an intrauterine tumor in the upper segment of the uterus, and two extrauterine or subperitoneal fibroids, the organ being of about the size of the womb at the sixth month of gestation. Every attempt was made by nutritious food and tonics to improve the general health preparatory to an operation. Iron was not given, through fear of increasing the hemorrhage. Under this treatment the patient improved rapidly, so that a more thorough examination was undertaken in November, when the tumor of the size mentioned was found in the upper segment of the uterus, its attachment being to the posterior wall, and apparently sessile. When recently an operation for its removal was undertaken, an attempt was made to dissect up the attachments by means of the scissors, but the situation of the pedicle was such that it was impossible to do so. Thomas' spoon-saw, with the shaft curved to a suitable form, was then employed, and with it Dr. Lee was able to detach the tumor with the loss of not more than six ounces of blood. The tumor weighed between one and two pounds. A bilateral incision had first been made in the cervix, and the tumor was pulled down with a strong volsella forceps during its removal by the ordinary saw motion with the spoon. Microscopical examination showed the tumor to be an ordinary myo-fibroma. The special interest in the case was the difficulty experienced in removal of the tumor, and the great advantage of Thomas' spoon-saw for this in addition to other purposes. The instrument with a straight shaft did not so well answer the purpose from the difficulty of manipulating it at the fundus. After the operation the uterine cavity was packed with carbolized cotton, and a tampon was maintained in the vagina.

DR. MUNDÉ could testify to the excellence of the Thomas' spoon-saw. Recently he had divided the cervix with scissors and removed an intrauterine fibroid as large as a fist (weighing 8 oz.) by means of the scissors and the spoon-saw, producing very little hemorrhage. The patient made a good recovery. In that case he closed the divided cervix in the usual way, several weeks later, with success.

OVARIAN CYST; METHOD OF TREATING THE PERITONEAL WOUND.

DR. HUNTER presented an ovarian tumor removed the week before at the Woman's Hospital from a patient sixty-four years of age, who, four years ago, first noticed slight abdominal enlarge-

ment, which had continued to increase since. The abdomen had become greatly distended and breathing was difficult. It was a question as to whether there existed simply an abdominal tumor, or whether there was in addition some ascitic fluid. The incision made was less than four inches in length. A thick sac, containing coffee-colored fluid, was first reached, and, when he was about to tie its pedicle, another cyst was seen containing perfectly clear fluid, and having a very thin wall. Between the two was the pedicle. The point to which Dr. Hunter wished to call attention was his method of uniting the peritoneal surfaces of the abdominal wall. He had noticed that a good many cases of mural abscess occurred from imperfectly united abdominal walls. In the present case he passed two silver sutures entirely through the abdominal wall, including the peritoneum, closed the peritoneal opening with catgut suture, and then closed the abdominal wound by superficial sutures of silk. The weight of the sacs and the fluid in this case was twenty-one pounds.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, April 3d, 1884.

The President, R. A. CLEEMANN, M.D., in the Chair.

DR. CLEEMANN made some remarks on the subject of

DIGITAL DILATATION OF THE OS UTERI DURING LABOR.

He had been taught not to dilate or stretch the os uteri with the finger, and for years this early teaching deterred him from making any attempt to supplement the contractile powers of the uterus by assisting in the process of dilating the os. Some time since he was called to attend a primipara, the waters had been discharged the previous day, the pains had continued, but the os uteri was very small and the cervical rim hard and unyielding. He felt called upon to interfere actively and tried to dilate the os with his finger; it softened rapidly and in half an hour was sufficiently dilated to allow the head to pass, and delivery rapidly followed. Since that occasion he has tried the same procedure on several cases, and always with gratifying results, the labors being brought to rapid terminations, where previously hours had been wasted in weary and painful waiting. The irritable condition of the os which had been lectured upon as the consequence of such interference has not been experienced; no injury has resulted in any case. The soft finger can do no more harm, if clean, than a Barnes' or other form of dilator, and there is no danger, as in the case of the latter,

of pushing the head aside, and converting a vertex into a shoulder or other faulty presentation.

DR. W. T. TAYLOR, since he had dared to deviate from the teaching of Professor Hodge, had used his finger to assist the dilatation of the os; he did not do so if the cervix was irritable or its edge wiry.

DR. GITHENS had practised digital dilatation of the os uteri throughout his obstetrical practice, a period of eighteen years. He does not confine it to any class of cases, nor does he wait until after the membranes are ruptured. In any or all cases he finds that a "pain" is accompanied by a contraction of the circular muscular fibres of the cervix, as well as by a contraction of the longitudinal fibres of the body of the uterus. The contraction of the circular fibres retards the progress of the labor. The intention of the digital distention is to paralyze these circular fibres, and thus favor the dilatation of the os. In practice this effect is rapidly produced. One or two fingers are swept around the inside of the cervix, the pulp of the finger being next the cervix, and the latter is pulled away from the head. This operation is kept up during the interval between pains; when the pain occurs the finger is withdrawn; the operation is repeated in the next interval. The membranes are not ruptured by this process. The irritable condition of the os, if such exists, is subdued. If the rim of the cervix is wiry and thin or hard and thick, it softens and yields; the cervix and vagina, if hot and unyielding at first, become cool and pliant; cervical tears are almost entirely avoided, and the time, pain, and exhaustion of the labor are reduced to a minimum. The process is useful in every case of labor throughout the first stage.

DR. PHILIP M. SCHIEDT practises digital dilatation largely, his patients say they recognize the assistance it gives them, and in subsequent labors ask the doctor to help them. By the great shortening of the first stage of labor resulting from this method the use of the forceps is frequently avoided.

DR. PARVIN would be sorry to see digital dilatation adopted as a rule for all cases. The fingers used as recommended do not act only, possibly not chiefly, as dilators, but by evoking uterine contractions. Voluntary efforts at bearing down are not needed during the first stage, they are dangerous rather than helpful. The method may be useful in some cases after the rupture of the membranes which is the natural dilating agent. There is also danger of septicemia from germs on the fingers. He does not think the finger so good a dilator as Barnes' dilator, because unequal partial pressure upon the os does not evoke the decided uterine contractions that general uniform pressure does. He thought the danger of a change of presentation by the use of Barnes' dilator very slight. He would prefer a mechanical dilator to the finger, whenever dilatation was necessary, but thought something ought to be left to nature. Any sort of interference carries a possibility of danger.

DR. ELLIOTT RICHARDSON thought there was a possible danger of rupturing the membranes. Our authorities caution us about the introduction of the finger into the uterus, and the too early rupture of the membranes.

DR. HARRIS remarked that one point had been overlooked. Why does the os not dilate easily when the head is the dilating agent? It is because it is a round surface over which the cervix does not slide easily. On the contrary the finger is applied at successive

points. One benefit of the method is that any change or danger is at once detected. The method should not be used indiscriminately, and we should not interfere unnecessarily.

DR. CLEEMANN would not recommend the method in every case of labor. He has resorted to it in cases where there has been early rupture of the membranes, and the assistance of the bag of waters has been lost. In a recent case he saved a patient hours of suffering, and the os was not bruised or injured in any way. The sight of any instrument causes the patient much anxiety, and the exhibition of the Barnes dilator and the water, syringe, etc., cause nervous excitement. The bags sometimes burst, and thus give the patient a terrible shock, with the added discomfort of the escaping water or air. He has always carried them, but does not like to use them.

EXANTHEMATOUS DISORDERS IN THE PUERPERAL STATE.

DR. PARVIN had recently an experience of the invasion of measles and scarlet fever in the obstetric wards of the Philadelphia Hospital. In the first case in which measles occurred, he did not think that the full term of pregnancy had been reached. Authorities state that premature labor is usually caused, a result of the high temperature of the exanthematous fever. One patient had septiceimia in addition, but recovered. The infants were not affected. In one case, soon after labor the temperature rose to 103°, and the patient was sent to the fever ward; at the second visit, a slight rash was observed which subsequently proved to be scarlatina. Desquamation was very abundant. Albuminuria was very marked on the tenth day, rheumatic pains were also felt. The patient recovered. The child remained well. Children have been born with measles, but he did not know of such an experience with scarlet fever.

DR. W. T. TAYLOR, some fifteen or twenty years ago, had reported in *Amer. Jour. Med. Sci.*, 1853, a case of congenital small-pox. The mother had some febrile symptoms and pain in the back, and the child had the disease, being marked with pits. He had a case of scarlet fever in a mother two days after labor; it proved fatal in two days; the child lived.

DR. HARRIS remarked that in the reports of the Rotunda Maternity Hospital in Dublin are a number of interesting reports on complications of the puerperal period by exanthematous fevers. Their morality has been very materially influenced by these epidemics. Erysipelas is the most interesting and most fatal of these complications; in some cases it resembles septic poisoning.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF LONDON.

Meeting, February 6th, 1884.

DR. GERVIS, *President, in the Chair.*

AUVARD'S "NEST."

DR. MATTHEWS DUNCAN exhibited the "nest" or "incubator" of Dr. Auvard. A similar machine had been used for some years in the Paris Maternité by Dr. Tarnier, but upon this Dr. Auvard's was an improvement. It was planned to maintain a uniform temperature. It was simple, cheap, and useful. Dr. Auvard stated that by its use the mortality of children under 2,000 grams weight at birth had been reduced from 65 per cent to 38 per cent. The child was placed in it clothed. The machine was kept warm by hot-water bottles, the air moistened by passing over damp sponges, and ventilation kept up and watched by a little anemometer. Any ordinary nurse could manage easily the whole matter.

DR. FANCOURT BARNES had seen a similar machine in use at the Hôpital Tenon. He believed that Dr. Tarnier, not Dr. Auvard, was its originator.

HEARSON'S INCUBATOR.

DR. GODSON exhibited Hearson's incubator, and introduced its inventor, who explained it.

UTERUS REMOVED PER VAGINAM.

DR. W. A. DUNCAN exhibited an entire uterus with epithelioma of the cervix, successfully removed per vaginam, by Schroeder's operation.

NEW CEPHALOTRIBE.

DR. FANCOURT BARNES exhibited a new cephalotribe he had had made. It was long, with a French lock, the perineal curve, and Aveling's handle-curve. It allowed traction to be made in the brim of the pelvis.

The PRESIDENT was accustomed to use Hicks' cephalotribe.

DR. ROBERT BARNES had put this instrument to a practical test, having used it in a case of contracted pelvis in which he had failed to deliver both with his own forceps and with Tarnier's. It worked admirably, and he thought it might be stamped "*probatum est.*"

A LOST MEDICAL WORK.

At the annual meeting of the Obstetrical Society of London, Dr. Aveling mentioned that the Society had some time since asked Dr. Fordyce Barker, of New York, to have made for them a transcript of Wolveridge's "*Speculum Matricis.*" This book, published in

Dublin, 1670, is the earliest original work on midwifery in the English language, and the only copy known to exist was in the possession of Dr. Fordyce Barker, who, at the request of the Society, employed a man to copy it. This person absconded with the volume, and died in Europe. Thus, it is feared, the only copy of this work has been irretrievably lost. It is asked that if any of our readers should meet with or hear of a copy of this rare book, they will inform the Honorary Librarian of the Obstetrical Society of London.

Meeting, March 5th, 1884.

H. GERVIS, M.D., F.R.C.P., *President, in the Chair.*

DR. HENOCKS showed a wooden ring pessary coated with a deposit of lead from the use of lead lotions. It had been worn for five years.

MR. THORNTON showed a fibro-cystic myoma of the uterus, with both ovaries attached, removed from a single woman aged forty. The patient recovered.

Also a large fibroid uterus with separated subperitoneal fibroids, small multiple cysts of the left ovary, and a large multilocular tumor of the right ovary with broad ligament cyst attached. The patient, aged forty, single, died of peritonitis on the fourth day.

DR. GALABIN showed (for Mr. Fielden, of the Chichester Infirmary) a cyst removed from the vulva, believed to be a hydrocele of a peritoneal pouch.

DR. HEYWOOD SMITH showed a dermoid cyst of the right ovary, containing skin, teeth, bone, and hair.

Also a large fibroid removed, together with the upper third of the uterus, from a woman aged thirty-nine.

DR. BOXALL showed a morbidly adherent placenta from a syphilitic patient, detached with great difficulty by the fingers. There were several accessory cotyledons joined to the main placenta by a membranous or velamentous interspace of chorion.

Also a placenta containing a large blood-cyst nearly as large as an orange, the greater part of the fetal surface being also occupied by old blood clots.

DR. BARNES showed an ovarian tumor, removed at short notice on account of symptoms of twisted pedicle, viz., sudden access of severe pain, vomiting, and depression of pulse, together with rapid increase in the size of the tumor. The case showed that ovarian tumors should be removed as soon as diagnosed, in order to avoid the numerous accidents to which their owners were liable.

DR. JOHN WILLIAMS read a paper on

CORRODING ULCER OF THE OS UTERI.

He referred to the rarity of the disease, to the almost entire absence of descriptions of it in systematic treatises, and to the fact that its existence had been denied by some observers. It had been mistaken for cancer, and vice versa. The histories of these

cases, which had been under the author's observation for varying periods of time—one for two years and two for about ten years—were narrated, and the post-mortem appearances, together with the microscopical characters of the ulcer in one instance, were described. The differences between corroding ulcer, both clinical and anatomical, were pointed out, and the views of Sir Charles Clarke, that the disease was distinct from cancer, upheld. The disease begins at the os uteri, and extends along the vagina, involving its walls in a symmetrical manner. Its progress is slow, and two of the author's patients died of other diseases, while the third, which had been under observation for ten years, was still living. It was not necessarily associated with loss of flesh, and the pain and discharges were different from those usually met with in cancer of the uterus, and the edges of the ulcer were not hard and thickened. The mode of progress of the ulcer was in one case by means of reddish, raised tubercles, which became ulcerated in another by slow ulceration without any preceding changes in the tissue about to be invaded, except some redness. The mucous surface, to the edge of the ulcer, appeared perfectly healthy. Microscopic examination shows this last observation to have been accurate, for the ulcer, its base and borders, presented no appearance except that of granulation tissue. The case in which the disease presented the characters of lupus is still living. In the second case, which died, and which presented the microscopic appearances mentioned, the ulcer appeared to be the result of a slow gangrene arising from calcification of the internal iliac arteries and their branches.

THE PRESIDENT expressed his admiration of Dr. Williams' paper, dealing so ably with a rare and interesting disease. He regretted that no autopsy had been allowed in the second case; but from the results of the autopsy in the third case, the symptoms observed by the writer through a series of years in each case, and the observations of other authors, referred to by Dr. Williams, he thought that the disease must now be definitely withdrawn from the list of cancerous or cancrroid affections. Dr. Williams' graphic description might have the effect of bringing cases, hitherto classed under the head of ulcerating epithelioma, under that of corroding ulcer.

DR. PRIESTLEY said that the rarity of the affection was proved by the fact that some gynecologists in large practice were doubtful if they had ever seen a case, and that its pathology was much disputed. Thus Kiwisch and Scanzoni regarded it as a variety of encephaloid cancer. Courty and West regarded it as a form of epithelioma. He thought he had seen the characteristic form of the disease more than once. Sir Charles Clarke described the pain as burning, and not gnawing, like that in cancer. He reminded Dr. Matthews Duncan of two cases which he had seen with Dr. Priestley in which a similar affection of the uterine body was not improbable without affection of the cervix. The symptoms were profuse serous or muco-purulent discharge, with occasional hemorrhage; the cavity of the womb was dilated, but there was no great enlargement of the organ. Both patients were

advanced in age; the disease lasted for several years; death occurred from exhaustion; treatment had little effect. If he saw the disease on the cervix in an early stage, he should try the effect of various caustics. But treatment was not very promising.

DR. MATTHEWS DUNCAN was deeply interested, for the disease had been the object of special study for many years to him, and he believed it to be of the nature of lupus. He was not sure that he had ever seen a case identical with those of Dr. Williams' as to situation, but he had seen many in the lower vagina and external organs. Lupus of the external organs seemed to produce hypertrophies greater than any produced higher up in the passages. He had also seen the disease in the uterus, and one of the cases referred to by Dr. Priestley had been fully described by Dr. Duncan in Vol. XXI. of the Society's Transactions.

DR. WILLIAM DUNCAN thought Dr. Williams' cases were lupus which had taken on an epitheliomatous nature.

DR. CHALMERS thought that the disease was not so rare as had been thought. He had seen two such cases in the last two years; in one case the patient died within a period specified in the prognosis given.

DR. GALABIN asked Dr. Williams whether he regarded the disease as having any analogy with rodent ulcer of the skin, which was now regarded as a form of rodent cancer. It must either be lupus or a form of cancer in a wide sense. He had examined the uterus in Dr. Duncan's case, and found the deposit thickening the walls to consist of leucocytes, some of them having the suspicious character of being united by tailed processes.

DR. GRIFFITH had seen a case which he thought similar. There was little pain, no glandular enlargement; it proved fatal in two or three years by repeated and uncontrollable hemorrhage. No autopsy was allowed, but the left side of the uterus was felt much thickened.

DR. ROUTH had seen two cases of the disease. There was no induration of surrounding parts nor fixation of the cervix. The absence of induration made him dissent from the view that it was lupus. Moreover, the ulcer never healed, as it does sometimes for a time in lupus. He suspected a scrofulous or syphilitic taint acting on ill-fed and weakly persons. Similar ulcers had been described by Samuel Cooper. He thought there was some inconsistency in the author's statement that one case was analogous to senile gangrene and another to lupus.

DR. AVELING said that Dr. Rigby believed that burning pain accompanying this form of ulceration was different from the darting pain of cancer. He asked Dr. Williams how far his observations agreed with this.

DR. WILLIAMS replied that rodent ulcer was epithelioma starting from hair follicles and sweat glands, and corroding ulcer of the cervix could not be such. In one of his cases, it appeared to be lupus, for the ring of tubercles with the cicatrix could be nothing else. This patient was still living, therefore no microscopical examination had been made. The third case was, in his opinion, senile gangrene, depending on calcification of the internal iliac arteries and their branches on both sides. He had used the word "malignant" loosely, as equivalent to incurable. He had seen one case of ulceration of the inner surface of the body of the uterus. The patient was over sixty years of age, had suffered long with offensive discharge, and was very fat. She died from

other causes. At the autopsy, the uterus was large, the walls thickened, the cavity large, secreting a quantity of broken-down dark material. Section of the walls showed nothing except the muscular fibres of the uterus running to the surface, and in them many round cells, but nothing to indicate malignancy. No local treatment had been pursued.

Meeting, April 21st, 1884.

H. GERVIS, M.D., F.R.C.P., *President, in the Chair.*

MR. DORAN read a paper on

THE RELATIONS OF PROLAPSE OF THE VAGINA TO HERNIA,

Illustrated by two pedigrees. The paper described cases of women affected with prolapse of the anterior wall of the vagina, where a very marked family history of hernia existed. The genealogies of the patients were given in a tabular form. The cases all tend to prove the close relation between hernia of the intestine and displacements of the female pelvic viscera.

DR. MATTHEWS DUNCAN had long taught that intestinal hernia and procidentia uteri were alternative conditions. The whole of Mr. Doran's excellent paper was a part of the great subject of the retentive power of the abdomen or its pressure relations.

DR. CHAMPNEYS said that in the work of Landau on movable kidney, various associated conditions were mentioned; hernia was found in seven per cent, descent of the uterus or vagina in thirteen per cent, "retroflexion" in fifteen per cent, and pendulous belly in twenty-five per cent. No explanation is given of the term "retroflexion," but it probably means the condition known as retroflexion with descent (or rather descent with retroflexion).

It was his habit to inquire of patients suffering from descent of the pelvic organs, as to family histories of hernia, prolapsus uteri, and prolapsus ani. One such history was the following:

A woman, aged fifty-two, had had procidentia since the age of twenty-seven, her eldest daughter (aged thirty) has had procidentia since the age of twenty-one; all her three sons had prolapsus ani as children.

DR. HORROCKS said allowance must be made for the fact that in the same family it often happened that circumstances obliged all the members to do hard work.

MR. DORAN, in reply, said that the history of rupture was often difficult to obtain, as the condition was naturally concealed, especially between the different sexes, and also between those of very different ages. In some cases the bladder was not displaced in descent of the vagina and uterus. He had recently seen a case of apparent cystocele in which no part of the bladder was prolapsed. The distinction between prolapse of the generative organs with and without cystocele is an interesting question to solve in relation to hernia.

DR. KILNER read a paper on

THE USE OF THE INDUCED CURRENT DURING PARTURITION.

The effects of the current are the relief of pain, prevention of

fatigue and post-partum hemorrhage, equalization of the pulse, increase in frequency and strength of the uterine contractions, and the prevention of vomiting.

The pain from stretching of the vulva is, however, not alleviated by it, nor does it to any extent relieve pain in cases of instrumental labor. The author now employs the coil (shown) in nearly all cases. In about three hundred cases post-partum hemorrhage only occurred twice. The coil sometimes fails to increase uterine contractions when most needed. After its use for an hour or one and a half hours, it exercises its sedative action, and no longer increases the contractions. In some cases it produces violent and almost continuous contractions. In one case a contraction lasted fifteen minutes. No prognosis with regard to these points can be made. It has no effect in diminishing after-pains. It generally prevents or stops vomiting. Statistics were given and directions for use.

THE PRESIDENT said that an agent which increased uterine action while lessening pain was almost too good to be true, but laboring women would be grateful for such a method.

DR. PLAYFAIR had tried the current and it had proved a failure, perhaps from want of special skill; but if special skill were needed, it could not be generally used. Its effects in diminishing pain were slight, and not to be compared with other means at our disposal. A battery was a cumbrous thing to carry about, and was likely to alarm patients. As an oxytocic he had found it useless. Dr. Kilner compared its action with that of ergot, which should never be used to accelerate delivery. Uterine manual pressure was the best and safest oxytocic.

DR. WALTER had used the current for more than seven years for inertia of the uterus after the expulsion of the placenta, and found it in some cases superior to ordinary means. He had found it useful in post-partum hemorrhage.

DR. HORROCKS asked for details as to live birth, suspended animation, hemorrhage, and whether premature labor had been induced. In this respect electricity had always hitherto failed.

DR. ROUTH had found the electrical current useful in migraine; in lumbago a man would come into his study walking like an old person, and would leave it as erect as a soldier. He had also dispersed uterine fibroids by its frequent use, and tumors of the breast.

DR. MURRAY thought that if the electric current had such power as had been described, the possibility of rupture of the uterus would have to be remembered. He had used it for hemorrhage, but considered its value very doubtful.

DR. KILNER replied that, as regards the lessening of pain, the current is more under control than an anesthetic, and never caused sickness. He had never seen a case of rupture of the uterus.

REVIEWS.

DES MÉTHODES ANTISEPTIQUES EN OBSTÉTRIQUE.—THE APPLICATION OF ANTISEPTIC METHODS TO OBSTETRICS. By DR. PAUL BAR, Obstetrician to the Hospitals, etc., etc. Paris: Alexandre Coccoz. 1883.

However much opinion may vary in regard to the necessity of the application of the minutiae of the antiseptic system in private obstetrical practice, a perusal of this monograph must set at rest forever the question of the utility of stringent detail in every maternity hospital. The author, after an impartial study of the history of puerperal epidemics, has reached the conclusion that they are dependent for their inception and propagation on the presence of germs, and thence is naturally led to the conclusion that the best treatment of the disease lies in every means by which the growth and propagation of the prime causal factors of the disease may be prevented. In other words, it is Dr. Bar's belief that puerperal fever is essentially the same as septicemia, and that there can be no septicemia if the septic agent which produces it is kept at a distance, or destroyed by the use of agents which the physiological chemist has taught us are inimical to it. To prove this point, he first sketches rapidly the history of the germ-theory, deducing thence his reason for the belief that the microbe, "whether multiple or single in essence, although variable in its morphology," must be present in order that the lying-in woman become affected; then considers briefly the many antiseptic remedies which have been from time to time proposed, giving the preference himself to the bichloride of mercury, and this done, proceeds to prove the worth of antiseptics, as judged by its results. A mere statement of these results is possible here, but fortunately the figures will speak for themselves. To take, for instance, the *Maternité* of Paris. A glance at the statistics presented by Dr. Bar proves that, with each additional advance in the application of antiseptics, the mortality percentage has constantly decreased. It was in 1870 that, through the efforts of Tarnier, the antiseptic system began to be applied to this hospital, though it is only within the last few years that all its minutiae have been introduced. Prior to this year the percentage of deaths varied between 3.5 per cent (1858) and 20.3 per cent (1864), the rates being nearest the latter figure in 1869 (11.6 per cent), 1861 (11.7 per cent), 1863 (13.7 per cent). After 1870 the mortality decreased constantly, with slight occasional elevations from 2.8 per cent in 1871 to 1.1 per cent in 1883, and these are the figures for the general hospital. In Tarnier's pavilion, whose construction is to-day a familiar matter to most obstetricians, and where antiseptics as complete as possible obtains, the figures are still better. In 1876, the year of its establishment, there was but 1 death out of 88 deliveries; in 1877 and 1878, out of 204 and 234 deliveries respectively, only 2 deaths; in 1879, out of 182 deliveries, again 1 death, and since then, up to the middle of June, 1883, out of a total number of 785 deliveries, not a single death. Better results are, of course, not to be expected, and these figures

alone would speak eloquently in favor of the use of antiseptics. But statistics from other sources are available. To take the statistics of the Prague Maternity. Before the year 1875, the mortality varied between 9.28 per cent (1865), 11.62 per cent (1869), and 3.08 per cent (1871). In the year 1875, a new building was occupied, and antiseptic methods were introduced. The percentage of deaths was immediately lowered to 2.75 per cent, and has since decreased to 0.56 per cent (1882). Further statistics, did space allow, might be taken from this monograph, but surely enough has been quoted to convince the most sceptical of the worth of antiseptics in maternity hospitals.

Dr. Bar then proceeds to note some statistics derived from a source outside of a maternity proper, namely, from the practice of those Parisian midwives licensed to receive lying-in women at their houses. These houses are under the supervision of practising obstetricians, and of these, Pinard has noted three deaths out of 1,001 confinements in nurses under his care, and Budin the same number out of 1,021. These midwives, it should be noted, are obliged to resort to practically the same antiseptic precautions as are taken at the Maternité proper. As to what these precautions are, it would be a thankless task to recapitulate them here, were it the place, for they are essentially the same as those which so recently emanated from the majority of the gentlemen taking part in the discussion on puerperal septicemia before the Academy of Medicine. As here, so in Europe, there is difference in detail. Some gentlemen use vaginal injections during and after labor, some do not except when specially indicated: the spray during labor is in favor with some, the application of a Lister dressing to the vulva after labor with others. The main reliance is on placing the patient in such an atmosphere and amidst such surrounding as forbid the development of septic germs. This accomplished, the less, according to our author, the woman is disturbed the better. If the germs do gain entrance, it is from some lack of detail, and the treatment then applicable varies in no respect from that in use on this side of the Atlantic. As a general rule for guidance, one worthy of emphasis and of approval, since its application, whilst in no wise interfering with physiological processes, takes just account of the pathological, we will content ourselves with stating here Dr. Bar's conclusion that "so long as no unfavorable symptoms develop, it is better to limit ourselves to careful washing of the vulva; if the lochia become fetid, give vaginal injections. At the first sign of infection, chill and fever, resort to intrauterine injections. The antiseptic system for us does not mean interference in every case. In obstetrics, as in surgery, precise indications are alone to be obeyed." In other words, render the surroundings and atmosphere clean, and then do not interfere with a normal and necessary function till the sequence of events in its fulfilment becomes abnormal.

The remainder of the monograph deals with subjects not strictly connected with the puerperal condition, such as the antiseptic treatment of cystitis, occurring during the puerperium, the treatment of ophthalmia neonatorum by Crede's method, etc., etc. That the monograph is valuable goes without saying, and that it is interesting all who are tempted to read it will agree.

EGBERT H. GRANDIN.

THE TRANSACTIONS OF THE EDINBURGH OBSTETRICAL SOCIETY.
Vol. VIII. Session 1882-3. Edinburgh: Oliver and Boyd, 1883.

This volume will be found full of papers of an interesting and practical nature. Most of the contributions are distinctively clinical. Whilst, generally, it is invidious to single out one in particular as being better than the others, in this instance no one will refuse the palm to PROFESSOR W. A. FREUND's contribution on Extrauterine Pregnancy. Opinion is not entirely settled as to the best course to pursue in certain varieties of ectopic gestation. This distinguished gentleman having had the fortune to witness many cases of the kind, whatever deductions he has been able to draw from them are certainly worthy of record: and, from the side of treatment, they may be recapitulated here briefly as follows: Where gestation has not advanced beyond three months, the fetus should be destroyed by puncture of the sac and the injection of morphia. In case of rupture, laparotomy should not be performed, for the reason that "death in this well-known catastrophe only rarely happens;" but the case be treated simply symptomatically. After the third month, the rule should be to wait, paying strict attention to the diet and bowels. Where the fetus has died spontaneously, or where its death has been effected artificially, as long as there is no reaction, the rule, again, should be to wait. If there result peritonitis, the abdomen is to be opened, evacuated, and drained. If the child be viable, laparotomy is indicated. It will thus be seen that in some point Professor Freund is not in accord with advanced views current in this country, where galvanization or electrolysis have been proved safer and more effective than puncture and injection of the sac, and where a few distinguished gentlemen, at any rate, would sanction immediate recourse to laparotomy after the rupture of the cyst. The discussion of Professor Freund's paper was adjourned to a future meeting, and therefore it is not as yet possible to state in how far the members of this Society agree or disagree with him.

PROF. ALEX. RUSSELL SIMPSON contributes four papers to this volume: the first, on a case of basilysis for dystocia from hypertrophic elongation of the cervix uteri, in which the instrument he has devised and perfected for comminuting the base of the skull worked admirably and to great advantage; the second, on the prophylaxis of ophthalmia neonatorum, a plea in favor of the method proposed by Cr  d  —the instilling of a drop of a two-per-cent solution of nitrate of silver into the eyes of the child, as soon as possible after birth—his own results according with Cr  d  s, though a weaker solution was used; the third, an essay on superinvolution of the uterus, his opinions and treatment being essentially such as have recently emanated from various gentlemen in this country; the fourth, a paper on the axis-traction forceps, of which he is a strong adherent, and the original of which he has somewhat modified.

DR. D. BERRY HART's contribution to the anatomy and etiology of rupture of the peritoneal portion of the vagina during labor constitutes another link in the series of valuable etiological and practical papers on the anatomy of the pelvic floor which he has presented to the profession. The described lesion is a rare one and occurs, by preference, on the posterior vaginal wall, because it is weaker than the anterior, the tear most commonly occurring at the point where the wall is covered by peritoneum, and resulting, like cervical rupture, from excessive tension at that point.

To mention the other papers by name only, though each has its value: Observations on the Bladder During the Early Puerperium,

by DR. J. HALLIDAY CROOM; Umbilical Cord Around the Child's Neck as a Cause of Delayed Labor, and Sometimes of Infantile Death, by DR. A. D. LEITH NAPIER; On Dangerous Hemorrhage from the External Genital Organs During and After Labor, by DR. PETER YOUNG; Obstinate Vomiting in Pregnancy, by Mr. W. J. BROCK; and, finally, Statistics of Two Hundred and Fifty Consecutive Midwifery Cases in Private Practice, by DR. A. S. CURRIE.

A valuable feature of these transactions are the quarterly reports of the Royal Maternity and Simpson Memorial Hospital. The volume altogether is a readable one, and reflects credit on the members of the Society whose proceedings it records. E. H. G.

DIE GEBURT BEI DEN URVÖLKERN.—LABOR AMONGST PRIMITIVE PEOPLES. By DR. G. J. ENGELMANN. Translated from the English, with certain additions, by DR. C. HENNIG, University Professor and Director of the Children's Institute at Leipzig. Wilhelm Braumüller, Vienna.

This work, as the title sets forth, is a translation into the German of Dr. Engelmann's well-known papers on Parturition, Pregnancy, and Child-bed amongst Primitive Peoples, a part of which originally appeared in this JOURNAL. The author is to be congratulated on this well-deserved compliment, and German medical readers are indebted to Professor Hennig for placing within their reach this unique, instructive, and interesting work. The translator's additions consist in occasional critical or explanatory foot notes, and in a few interesting pages concerning labor amongst the Japanese, with a description of the peculiarities in formation of their pelvis. Much of Prof. Hennig's time during the past ten years has been devoted to the study of the pelvis as typified amongst various races, and his views, therefore, carry with them the weight of authority.

Further comment on this work is unnecessary in this country, where Engelmann's papers have been generally read, and have recently reappeared in book form. E. H. G.

ITEMS.

1. DR. FORDYCE BARKER has safely returned from his hurried visit to Edinburgh, where he received the well-merited distinction of Honorary LL.D. His numerous friends and admirers here and abroad will agree that this great honor could not have been more worthily bestowed.

2. DR. PAUL F. MUNDÉ has been appointed Examiner in Obstetrics in the University of the State of New York.

3. The work of fitting up the building recently bought by the NEW YORK POLYCLINIC (a portion of which the School has hitherto occupied), has begun and will be completed in time for the opening of the Fall term on Oct. 1st. Ample facilities will be afforded for clinical instruction in Gynecology.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY

GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

INCONTINENCE OF URINE IN CHILDREN.

BY

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How a subject of as much importance as this should have so long escaped presentation and discussion before the American Medical Association is surprising; but the fact exists that it never has been presented to this body and for that reason I have chosen it.

There is not much importance attached to the history of the disease. With the assistance of Drs. Kolipinski and McArdle, of this city, I have examined all the literature on the subject from 1784 to the present, including articles in German, French, Italian, and Spanish. In 1784 Mitchell wrote on the disease as clearly as any subsequent author, and its pathology was as well understood then as now. The tendency at that time was to let it alone with a hope that puberty would restore the function of the bladder.

From birth the child instinctively voids its urine and we take it for granted that the act is reflex. But with the evolution of the teeth, speech begins, intelligence is developing, and we expect the will to control the sphincter vesicæ. The rule is that about the eighteenth month the child is taught to exercise complete control over the sphincter. If after this age the urine is passed involuntarily the tendency is to attribute the disgusting act rather to carelessness than to a pathological state, which to my mind is an injustice to the child. From observa-

tion and the supervision over children of every condition of life I am loath to accept this conclusion, and am proud to state that all the cases which have come under my observation have had specific causes, and were not the effects of laziness. So believing, as I do, in a pathological state, it delights me to attempt this defence of the long-abused unfortunate.

Many a child has been repeatedly and unmercifully punished for wetting his clothes or bed in the face of repeated protestations that he could not help it. The disease and punishment go on together until the patient becomes such an object of disgust to himself and his family that they are impelled to seek professional advice. Then the parents learn that they have been chastising their child, perhaps for years, for a fault which was the result of disease, and therefore uncontrollable, when they would have quickly resented a just punishment, if administered by some one else, for a real fault less offensive in its character.

Again there are others who, while they believe the act involuntary, will let it run on for years with the hope that education and the inculcation of habits of cleanliness will effect a cure, or that their only hope for relief is in the establishment of puberty.

The child instinctively becomes neat and seeks the proper time and place to empty its bladder, and I am unwilling to admit that it ever deliberately soils its clothes after it has once been taught to use the vessel. None of the brute creation will lie in their urine if they are not tied or penned; then why do we attribute this practice in the rational being to laziness? Simply because some are not able, by a careless and superficial examination, to find the cause, and well knowing that their reputations will be at stake if they do not account for the act, they too often condemn the helpless child to daily floggings. There must be a pathological condition to account for an act that makes nature an abhorrence to herself, and it is our duty to seek diligently for it, remove it, and thus transmute the filthy child into the cleanly.

But in too many cases the act continues untreated, in spite of punishments and the jeers of companions, until well-marked psychical changes take place. The child, bright and cheerful by nature, soon loses his vivacity; shrinks from the presence

of his companions, becomes morose and spiteful, pale and haggard, restless and nervous; will not look you in the face; and with chin depressed and upper lids drooping presents, indeed, a striking likeness to the onanist.

We generally find the disease divided into three varieties. In the first class the subjects suffer from a constant dribbling of urine day and night. This variety is infrequent, and when found it is usually associated with some serious pathological state. I have met with but two cases of this kind, in boys about eight years of age, who for several years had been unable to retain their water; examination revealed a vesical calculus in each case which, being removed, the function of the bladder became normal.

A second class comprises those whose incontinence is intermittent in character, and occurs in the day as well as at night. We find that in this class the urine is retained for a short time during the day, when the desire to void comes, but before the child reaches a convenient place the sphincter is overcome and the poor child is powerless to stop the flow. This is the form usually met with in the girl. In fact the histories of those I have seen have been that the girl would suddenly be taken with a desire to urinate, while in school or on the street, but before they could reach a closet the power of control gave way. The cause in these cases was found to be vulvitis or urethritis as a result of the irritation from ascarides in the vagina.

But the third class is the one that interests us most because of its frequency in both sexes, its nocturnal character, its possible concealment for years, and the promptness with which it yields to treatment. It is the children of this class who are so frequently punished for bed-wetting when they are as powerless to control the sphincter during the night as are those of the two preceding classes during the day. They may, and usually do, urinate before retiring, and yet about midnight, during a profound sleep, the urine is passed again; or it may be that the night is passed without the accident, but just before rising in the morning the contents of a full bladder are involuntarily set free. Patients of this class generally dream of urinating. In the boys of this class, urination takes place during erection of the penis. Again, we meet with cases where the cause is obscure, but, nevertheless, the nocturnal in-

continence occasionally takes place. In these patients I attribute the accident to causes that favor a perfectly physiological process in the adult. We well know that late suppers, rich food, wines, certain positions during repose, profound sleep, amorous or lascivious dreams, and many such causes produce a nocturnal pollution in the adult, and I am convinced that the same causes excite a similar irritation in the child; but, instead of the seminal discharge, the physiological process of which is not yet established, the bladder is emptied. In each instance the discharge is the result of a conservative process of nature to relieve the irritation. Indeed, this theory seems the more plausible because in many instances the nocturnal bed-wetting goes on undisturbed until the full establishment of the sexual functions, when the enuresis is superseded by nocturnal pollutions. This theory is also tenable because most of the remedies which cure the adult of his complaint very quickly relieve the child of its.

The disease is more frequent than the statistics of hospitals would seem to indicate, for the greater number of cases go untreated until a spontaneous cure is reached. The probability is, it is not always differentiated and recorded as a distinct disease, since in the statistics I have examined it seems to have occurred only 46 times in 15,169 children treated at the Children's Hospital, District of Columbia; 5 times in 2,058 at the Children's Hospital of Boston, and 4 times in 2,034 at the Children's Hospital of Philadelphia, or 55 times in 19,261 sick children.

It most frequently exists in children between eight and twelve years, but may occur at any time between two years and puberty. It is common to both sexes, white and black.

Trousseau was the first to trace a relationship between incontinence and epilepsy, claiming that in rare instances one succeeded the other, and that these histories always pointed to the transmission of one of the neuroses.

Some writers claim that there is a reflex relation between hip-disease and nocturnal incontinence, but the records of the Children's Hospital of this city, where a great many cases of hip-disease are treated annually, do not accord with this statement.

The following cases include some of each of the three classes :

CASE I.—Earnest P., four years, W., was admitted to the service of Dr. F. A. Ashford, at the Children's Hospital, D. C., during my term as resident physician, March 11th, 1879. He had had difficulty in retaining and passing urine for several years. There was now constant dribbling. March 16th, a calculus weighing 110 grains was removed by the lateral operation, and the patient was soon after discharged cured.

CASE II.—Sarah C., fifteen years, C., was admitted to the service of Dr. Busey, in the same institution, October 28th, 1876. She had had constant dribbling of urine for seven years. She was treated with tincture of belladonna,¹ and was discharged cured December 4th, 1876.

In such cases as this, it is not always easy to determine the pathological condition. Usually atony or paralysis of the bladder, enlarged prostate, or stricture of the urethra is present.

CASE III.—Frank C., seven years, W., was admitted to Dr. Ashford's service April 19th, 1879. He had had dysuria for some time. As soon as he desired to urinate, he had to run to the closet to avoid wetting his clothes. Wet the bed at night. Blood passed in urine at times. April 24th, a calculus weighing one hundred and seventy-five grains was, with great difficulty, removed by the lateral operation, and the patient was soon after discharged cured.

CASE IV.—H. S., four years, C. M., was admitted to the same service September 12th, 1871. For two years he had had difficulty in retaining his water, and pain on micturition. A calculus weighing fourteen grains was removed by the lateral operation, and the patient was discharged cured.

CASE V.—L. M., twenty-two months, W. M., was admitted to the same service August 4th, 1873. Had frequent desire to urinate, at times painful, sometimes involuntary; urine offensive. A calculus weighing fifty-two grains was removed by the lateral operation August 11th, and the patient was discharged cured.

CASE VI.—S. T., eight years, C. M., was admitted to the same service August 5th, 1880. He had difficulty in retaining his water and sometimes wet his bed. Examination revealed phimosis. Circumcision was performed August 21st, and the boy was soon after discharged cured.

CASE VII.—C. W., four years, C. M., was admitted to the same service March 13th, 1882. He had had for some time frequent and difficult urination; at times involuntary. Circumcision was performed and the boy cured.

CASE VIII.—James S., four years, W. On the 19th of March, 1884, Dr. Busey invited me to assist in circumcising this boy. From birth the little fellow had been accustomed to pass his

¹ Dr. Busey's method of treatment is to give the tincture of belladonna at bed-time, and increase the dose one drop daily until improvement begins, and then hold that dose until a cure supervenes.

water frequently, but after straining for several minutes he would become tired and cease trying, apparently before the bladder was emptied. Dr. Busey's attention was called to this condition, and examination revealed an elongated prepuce with an opening about the size of a knitting needle. The patient at this time was pale, languid, thin, restless, slept at short intervals during the night; retained his water but only a short time. He, being etherized, was circumcised as described later on. The adhesions to the glans were so firm that it required a longer time than usual to break them up. He soon recovered from the operation and has not had any difficulty about urination since. Three weeks after the operation Dr. Busey informed me that I would hardly recognize the child. He had become fat, rosy, and cheerful.

It may be claimed by some that this case is not properly classed, as there was no actual incontinence. But if it is admitted that in a perfectly healthy child of his age the urine should be retained three or four hours, then an uncontrollable desire to pass water at shorter intervals being shown, it would seem to approximate incontinence, and hence this classification. Although I cannot state positively, still I venture the assertion that, when the desire to urinate came, if he had not hurried to a convenient place, he would have soiled his linen.

CASE IX.—Mary W., four years, C., was admitted to Dr. Busey's service April 19th, 1871. She had had incontinence and intense pain on micturition for one week; the incontinence mostly at night. She was treated with tincture of belladonna and was discharged cured May 17th.

CASE X.—W. H., twelve years, W. M., was admitted to the same service February 15th, 1879. Four years before he had had measles; from that time he had had incontinence day and night. He was put on the belladonna treatment and was discharged cured March 6th.

CASE XI.—Guy P., eleven years, W., was admitted to the same service September 3d, 1879. He would generally hold his water until the bladder became distended, when he lost control over the sphincter. He was improving rapidly under the belladonna when his parents removed him without the consent of the attending physician.

CASE XII.—Nellie C., eight years, W., was admitted to the same service May 9th, 1882. She had had incontinence day and night for six years. She was given the stigma of maize and belladonna, and was discharged cured June 11th.

CASE XIII.—Alice L., four years, W., was brought to me for an excessive leucorrhœa, and inability to retain her water for any length of time. At short intervals she would run for the closet but before reaching it she would lose control over the bladder. Examination showed the presence of ascarides in the rectum and

vagina. She was given santonin internally and enemata of aloes and milk, and was speedily cured.

Sometimes we meet with cases of incontinence with high-colored, acid, and offensive urine, when the administration of bicarbonate of potassium will quickly effect a cure.

Finally, another class is met with where there is an adherent prepuce with a collection of smegma behind the corona. In such cases breaking up the adhesions and cleansing the part will generally effect a cure.

CASE XIV.—Ida B., thirteen years, C., was admitted to Dr. Busey's service April 12th, 1883. She had had nocturnal incontinence for two years. She was treated with belladonna, and was discharged cured June 8th, 1883.

CASE XV.—Fred R., three years, W., was taken with balanitis in November, 1883. Upon examining him I found an elongated prepuce with an opening about large enough to admit an ordinary darning needle, from which was pouring a profuse purulent discharge. The inflammation was believed to be due to the phimosis and I advised circumcision. At this time I questioned the mother as to whether he was the subject of nocturnal enuresis and learned that he sometimes wet the bed, but was in the habit of calling her three or four times during the night to put him on the chamber. On the 24th of November, with the assistance of Dr. McArdle, circumcision was performed. The mucous membrane was adherent as far as the meatus and a large quantity of smegma was behind the corona. In ten days the patient was discharged and up to date (May, 1884) has not wet his bed, holds his water well during the day, and rarely has to be taken up at night.

CASE XVI.—Charles O., eight years, W. Four years prior to operating, his father informed me that the boy was in the habit of wetting the bed, at least once, and often several times, every night. At that time, I did not examine the boy, but accepted the father's statement that the prepuce was long, incapable of being retracted, and with an opening about large enough to admit a small probe. Owing to the father's prejudice against anesthetics, and my unwillingness to operate without them, the trouble continued unchecked. I refused to advise the administration of drugs, stating that, in my judgment, the incontinence would never be cured without circumcision.

In November, 1883, he had measles and was under my care. When convalescent and in good condition, I proposed circumcision and obtained the consent of the parents, as well of the patient, the latter being anxious to be cured.

On the 2d of December, with the assistance of Dr. McArdle, circumcision was performed.

While the night before the operation the patient voided his urine three times during sleep; since then, he has only done so twice, at long intervals. On these occasions, the incontinence was attributed to salty diet and the taking of large quantities of water.

At my request, Dr. H. D. Fry, of this city, gave me the follow-

ing details of a case that came under his observation. It properly belongs to class one.

CASE XVII.—“September 13th, 1882, I was requested by Dr. J. H. Davidson, of Montgomery Co., Md., to perform circumcision upon a patient of his who was suffering from incontinence of urine due, apparently, to an elongated prepuce.

“The patient was fifteen and one-half years of age, and fairly well-grown. The penis was small and undeveloped; and, extending from the end of the organ was a long, snout-like projection of preputial tissue which could not be retracted over the glans penis. His mother gave the following history of the case: Until the age of five years, he was not unlike other children, as regards both his general condition and the appearance of his privates. At this time, his penis broke out with ‘poison oak,’ and the prepuce could then be drawn back, in order to attend to the dressing of the part. After his recovery, the glans penis could not be exposed.

“When about eleven years of age, the parts became sore again, and from this time he had no control over the discharge of urine. His general health declined; complexion was bad; complained of pains in his side; and became nervous and irritable. His manner at school was so different from that of other boys that his teacher’s attention was called to his actions, and he spoke to the boy’s parents and advised them to have something done with him.”

“Such was the state of affairs at the time of the operation, and it had existed about four and a half years.

“I removed the redundant tissue back to the corona, broke away some adhesions that existed, and left the case with the request that no medicinal treatment should be employed. I wished to see the effect of the operation *per se*.

“April 28th, 1884, the boy’s mother wrote the following report: After the operation, he had no fever; slept well, without opiates; and suffered little pain. There was little or no suppuration. In a week, he left his bed and, in a month, returned to school. His general health, since, has improved slowly; is less irritable; complexion better; and retains his water.”

In the few cases of occasional nocturnal incontinence the cause may be an overloaded stomach or bowel, intestinal worms, hip disease, adherent prepuce, or amorous or lascivious dreams.

I myself do not doubt that, in cases of phymosis, the nocturnal incontinence is due to reflex irritation. In some cases, the irritant is the smegma behind the corona which keeps up a constant excitation. During the day the will is powerful enough to overcome the action of the spinal centre presiding over urination, while at night the will is asleep, and the reflex reaches its maximum. In other cases, owing to the contracted orifice of the prepuce, the bladder becomes tired from prolonged ex-

pulsive efforts, and relaxes before it is emptied. During the day, frequent micturition is observed, but the bladder is not emptied; at night, the desire to urinate is just as frequent, but the will is asleep, and the spinal centre responds to the irritant by involuntary micturition.

To determine the pathological condition is not always an easy matter. Atony of the sphincter vesicæ, or spasm of the detrusor urinæ, may be the cause. The controlling power over the sphincter, which is largely reflex, may, by the influence of the will, prevent the escape of urine during the day; but at night, when the will is asleep, this power is relaxed, and the consequence is involuntary micturition, regardless of cerebration.

As I do not admit that incontinence is frequently the offshoot of fear or laziness, of course I hold that moral suasion and corporal punishment are not efficient correctives, and equally, of course, I contend that, in most cases, the indicated procedure is either remedial or operative.

It would be more interesting than instructive to examine the literature of the internal remedies which have been successful in these cases. Nearly every drug in the *materia medica* has been tried with equal success, if we are to credit the books. Drugs physiologically and chemically incompatible have been combined, and success claimed for them. Accepting the theory of a want of tonicity in the muscular walls of the bladder, some have used remedies that should have aggravated the trouble, and yet have claimed a perfect cure. In such cases, the results were probably accidental, and most likely were brought about by nature, in spite of the treatment.

Leonardi and others extol chloral, but others have failed to derive any benefit from its use. It acts by allaying the reflex irritability in the cord. I do not approve of the use of this drug with children; certainly not except with the utmost caution. Owing to the variability in strength of the preparation, and the uncertainty of its physiological action, as well as to individual idiosyncrasies, it must, in my opinion, be regarded as one of the most dangerous and uncertain remedies in common use.

The bromides take foremost rank in the treatment of those cases in which an exalted nervous condition can alone account

for the incontinence. They should be given in large doses at bedtime.

But belladonna is the remedy *par excellence* in the treatment of those cases believed to be associated with a tonic spasm of the bladder. One of its physiological actions is to relax the tonic contraction of the involuntary muscular fibres. The muscles of the bladder being of this class, when the atropia in the urine comes in contact with the walls of the bladder it allays irritability and relaxes spasm. In order to derive benefit from the drug it should be given in large doses at bedtime, which should be increased, drop by drop, daily, until improvement results or its physiological effects are obtained. It must be borne in mind that children will bear much larger proportional doses than adults. I speak in the highest terms of this drug because, as will be seen from the foregoing cases, it alone produced beneficial results in about four weeks.

If there is a relaxation of the sphincter vesicæ, or paresis of the muscles the bladder, strychnia is indicated. But as I have never seen a case of this kind, I have had no experience with it.

If the patients are puny or in ill-health, we would naturally expect better results by improving the general health while we are administering remedies.

Experience has taught me that the best success attends those patients who are treated in hospitals. In these institutions the diet of the child can be regulated, and many advantages gained in treatment. Parents will insist and believe that the physician's instructions are carried out in full; but they forget the sympathizing friends about the house, who think the child unduly restricted, and who are, therefore, willing and ready to cater to its whims. For this reason, it has been my custom to advise children to be placed in the hospital. If the patient can be fully controlled, success will quickly attend the treatment indicated.

In order to make statistics of operative procedures valuable, the use of remedies should be suspended. If we operate then, the operation should stand upon its own merits, and not be embarrassed by drugs. After a circumcision, if belladonna is given, how can we tell which means effected a cure? In the cases above reported as operated on, nothing but soporifics were given.

I do not advocate circumcision as the certainly indicated remedy in all cases, nor do I believe that every boy who has incontinence, with an elongated prepuce, should be compelled to undergo the operation. If the prepuce cannot be retracted, then I would advise operating; and while sometimes the opening in the prepuce is large enough, yet it cannot be retracted owing to adhesions. In such cases, if these adhesions are broken up, favorable results will follow.

I prefer to perform the operation of circumcision in the following manner: The prepuce is drawn forward and Henry's clamp tightly applied; the end of the prepuce is then cut off with scissors and the clamp left on the stump for several minutes to check bleeding; a director is then pushed along the upper surface of the glans, and the mucous membrane divided beyond the corona. The membrane is then turned back to meet the retracted skin, and made fast by five silk sutures. In about ten days the patient is well. I remove only that part of the mucous membrane that is cut off with the end of the prepuce; for by leaving a long membrane it can be turned back and thereby hide the cicatrix, which is a source of mortification to many parents. In Cases VIII., XV., and XVI., there is nothing to attract the attention to circumcised organs.

One objection to the operation is that the glans will be left uncovered and that it destroys, to some extent, the sensitiveness of the organ. If this were true it would in many cases prove a great blessing, but unfortunately it is not true. Another objection is that the beauty of the organ is destroyed, whereas, in fact, if the circumcision is neatly done, the penis presents a far better appearance than it does with a redundant foreskin.

From the foregoing cases, which have invariably been attended with complete success, without the administration of a drop of medicine, is it to be wondered that I so strongly favor circumcision?

Some have advised forcibly stretching the contracted prepuce, a process both tedious and unsatisfactory.

Teewan, a British writer, advocates slitting the meatus; which he thinks is too small in most children with incontinence. As I have not seen any cases requiring the operation, I must accept his statement that benefit was derived from it.

There is one other class of treatment discussed and recommended in the books, what may be called the mechanical.

Many, I dare say, have been called to treat an edematous prepuce caused by tying a string around the penis to prevent bed-wetting. Perhaps some little fellow, after frequent thrashings has finally, in sheer desperation, conceived the idea of eluding the lash by tying up the escape-pipe. He may have missed the chastisement, but he paid dearly for the immunity.

Corrigan, apparently a writer of note in Great Britain, deliberately and strongly advocates hermetically sealing the prepuce with collodion. He stretches the prepuce and forms a pouch into which the collodion is poured, solidifies and thereby prevents the escape of urine!

Compression of the membranous portion the urethra by a pad, and packing the vagina have also enthusiastic devotees!

Knotted towels, spiked belts and other uncomfortable rigging to prevent the child from lying on its back have also received their share of commendation!

It seems hardly necessary to comment on these various mechanical methods of treatment, since they seem to evince a hopeless ignorance of the etiology of the disease, and are not only valueless, but actually harmful.

The bibliography of this subject has been very carefully examined, with the courteous and efficient aid of Drs. Fletcher and Wise of the Surgeon-General's Office, U. S. A., and I believe the following is a measurably complete list:

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CORRESPONDENCE.

TO THE EDITOR.

DEAR SIR :—In your edition of April, 1884, I had the pleasure of reading a communication from Dr. C. C. P. Clark, of Oswego, New York, concerning the use of morphia hypodermically in infantile convulsions.

Shortly after perusing the article, I was summoned to see a child, boy of fifteen months, who had been in a continued convulsion for about an hour. When I arrived nothing had been done or given, and, as the convulsions were extremely violent, without trying any of the ordinary remedies I injected subcutaneously four minims of Magendie's solution.

In two minutes the child began to get quiet, and in fifteen was sound asleep.

He awoke in six or eight hours all right, and has had no return of the trouble. As may be imagined, I watched the child very closely for a few hours, but could not notice any unfavorable symptom from the morphia.

A single case is not enough to base an opinion upon, but from the happy result in that case I shall not hesitate to try it again.

G. E. GOODFELLOW, M.D.

TOMBSTONE, ARIZ. T.

ABSTRACTS.

1. Lindner: **What Influence does the Discovery of the Tubercle-Bacillus Exert on the Theories of Granulating Articular Inflammation?** (*Jahrbch. f. Kindhdkde.*, XXI. B., 1 u. 2 H.).—By granulating inflammations of joints, the author means those affections which were formerly called fungous inflammations, arthroceae, and other names, an inflammatory process which, under the influence of a slight but continuous irritant, does not lead to acute cell proliferation or suppuration, but to the development of a product resembling the granulations of a wound, yet not having the power possessed by these latter to change at a certain stage of their growth into cicatricial tissue. For a long time, the coincidence of occurrence of this and "scrofula" or tuberculosis was noticed by surgeons, but it was not till Virchow brought light into chaos by his description of the miliary tubercle as the characteristicum of the disease and his explanation of caseous metamorphosis, etc., that a distinction could be made between the so-called scrofula and the caseous processes of tuberculosis. The next step was the discovery of miliary tubercle in the granulations in these joint diseases, and the general view of late has been: "The granulating joint inflammations are either a symptom of a general tuberculosis, or the first localization of tuberculosis in the body, a localization which may usually remain single, but which, under favorable circumstances, may lead to infection of the whole organism and an outbreak of tuberculosis in the most different organs." After Koch's discovery of the tubercle bacillus and the repeated confirmation of his views by its constant discovery in the granulations in affected joints, too much was at first anticipated. König guarded against any too great anticipations by writing that Koch's discovery was nothing more in these cases than a confirmation of what surgery already knew. It is the author's purpose to show that the influence it should exert is, however, of more weight than this. In regard to the pathology of the condition he agrees with König, but he claims that Koch's discovery of the parasitic nature of the disease will influence our treatment, not only by making clearer the rationale of some methods of procedure, but by modifying the technique of some operative measures.

Some years ago, Hueter, *supposing* a parasitic cause for the disease, treated it with injections of carbolic acid with some success. Others

failed with the treatment, and it has fallen into disuse. Now *knowing* the parasitic nature of the trouble, the author claims that careful examination should be made of Hueter's method. We should find what parasitocides are most effective and least harmful to the tissues, and endeavor by all means to ascertain if there is not some substance which will meet these conditions. The experiments are difficult, but theoretically they should succeed. Let us suppose we have a substance which is death to the parasite, but safe to inject into joint or bone, what may we expect from it? In children, almost all the cases are of osseous origin. In the apophysis of one of the articulating bones, there first forms a little collection of tubercular bacilli. This, by its growth, destroys the bone, enters the joint, and invades synovial and other tissues. It certainly would be possible to destroy this primary collection if we could reach it with our interosseous injection. Let experiment decide what the injection shall be, and let all methods of diagnosis be perfected (bone-percussion, etc.), that these spots may be early discovered.

After the process has invaded the whole joint, the author believes but little is to be expected from antiparasitic injections. There would always be some point unreached and "now," when we know that *one living bacillus* which may remain in the joint is sufficient for the reinfection of the otherwise entirely healed surface," we may be sure that complete cure would be the rare exception. This view has been substantiated in frequent cases in the author's experience, an apparent improvement being only temporary; and he advises for these cases the removal of the whole disease by resection. The same remarks apply to ignipuncture. It can only be of service when the growth of bacilli is limited to a small area. The frequent washings of joints with dilute antiseptic solutions has also proved of absolutely no value.

More rational is the removal of the disease by arthrotomy and destruction of the diseased tissues either with the sharp spoon or the actual cautery. Here, too, success would depend greatly on the extent of the disease. In one case reported, the author had eminent success, but he thinks such cases will be rare.

In discussing medical means for treating these cases, he entirely repudiates arsenic, and is strongly opposed to wasting and losing the best time for operation by sending patients to sea-salt or sool-baths, as is too often done. It is all right to build up the system for a better battle for existence, but it is much better to kill the bacilli, to remove the disease, and then build up the system.

In regard to operative methods, the point made by the author is that we should not confine ourselves to the longitudinal incision parallel to the muscular fibres as advised by Langenbeck. It is best to do the operation subperiosteally, but it is of so much more importance, according to our new light, that every minute portion of the lining membrane of the joint should be removed, that we should not hesitate, operating under antiseptic precautions which allow of healing per primam of all tissues, to make any incisions, transverse or otherwise, which may give us a better view of the diseased joint. The new methods of operating (by Volkmann, Hueter, Beck, Vogt, and others) are in accordance with this principle. It is one of great importance, and the author believes that this influence alone of Koch's discovery is of very great weight, to say nothing of its making us clearer and surer in our other therapeutical measures.

J. F., JR.

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ORIGINAL COMMUNICATIONS.

OUR PRESENT KNOWLEDGE OF THE RELATIONS BETWEEN
MICRO-ORGANISMS AND PUERPERAL FEVER.

BY
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THE recent discussion in the New York Academy of Medicine upon puerperal fever demonstrated very forcibly what various opinions are still held upon this subject, and especially as regards its contagious principles, by some of the leading members of the profession in America. But ideas expressed a few years since in the memorable discussion of the London Obstetrical Society show that in that country the subject is considered from still more widely divergent standpoints.

A certain stage of agreement has no doubt already been attained, based upon similar views and opinions, but what we now especially desire are facts, not arguments; facts proving whether certain small micro-organisms found in this disease are or are not its cause, and if they are, what rôle they play and what their presence signifies. The opinions of more experienced physicians are, no doubt, of great value in a question of this nature, but they still remain more or less questionable as long as positive facts are not brought forward to substantiate them.

It is not the object of the present paper to attempt to solve

these questions, but one of a much more humble nature, convinced as I am that the puerperal diseases form an extremely complicated network, which will yet require the work of many men before everything appears clear and comprehensible to us. Leaving aside all theoretical deductions and personal opinions as far as possible, my intention is to collect the *facts* which have thus far been offered as such, regarding the relationship which exists between micro-organisms and puerperal fever. The reader may then form his own opinions, but may rest assured that I treat the question with the utmost impartiality. I shall consider the subject under different headings, taking each in its turn, separately, referring to and quoting different authors, and hope in this manner to render it less complicated, and at the same time more thorough and interesting.

I.—*What Facts do we derive from Pathological Anatomy?*

Mayerhofer¹ was among the first to direct his attention to this line, and was the first who discovered micro-organisms in purulent fluids from the pleural and peritoneal cavities of puerperal patients. These he describes as long jointed and disjointed threads, or small round bodies arranged as threads. Coze and Feltz² saw, in 1869, in the blood of puerperal fever patients, "round corpuscles arranged in form of chains." Then comes Rindfleisch,³ who mentions having found micro-organisms between the muscular fibres of the heart, and afterwards Recklinghausen,⁴ who finds the same variety in abscesses.

Waldeyer⁵ found them in the parametrium, and in pus from the peritoneal cavity; he could follow them through the lymphatic vessels of the diaphragm into the pleural and pericardial cavities, and describes them as round-shaped micrococci, in the form of chains, which were composed of from two up to ten members.

Orth⁶ again found similar ones in peritoneal pus, and also in blood taken from the heart, in fresh cadavers. At the same time as Orth's publication appeared, Heiberg describes some

¹ Mayerhofer: *Monatsschrift f. Geb.*, vol. xxv., 1869, p. 129.

² Coze and Feltz: *Gazette Médic. de Strassb.*, 1869, p. 30.

³ Rindfleisch: *Lehrbuch der Path. Anat.*, I. edit., p. 204.

⁴ Recklinghausen: *Centralblatt f. Med. Wissensch.*, 1871, p. 713.

⁵ Waldeyer: *Archiv. f. Gyn.*, vol. iii., p. 293, 1872.

⁶ Orth: *Virchow's Archiv*, vol. lviii., p. 441, 1873.

⁷ Heiberg: *Die puerperalen u. pyämischen Processe*, p. 14-39, 1873.

round corpuscles arranged as long chains, which he found in abscesses of the uterine wall, in pleural and peritoneal exudations, and in the heart and kidneys. Klebs¹ saw some very similar to these in the ventricles of the brain, and in one case, also another species, in the form of bacteria.

Wolff² likewise found in peritoneal exudations, besides the chain-like micrococcus, other bacteria. Litten³ observed meningeal hemorrhages, in which the capillaries were found filled with micrococci, the same being also found in abscesses of the brain; and Pasteur⁴ the chain-like form in peritoneal fluids and in cases of suppurative arthritis, where he likewise demonstrated bacteria.

Again, Ehrlich⁵ pointed out, besides the ordinary micrococcus, different sorts of bacteria both in the blood and in pus from cases of empyema. Recklinghausen's investigations were lately confirmed by Steurer,⁶ who was able to follow the round chain-like micrococcus from the diphtheritical lining of the endometrium through the uterine lymphatics into the peritoneal cavity. In other organs, especially the kidneys, he also saw them. Fraenkel⁷ several times found the same in peritoneal exudations.

According to this enumeration, it would seem indeed as if all had been successful, and in no place in the literature can I find mention as to the contrary. The chain-like form were found in every case, and in some, varieties of bacteria were also observed.

Let us now consider those cases in which the former only were found. These, as a rule, were discovered without the authors having knowledge of the existence of any similar publications, and as they all give similar descriptions, and the drawings being similar, it is extremely probable that what they saw were morphologically the same form. We also found them in three cases which occurred during the past winter in the University Polyclinic which is connected with the Woman's

¹ Klebs: *Archiv f. experim. Pathol.*, vol. v., p. 417.

² Wolff: *Centralb. f. Med. Wissens.*, No. 32, 1873.

³ Litten: *Zeitschrift. f. Klin. Medicin.*, p. 408, 1880.

⁴ Pasteur: *Comptes rendus*, 1880, p. 1038.

⁵ Ehrlich: *Charité-Annalen*, vol. vii., 1882, p. 206.

⁶ Steurer, Lusk: *Midwifery*, 1882, p. 617.

⁷ Fraenkel: *Deutsche Klin. Wochenschrift*, 1884, No. 14.

Hospital in Berlin. These patients were delivered at different times, in different parts of the city, and no two attended by the same student or midwife, so that it was impossible for the same contagion to have been at fault in each case. The first was as follows :

CASE I.—Bertha S., thirty-six years of age, was delivered spontaneously at her residence on the 10th of December, 1883, in the presence of the midwife, who sent for assistance on account of post-partum hemorrhage. The student who took charge removed a portion of the placenta manually from the uterus. Severe illness ensuing, she was received into the clinic, and found to have parametritis on the right side, besides endometritis with fetid discharges. High fever, frequent rigors, pains in the joints with swelling, and diarrhea were the most prominent symptoms. Permanent intrauterine irrigation was employed for the first few days, when the discharge ceased, although the further course of the disease did not seem to be materially altered. On the 23d, *i. e.*, a fortnight after confinement, pemphigus appeared upon the hands and feet, and a few days afterwards upon the arms and legs. The fluid contained in the vesicles was slightly clouded, and microscopic examination showed it to contain enormous quantities of the round, chain-like micrococci, also some flat epithelial cells and detritus, but no pus. The vesicles shortly after broke, and soon dried up, leaving thin superficial crusts or scales. Meanwhile, new ones appeared, but they all contained myriads of the micrococci. Frequent vomiting and diarrhea occurred, and the vesicles kept appearing until the 31st, when the temperature fell, and we were inclined to think that the patient would recover; but suddenly, on the 7th of the following month, she died. This occurred thirty-one days after delivery, fourteen days after the appearance of the pemphigus, and seven days after its disappearance. The urine had frequently been examined, but showed only slight traces of albumen. *Post-mortem* showed endometritis and metritis, with small abscesses in the uterine walls, the pus from which contained great quantities of the same micrococci found in the vesicles. The kidneys were enormously enlarged through multiple abscesses, which had destroyed the greater part of the tissue, and in these again the micrococci were found, as also in the liver and spleen, which were both enlarged. Beyond this, adhesive pleurisy, with a small abscess of the lung, was present.

CASE II.—Wilhelmine V., æt. 37, IXpara, was delivered in this city on the 7th day of January, 1884, turning and extraction being necessary on account of placenta previa. Loss of blood not great; operation easy. Twenty-four hours after the confinement, patient was taken with a chill, and symptoms of peritonitis setting in, she was received in the clinic on the 11th of January. Examination showed general peritonitis, with a dif-

fuse phlegmon of the right labio-crural region, phlegmon of the lower part of the left leg, with varicosities in a state of superficial gangrene, and diffuse phlegmonous swelling of the right hand and lower part of the right arm, also exquisite euphoria was present. On the following day—five days after the confinement, patient died, shortly before which we had drawn a small quantity of blood (with all the necessary antiseptic precautions), and found it to be crowded with chain-like micrococci, each chain being composed of from two to twelve members, and to every two or three one blood-corpuscle being present. On *post-mortem*, performed *one hour after death*, a necrotic endometrium, with purulent peritonitis and the above-mentioned phlegmonous processes, were found. In the peritoneal cavity pus, as well as in specimens of the liver and kidneys, the same micrococci could be traced, and in the lymph from the phlegmona; besides these, we also found a variety of bacteria.

This case was especially interesting on account of the astonishing rapidity with which the morbid processes developed. For instance, on the evening of the fourth day, an insignificant swelling appeared upon the right arm, and fourteen hours later the whole hand and lower part of the arm were transformed into an edematous tumor, with the integument of a greenish-violet color. Then, again, the enormous number of micrococci was particularly striking. A drop of blood is but a small fraction of the total amount in the living body, and of this drop but a small portion is taken for microscopical examination. If, then, from this small portion but a minute part comes under the field of observation at one time, and we then see them in such numbers, the total for the whole body must indeed be something enormous, and one can easily imagine with what rapidity they must have increased, for the whole sickness only lasted five days. Comparing the cases, we find that the same variety was found in both, at least morphologically the same, and that in one patient they were able to circulate in the system for a fortnight, for they were found this length of time before death (which took place on the thirty-first day) in the pemphigus vesicles, and in the other death took place five days after delivery. Possibly the presence of another micro-organism in the last case might explain the different courses.

CASE III.—Louise S., æt. 30, Viipara, confined in her lodgings February 27th, 1884. Twin birth; both children born spontaneously in the presence of the student. Twenty-four

hours afterwards, chill and symptoms of peritonitis came on, when she was transferred to the clinic, and died on February 29th of general peritonitis. The blood was frequently examined, but we were never able to discover micrococci. At the *post-mortem* examination, *one hour after death*, we found diphtheritic endometritis, a small abscess in the wall of the cervix, and extending from it, purulent infiltration of the right parametrium, with diffuse peritonitis, also serous pleurisy and beginning endocarditis. Micrococci were then found, not only in the pus from the peritoneum, but in all the organs.

Let us now, perhaps, recapitulate the facts which we have thus far been able to gather from pathological anatomy :

I. The chain-like micrococci have frequently been found both in the exudations and in the organs of patients having died of puerperal fever.

II. All observers have noticed the same characteristic variety.

III. Different species of them have hitherto never been described.

IV. It seems as though all who sought for them have been able to find them in every case.

V. More recent researches have shown that, besides this chain-like micrococcus, other micro-organisms, *i. e.*, bacteria, can be found in puerperal fever.

Let us now proceed and ask the question what have we learned from experiments, and what from the cultivation of these micro-organisms.

II.—What facts have we derived from the cultivation of micrococci ?

The first which we find in this connection are the experiments of Coze and Feltz¹ in 1869, when they attempted the cultivation of blood from puerperal patients in sugar water. Their results were negative. Orth² found that the chain-like micrococci could be kept quite a length of time without change of character, and without the appearance of resultant bacteria. He therefore concluded that they must be beings *sui generis*.

Pasteur³ cultivated blood in chicken bouillon, and found

¹ L. c., p. 42.

² L. c., p. 443.

³ L. c., p. 1038.

in one case that three days before death he could demonstrate germs capable of growth, which on microscopical examination proved to be the same chain-like form; and which without doubt were, morphologically speaking, the same as those found by other observers in the cadaver. He describes them as: "*des chapelets flexibles. On les voit en petits paquets encheretrés comme des fils de perles brouillés.*" In other cases he found still another variety, and these he calls "*organisme du furoncle.*" Chain-like micrococci were detected in the walls of the uterus, in abscesses and in the blood, and then again in his cultivations of patient's milk, but frequently he was unable to detect any germs capable of growth, either in the blood or in the lochia, although micrococci in great quantities after death were found. His next experiments are of special interest. From the blood of a rather sickly new-born child, whose mother was presumed to be suffering from puerperal fever, he cultivated a form of bacteria which he calls "*vibrion pyogenic.*" From the mother he was unable to obtain anything, and at her death nothing relative to puerperal fever whatever was found, the genital organs being quite normal. She had, however, an old hepatic abscess, and in pus taken from this he discovered the same *vibrion pyogenicum*. His second experiment was with the blood of a patient who had suffered from *tympania uteri*, and who was thought to be so severely infected that she could survive but a few days. Here he was unable to find anything capable of growth, and the woman afterwards recovered. These observations are of great interest and importance. They prove the possibility of cultivating germs from patients suffering with puerperal fever, and show that not only can these germs be found in such patients, but that when puerperal fever as such does not exist, they cannot be found.

These facts, together with those furnished us by pathological anatomy, attract our attention more and more to the round chain-like micrococci.

Pasteur's investigations seem to have been published in rather a remote corner and have consequently not been properly brought into notice. We ourselves were quite ignorant of them, until recently similar experiments had been made with material from our clinic. After our having found micrococci in such enormous quantities in the case of pemphigus, Prof.

Schroeder requested that our investigations might be continued at the Imperial Health Bureau.

Dr. Struck, director of the above institution, therefore authorized Dr. Jovanovic to experiment with the material furnished by the clinic, and with their kind permission we are enabled to publish what little we already possess on the subject. I should remark, however, that the experiments are far from being completed, and for further results I must refer the reader to publications which will later appear from the Imperial Health Department.

Our first endeavors were connected with the case of pemphigus, where we tried, although unsuccessfully, to cultivate the fluid from the vesicles. After this we made blood-tests of every patient suffering from puerperal fever in the hospital, the microscopical examinations been partly carried out in the clinic, partly in the Health Bureau by Dr. Jovanovic. In the case of patient No. II., the micrococci already alluded to were discovered by Dr. Jovanovic. A number of light cases of fever were examined, but with negative results. The method was as follows: after the thumb of a patient had been thoroughly cleansed with soap and water, disinfected with a sublimate solution of 1 : 1,000, and wiped off with alcohol, an ordinary needle, previously having been made red hot, was made to puncture the skin, and the blood drawn off into capillary tubes which had also been subjected to the heating process. Only once, however, were we successful in detecting the micrococci, and in this case we cultivated the blood.

Trials were made by Dr. Jovanovic with the pus taken from the parametritic abscess of case III. with all the necessary precautions, and here eight generations were obtained, by inoculation from one tube to the other. The medium used was a composition of sterilized meat extract, peptone, and gelatin. In shape, the colonies were seen as small, white, round, lucid spots, arranged in the manner of rose beads. Their increase was slow, and it was found that they could also be generated upon fresh potatoes, and that differences of temperature had no effect whatever upon them as regards their powers of multiplying. Microscopically they formed long chains, often twisted and united so as to form bundle-like masses, the chains themselves as regards form and length seemingly dependent upon

the presence or absence of resistance in the surrounding medium. Thus, in fluids, it seems that they can grow to an unlimited length (in the peritonital exudation they were of quite good length), while in the organs they are met with in groups or masses, and only where the tissue offers less resistance, as in the lumen of a blood-vessel, can they again be traced in their chain-like form.

It should be remarked that in three cases examined, no morphological difference could be detected, nor could any difference in their manner of increase or growth be discovered.

III.—What facts have we derived from experiments upon animals?

Coze and Feltz¹ experimented upon rabbits with hypodermic injections of blood taken from puerperal fever patients. Most of the animals died of diarrhea and convulsions, the same symptoms occurring when the blood of one of the infected animals was used as the injecting fluid. In this blood the observers found “*de petites chainettes à 2, 3 et 4 grains disposés quelquefois en ligne droite, quelquefois de manière à former des angles,*” which description showed us that the same chain-like micrococcus was present.

Eberth² inoculated the corneæ of rabbits with croupous exudations from women who had died of puerperal fever, and found that the diphtheritical process was continued upon the tissue of the cornea, and that microscopical examination of the masses showed that chain-like micrococci were present here also. Orth³ obtained the same results after similar experiments. Twelve hours after the inoculation, he was able to demonstrate with the magnifying glass small white spots issuing from the spot at which the poison had been introduced, which again upon examination proved to be masses of the chain-like micrococci. Inoculations from one animal to the other gave similar results, and from these experiments no deaths occurred, but they did occur when he injected peritoneal fluid, and here again the micrococci were found. Heiberg,⁴ however, on the other hand, tried in vain to kill the animals, using the same fluid as injecting material.

¹ L. c., p. 30 and 38.

² Eberth: *Centralblatt f. Med. Wiss.*, No. 8, 1873.

³ L. c., p. 449.

⁴ L. c., p. 42.

Dr. d'Espine¹ injected pus from a patient who had died of puerperal fever into the uterus of a rabbit, the animal dying from peritonitis with abscesses of the uterus.

These experiments show that with impure material for injection, no typical results can be obtained.

The investigations now going on in the Imperial Health Bureau have not as yet been carried sufficiently far to warrant any decisive results being published, and on this question we must again refer to later works from this office.

Through a paper recently read by Fraenkel² before the Society for Internal Medicine in Berlin, we get some information as regards injections with pure material. After cultivating peritoneal fluid from patients with puerperal fever, he uses this for the injections, but obtains no *uniform* results, viz.: Guinea pigs were not in the least susceptible to the poison, and mice only died after injections of enormous quantities, while rabbits were either not at all affected, or only exhibited slight temporary elevations of temperature. Even one-half syringe-full of the parasites introduced into the jugular vein seemed sometimes to have no effect, although in some cases the animals died. In these latter cases, chain-like micrococci were found in the blood and in the spleen. What seemed to have the greatest effect and result was the injections into the pleural cavity, the exudations following which being found full of the organisms, and these seemingly being much more virulent than the original poison. Fraenkel lays particular stress upon the fact that erysipelatous processes showed themselves in many of his experiments.

In the foregoing, I have endeavored to lay before the reader all the *facts*, as such, which we are able to collect in the literature upon the subject, and which have up to the present day been made known to us. Theory and all preconceived ideas I have purposely omitted. The compilation of such facts might confer upon some the idea that there could be no doubt as to the so often observed chain-like micrococci being the real cause and transmitters of puerperal fever; but by the following I hope to show that we cannot be too cautious in drawing our conclusions.

¹ D'Espine: Contribut. à l'étude de la sept. puerperale. Paris, 1873.

² L. c.

In the first place, it is not at all satisfactory that the experiments upon animals have given such inconstant results, and, again, according to Koch, it is only allowable to call a micro-organism pathogenetic, when its presence has been proved anatomically in the body, and also when, after being introduced into another body, *the same process* is produced in this second instance.

Now, we have seen the first of these conditions fulfilled, but not the second. In Fraenkel's experiments with pus from puerperal patients, the animals seem to have gotten no peritonitis whatever, and he himself says: "Even after injecting the products of the cultivations directly into the abdominal cavity, I often saw no result." This may, however, have been caused by rabbits (upon which he experimented) being but slightly susceptible to the poison, for upon the guinea pigs no effect at all was observed.

Inoculations upon human beings have not as yet been made.

Let me now ask what we really do know about the chain-like micrococci.

Are there different varieties, different species, varying in size, in disposition, and in their relations towards tissues, and is their behavior towards coloring substances different?

It is very probable that such is the case, and that in time we will be able to distinguish them; but I would merely like to lay stress upon the fact that hitherto we have had no positive or definite knowledge as to differences which would make a differential diagnosis accessible to all. What has been published as to this point is but meagre.

Fehleisen¹ believed to have found a variety which he says are characteristic of erysipelas, and states in his book upon the subject that he was able to discern several other sorts of chain-like micrococci such as occur in wounds and in the discharges of phlegmonous processes, but he does not mention in what their differences consist.

Doleris² affirms to have seen several varieties, mostly distinguishable by their size, and Koch³ found a species causing the "gangrene of mice," their diagnostic point being their typical

¹ Fehleisen: *Etiologie des Erysipels*, 1883, p. 17.

² Doleris: *La fièvre puerpérale*, p. 202.

³ Koch: *Etiologie der Wundkrankheiten*, 1878, p. 47.

and constant action. Morphologically, the latter could, however, not be distinguished from other chain-like micrococci. In the same publication in which he describes these experiments, he expresses his astonishment at the curious similarity in the morphological structure of the micro-organisms in erysipelas, pyemia, diphtheria, and in puerperal fever. Let us note his views, and ask ourselves to what degree this coincidence can be maintained to-day.

Relations to Scarlet Fever and Diphtheria.

Loeffler¹ lately published an extremely interesting paper concerning some experimental studies which he made in the Imperial Health Bureau upon cases of diphtheria complicating scarlatina. He states that the chain-like micrococci were found in great quantities in the mucous membrane of the throat, and in many cases had penetrated into the deeper tissues, from whence they were carried to all organs of the body. In simple diphtheria they were also found, but the condition was mostly characteristic of cases complicated with scarlatina. He did not look upon these organisms as being the cause of the disease, but thinks that their presence was merely accidental, and explains his views as follows: Given one primary noxious cause which infects the body locally with diphtheria, we have the field thus prepared for other micro-organisms accidentally present; the latter then, finding full conditions of existence and increase present, also invade the body and thus a *mixed infection* results.

Now the very fact that the chain-like micrococci were found especially in those cases complicated with scarlatina is in itself remarkable, for let us bear in mind that many of the foremost English obstetricians, such as Playfair,² Braxton Hicks, etc., are fully convinced that scarlatina is one of the possible causes of puerperal fever, and even those who will not admit this as doctrine, have certainly seen so-called cases of septic exanthema in the course of puerperal fever which were barely if at all distinguishable from true scarlet fever. Litten³ has described several cases of this sort, where he says that both the

¹ Loeffler: Mittheilungen aus dem Reichs-Gesundheits-Amt, 1883.

² London Obst. Transact., Vol. xvii., p. 110 and 201.

³ Litten: Zeitschrift für klin. Medicin, 1880, p. 432.

septic exanthema and the scarlatina complicating puerperal fever present quite the same appearance on the integument as ordinary scarlet fever. In reading works by such authors as D'Outrepoint and Berndt, one cannot but be struck by their descriptions of the integumental changes in puerperal fever. However these cases may be explained, in one way or the other, the appearances upon the skin were always the most characteristic symptoms. The literature upon these last points is especially rich, as evidenced by the latest work by Legendre,¹ who alludes to cases by the hundreds.

The position taken by scarlet fever as regards puerperal fever is quite different compared to that of our other infectious skin diseases, for who, for instance, has ever heard of measles causing and complicating puerperal fever? As a result of Gautier's² late search of the literature upon this point, but nine (9) cases could be found which had occurred during child-bed, and several of these were of a doubtful nature, most of them passing off without causing any especial disturbances, although several of the children were said to have had an unquestionable measles eruption. But how is it that scarlet fever plays such an entirely different rôle, if there is no connection between it and puerperal fever? It is in order to lay stress upon the *relations* of the two diseases that these cases have been cited, and authors referred to, not in any way to prove that they are identical, although the micrococci found in both have not as yet been shown to be different.

Now, as to the relations between puerperal fever and diphtheria. Clinically the connection between these two has been frequently mentioned, and has been made especially apparent by the similarity of the local pathological changes. Diphtheritical endometritis is certainly a very common occurrence in puerperal fever, and E. Martin³ even thought it was the real essence of the disease, although this we know is not strictly the case, for in some cases it is not at all to be found, while in tympanites uteri the most exquisite examples (taken in the true pathologico-anatomical sense of the word) are sometimes seen without puerperal fever being present. I mentioned the fact that

¹ Legendre : Etude sur la scarlatine. Paris, 1881, p. 65.

² Gautier : Annales de Gynécol., 1879.

³ E. Martin : Berliner klin Wochenschrift 1871, p. 32.

the organisms found in puerperal fever and those in diphtheria complicating scarlatina were morphologically the same. In both diseases they have been cultivated, and it has been found that their growth is precisely the same; from both they have been grown in hard gelatin, on potatoes, and in fluids, and no difference whatever could be detected. For this information I am indebted to Dr. Loeffler himself.

Relations to Erysipelas.

One of the most interesting facts brought forward by Loeffler is that no differentiation could be made between the chain-like micrococci found in diphtheria and those described by Fehleisen as specific for erysipelas. He made studies of both diseases for a period of more than two years without being able to demonstrate that they were not the same, perhaps the only noticable point being that Fehleisen was not able to detect them in the blood-vessels in erysipelas, whereas Loeffler found them in the blood-vessels in cases of diphtheria. As regards erysipelas and puerperal fever, the similarity has been noticed by a number of experimentalists; Koch, as already mentioned, being one of them, still later Doleris¹ and Fraenkel.² Years ago Virchow³ pointed out the similarity between the pathologico-anatomical processes of erysipelas and certain forms of puerperal fever, which on this account he designated as "erysipelas malignum puerperale internum," and clinically we certainly fear one as the possible cause of the other, while at the same time it is difficult to say whether all cases cited as proofs of this idea are free from faults. Spencer Wells⁴ mentions some which no doubt speak in favor of the possibility, and certainly the idea should not be lost sight of. In England, phlegmon and erysipelas are considered as the same, but according to the accepted idea in Germany (Fehleisen), they are two entirely distinct processes altogether. In this connection I cite the following interesting though negative case:

At nearly full term, a patient was admitted into this institution suffering from facial erysipelas. Fever accompanying, pains came on, and delivery took place, while the disease was progress-

¹ L. c., p. 249.

² L. c.

³ Virchow: Virchow's Archiv, V. xxiii., p. 425, 1862.

⁴ Spencer Wells: Lond. obst. trans., xvii., p. 267.

ing from the face to the scalp. Here at least we thought everything favorable for puerperal fever, and therefore all necessary precautions were taken to exclude any other possible source of contagion. During the labor the woman was not examined, and the child was born (in occipital position), without the perineum being supported, in fact the genitals were not touched at all, excepting by the patient herself, who, after the placenta had been expressed, washed the parts. The process became well-marked upon the scalp, and it was unavoidable for the patient, after touching the same, not to bring her hands in contact with the vulva; thus certainly everything was favorable for the development of the fever; still, two days later, the temperature fell, and the erysipelas disappeared, with no further disturbances.

Hugenberger¹ states that erysipelas only occurs sporadically in child-bed, and that where it does appear, it has no connection whatever with puerperal fever, and that it is to be looked upon as a disease *sui generis*. Amongst 7,536 lying-in women, it occurred once in every 500; and once among 125 cases of fever.

Duncan² proved statistically that the weekly or monthly occurrence of puerperal fever and erysipelas have no relation to one another, inasmuch as the frequent occurrence of the one is not at all contemporary with that of the other, and on the other hand he could also prove that both sicknesses, separately, occur more frequently in winter than in summer, in this respect a certain similarity existing. As to scarlatina, he found no connection whatever between it and puerperal fever. Practically *we fear* erysipelas in obstetrics, for no physician would, for instance, care to see a member of his family delivered by a physician who had previously been in immediate contact with it. At the bedside, however, erysipelas and puerperal fever are quite different diseases, not alone as to course and prognosis, but also as to treatment. Nobody doubts this, and notwithstanding, in both, we find morphologically the same micrococci, these not only having the same appearance, but also manner of growth, and as Fraenkel noticed, they give similar results with vaccination. I do not mean to say that these organisms are identical, but they resemble each other to such a degree that no means of distinguishing them has yet been discovered.

¹ Hugenberger : *Archiv f. Gyn.*, Vol. xiii., p. 389.

² Duncan : *Edinb. Med. Jr.*, March, 1876

Thus far we have considered scarlatina, erysipelas, diphtheria, and puerperal fever, and have found that the organisms in each, to all appearances, and so far as we know, are the same.

Let us now continue the question, and inquire as to their occurrence elsewhere.

Relations to Pyemia, etc.

Eberth¹ has pointed out the similarity which exists in this respect between pyemia and diphtheria, and says that pyemia is really nothing but diphtheria. B. Hirschfeld² publishes some very interesting experiments made with pus taken from wounds which, although nothing as regards cultivation was done, are particularly interesting, and are described with such clearness and precision that they are well worthy of mention. He found that sometimes the secretions were quite free from micrococci, or at least only contained a few accidental bacteria. In such cases, on the other hand, in which the chain-like organisms were present he could invariably give a bad prognosis as to the future appearance of the wound. The degree of disintegration progressed parallel with the number of micrococci found, and with the disappearance of the latter, the wound would always present a better appearance. In this same manner did the general infection correspond to the number of micrococci found in the blood, and again, if after an amputation it was possible to discover many in the secretions, they were also detected in the blood, and towards the end of the disease, *i.e.*, shortly before death, they had multiplied so rapidly that two or three chains were seen to every blood corpuscle. In some cases of doubtful pyemia, however, he was unable to detect them at all in the blood.

Among animals which he did vaccinate, he found that, if the pus used for the injections was free from chain like micrococci, no death resulted, and likewise if but very few were present, but whenever they were seen in great numbers, the end was almost invariably fatal, and this within sixteen or twenty-four hours.

Similar investigations have been made with lochial dis-

¹ Eberth: *Centralblatt f. med. Wissensch.*, 1873, No. 8.

² Birch-Hirschfeld: *Archiv f. Heilkunde*, 1873, p. 208.

charges, for instance, by Hausmann¹ and Karewsky² who quite frequently found the micrococci in them. Especially when fever is present they are found in greater quantities than when the latter is absent. I at least have found this to be the case, and in the experiments of Braidwood and Vacher,³ Rokitansky,⁴ and others, injections made from patients with fever were always more poisonous than those made from normal cases, although Kehrer⁵ and Karewsky found that normal lochia also produced poisonous effects. The last-mentioned observers also found that the degree of virulence seemed to correspond with the length of time which the patient had been sick. This may partly be explained by the fact that the lochia become more and more concentrated, as it were, as the lying-in period advances, and partly upon difference in reaction and chemical composition which naturally has its influence upon the existence and increase of germs. Mayerhofer⁶ has already pointed out the importance of these factors. It may be mentioned that, although these experiments have been made with great care and painstaking, also those of other observers, such as Hugh Miller,⁷ they are still nowadays not accorded so much importance as formerly, as the micro-organisms occurring here are necessarily subject to such variations and external influences that no real conclusions can be drawn, for the vagina, with its free access of air, is only to be placed upon the same footing as the external parts of the body. Only those micro-organisms, therefore, which are found in deep-seated organs, where external germs can have no influence, are of real value for diagnostic purposes.

I have thus endeavored in the foregoing lines to collect all the facts in connection with our subject which are known to us at the present day. I have shown that in puerperal fever, in erysipelas, in scarlet fever, in diphtheria, and in the secretion of wounds, chain-like micrococci are found which have as yet not been proven to possess individual differences. Their form

¹ Hausmann: Beitr. z. Geb. u. Gyn., vol. iii., p. 412.

² Karewsky: Zeitsch. f. Geb. u. Gyn., vol. vii., p. 345.

³ Braidwood and Vacher: Brit. Med. J., 1882, p. 145.

⁴ Rokitansky: Stricker's Jahrbücher, 1874, p. 161.

⁵ Kehrer: Beitr. z. vergleich. Geburtsk., 1875, No. 4.

⁶ L. c.

⁷ Hugh Miller: Edinb. Med. Journ., 1878, p. 392.

and manner of growth under cultivation must still be considered as identical, but their action upon animals on the other hand has not been constant.

Doleris,¹ however, mentions a chain-like micrococcus three times the size of the ordinary variety, and also asserts to have seen them move, all other investigators having described them as motionless. In a recent conversation with Dr. Fehleisen upon this subject, I was informed that since publishing his paper on erysipelas he had been able to cultivate several varieties, different from those characteristic of erysipelas, inasmuch as vaccination with them upon animals never produced this disease.

Further, as we have mentioned, Koch found a certain species which were peculiar in their constant effect upon mice.

Thus it may not be at a very distant date before we will be able to show that there are different kinds of chain-like micrococci, that each kind has its distinct and specific action, such as we know to be the fact with the tubercle bacillus, etc., but as yet we are ignorant.

Technical difficulties have arisen which I believe will have more or less influence as regards their classification, that is as to size, and we find that heretofore these have been quite insufficiently alluded to. The same micrococcus, for instance, under different circumstances and at different times, will present quite a well-marked difference as to its size, sometimes looking smaller, sometimes larger, and this with the same objective. True optical delusions are sometimes at play. With coloring matters, we find it well marked, as with gentian violet, when they appear larger than if prepared with fuchsin, but the greatest apparent variation appears when we compare a fluid preparation, as, for instance, purulent peritoneal fluid which has been dried over a flame, with a solid, as, for instance, a preparation from the liver, colored with the same material, when we find that in the latter they will invariably seem larger than in the former.

The result of our inquiries, although interesting in a nosological sense, have given us very little material which can be of clinical value, for clinical experience tells us that erysipelas,

¹ L. c., pp. 281, 174, 202.

diphtheria, and pyemia are quite distinct and separate diseases, and certainly at the bedside no one will deny this. The results of pathological investigation being the same does not warrant us in concluding that the processes are the same.

Puerperal fever perhaps, clinically speaking, has a more general relationship with the diseases, for we always fear them more or less as causes or complications.

Some will perhaps argue that micrococci, being found everywhere, are but accidental complications, having nothing whatever to do with the diseases. The discussion of this question I cannot enter into, as it would lead us away from the domain of facts, and we would be obliged to deal more with theoretical arguments. I wish, however, to refer to one pathologico-anatomical fact which shows, aside from all experiments with vaccination, etc., that the presence of these micro-organisms at least cannot be called insignificant—that is, that they have been seen to have caused a necrosis of the surrounding tissues, viz.: Loeffler mentions having seen them in diphtheritical tonsillitis, when, after boring wedges into the tissue, he could distinctly see this action in their tracks. Weigert saw them in small patches upon the coats of the blood-vessels; where they had gained ground, he found that necrosis resulted both in the wall of the vessel and in the surrounding tissue. And, finally, Fleischhauer¹ describes this peculiar action during the formation of abscesses. Sometimes, however, these necrotic processes may be seen to heal, to a certain extent, notwithstanding the micro-organisms still present. Thus Osler² found micrococci in old fibrous changes of the endocardium resulting from ulcerative endocarditis. In such cases, the acute process had given place to a more chronic one, or had even healed.

Hitherto I have discussed but the one variety of micro-organisms observed in puerperal fever, intentionally, that no confusion might arise. Other forms, I said, had been noticed by different observers, and to this category the following two cases belong.

CASE IV.—Elise S., æt. forty, miscarried on the 26th of December, 1883. The placenta was retained and not removed till

¹ Fleischhauer : *Virchow's Archiv*, vol. 65, p. 396.

² Osler : *Lond. Med. Congress*.

one week later, when we did so, by means of the finger, through a narrow cervix. Discharge, at this time, was very offensive, with a temperature of 39° and a pulse of 140. Notwithstanding energetic disinfection of the uterine cavity, the fever did not disappear, and on January 6th, four days after removal of the placenta, a tender, sensitive swelling appeared on the outer side of the upper part of the right thigh. Intumescence increased, and on January 16th, deep fluctuation being made out, an incision was made. Nearly the whole length of the thigh was found to be undermined by an enormous abscess, situated between the peritosteum and soft parts, although in no place was the bone denuded. Microscopical examination of the pus (which was exceedingly offensive) showed myriads of micro-organisms, of different sizes and shapes, both bacteria and micrococci, to be present, among them being a number of the characteristic chain-like form. The cavity was drained, and patient recovered.

CASE V.—Mrs. G., Ipara, was delivered on February 5th, by means of cephalotripsy and an extremely difficult extraction. Labor had been going on for three days previous, and tympania uteri, with high fever, was present. Pelvis was generally contracted, and the os uteri was only about two inches in diameter at the time of operation, which lasted two hours. Ruptured perineum resulted and was stitched. For the first few days, the pulse remained normal, and there was but slight elevation of temperature. On the 8th, however, rigors set in, became more and more frequent, and lasted till death occurred, on the twenty-second day after delivery. No other symptoms present. *Post mortem* revealed abscess of the symphysis pubis and multiple abscesses of the pelvic cellular tissue, without external communication. Peritonitis was not present.

The pus had an exceedingly offensive odor, and examination again gave us enormous quantities of all sorts of micro-organisms. Dr. Jovanovic examined some of the peritoneal fluid, but found here only the chain-like variety, which were also present in the liver, kidneys, and spleen.

Such cases as these have been called ones of mixed infection, and have been explained in various ways by different writers, some considering that all the organisms enter the system simultaneously, others that one variety infecting the body locally prepares the field for others accidentally present, whereas heretofore the healthy tissue was able to withstand their invasion. Can it be that these micro-organisms found in cases of mixed infection are characteristic for puerperal fever? Or rather let us state the question: Have any other micro-organisms, besides the chain-like variety, been found which can be considered as specific?

Ehrlich¹ found in the spleen of a woman who had died of septic pleurisy, extremely small micro-organisms, bordering on the limits of what is visible with the strongest powers. He only saw them in this one case, and characterized them on account of their minuteness, and as to their capability of coloring with scarlet anilin. Further than this nothing has been published. Doleris also discovered several varieties in the cadaver, in the blood and exudations, some also being found during life. According to these, he attempts to distinguish different kinds of puerperal fever, showing, clinically, the different courses. This, however, seems to be rather premature, for it is a matter of fact that as yet we have no means of distinguishing clinically those cases in which the chain-like form is found from those so-called cases of mixed infection, where the others are more frequent. And not only this, but Doleris' observations themselves cannot be warranted as having been free from external influences; for instance, (p. 145) he makes the autopsy of a patient who had been dead for twenty-four hours, and when, as he says, "the abdomen was of a bluish-green color, which, even before being opened, emitted a very foul odor." Now, from this cadaver he makes his experiments, and finds four different kinds of micrococci in the blood, another variety in the meningeal fluid, and still another in the peritoneal fluid. In a second case, he finds no less than seven. These, as can easily be seen, are without doubt the results of decomposition. If we allow a body to lie for forty-eight hours, we can even find maggots in it, and certainly twenty-four hours is sufficient time for changes to have begun; for the skin has already changed color, and the odor itself points out what is going on. Conclusions can certainly not be deduced from such material. Why cannot these processes of decomposition which, after twenty-four hours, are perceptible to our senses, have been present within the first twelve or six hours; or, indeed, why are they not already at work at the time of death? Positive evidence is wanting, but it is highly probable that this is not only the case, but that they are even present before death; for in the above-mentioned case (case II.) we find that the discoloration had taken place over the phlegmonous regions while the patient was still alive. Doleris' observations in them-

¹ Ehrlich : *Charité-Annalen*, 1882, p. 220.

selves were undoubtedly correct; to the deductions only do I take exceptions. The following case which we have observed will be seen to be somewhat similar:

CASE VI.—X., VIpara. Labor in seventh month, which had been going on three days; high degree of stricture of os externum present from previous amputation of the portio vaginalis. Different means of dilatation were tried without result, and finally, fever setting in, embryotomy was performed through an os barely admitting two fingers. Removal of the fetus extremely difficult. Permanent irrigation of the uterus was then made, but, acute septical symptoms appearing, death took place on the fourth day. During these four days, the blood was examined, approximately, twice daily, but *at no time were micro-organisms found*. Twenty hours afterwards, when the abdomen had begun to take on a greenish hue, a drop taken from the heart showed *four different varieties*, perfectly distinguishable in form, also several forms of bacilli in the peritoneal fluid. The heart, kidneys, and liver, examined by Dr. Jovanovic, *showed only the chain-like form*.

These very likely would have been considered by Doleris as having all been present before death, and the case would no doubt have been classified according to the varieties found, but such conclusions are not allowable, and certainly, before proved, should not be spoken of as facts by those who gain their knowledge only from literature, for Doleris himself states that it is only in the highest degree *probable* that the theory is correct.

The appearance of these different forms of micro-organisms after death might be explained by the decomposing processes which take place post mortem. It is a well-known fact that the larger a body is, the more quickly does decomposition set in, because a larger body retains heat a greater length of time than a small one, and under this condition the germs present in the intestinal canal have better opportunity to invade the surrounding tissue than when the body is small and cools off quickly. Doleris himself mentions micrococci found in the vena cava of a horse sixteen hours after death, as result of post-mortem changes, but at the same time it does not seem to occur to him that this might also be true as regards the human body.

In this connection I should like to say a few words concerning Fraenkel's publication.

According to his idea, there exist two kinds of puerperal fever,

in one of which the lymphatic system is affected, while in the other the blood-vessels alone are invaded by micro-organisms. This again would all be very well were it substantiated by facts, but without such it cannot be accepted. One is very willing to be convinced of the truth, could it be proved such. Much better is it simply to collect facts for the time being, and leave their explanation until a time when we are better fitted to solve them than at present seems to be the case. Several cases might here be interesting, although again negative as regards micro-organisms. We are indebted for the experiments, which were conducted at the Imperial Health Bureau, to Dr. Jovanovic.

CASE VII.—Dorothea B——, æt. twenty-seven, Ipara, was delivered on December 15th by turning and extraction; transverse position, narrow pelvis, and tympanites uteri. After confinement, the temperature gradually became elevated, and in spite of permanent irrigation of the uterus, rigors set in with well-marked tenderness of right parametrium, together with dyspnea and pulmonic râles. On December 22d pleurisy of the right side was discovered, and two days following this the patient died.

Post-mortem showed diphtheritical endometritis with metritis, besides a deep circumscribed ulceration of the anterior wall of the vagina; thrombo-phlebitis of the veins of the right leg and of those of the abdomen; purulent exudations in both pleural cavities, and circumscribed abscesses, together with edema of the lungs; also intumescence of the spleen.

The post-mortem was performed a few hours only after death. Small pieces of the liver, kidneys, spleen, and a thrombus taken from the heart were put in absolute alcohol, but examination of these, although made with the greatest care, failed to detect any micro-organisms.

CASE VIII.—Minna Kr——, æt. twenty-nine, Ipara. Forceps delivery; difficult extraction, ruptured perineum. On the third day peritonitis with fever and vomiting set in, followed by icterus, pains in the chest, and dyspnea, with rapid depreciation of general strength. Shortly before death, which took place on the ninth day, a swelling of the parotid of the left side appeared. In this case also, permanent uterine irrigation was employed.

Post-mortem, performed a few hours after death, showed deep laceration of the left fornix vaginae, diphtheritical endometritis, abscesses in the broad ligament, diffuse suppurative peritonitis, and intumescence of the spleen. Fresh adhesive pleurisy, infarctions of the right lung; edema pulmonis and chronic interstitia, nephritis.

Here, portions of the kidneys, spleen, and liver were examined, but likewise showed no micro-organisms.

CASE IX.—Dr. Colpe, assistant of Professor Credé, in Leipsic,

sent me, with the kind permission of my former teacher, specimens of blood, and of the liver, spleen, and kidneys from a patient who had died of puerperal fever. There had been frequent rigors up to the time of death, a fortnight after delivery. Neither in the blood nor in any of these organs could anything be detected.

Not that in any of these cases the micrococci were not present, for that might have been true in all, but simply they could not be detected. Perhaps had the exudations, or the local pathological changes been looked into, the result might have been more successful. It has been proved that there are some micro-organisms which, without entering the body at all, can cause death, merely by their local action, their toxic matters being sent into the tissues and secondarily causing changes and exudations without they themselves taking part at all. Loeffler showed this on guinea pigs, which he inoculated with bacteria collected from diphtheritical membranes. Why cannot similar processes occur in puerperal fever?

I have thus arrived at the end of my subject, and before closing, it would perhaps be useful should I sum up in as few words as possible the conclusions to which we are led and the deductions which we may draw from our inquiries and investigations.

1. Of all micro-organisms found in puerperal fever, the chain-like micrococci seem to be those to which we should especially direct our attention, and to which we should attach the greatest importance.

2. When in any case of puerperal fever their presence has been detected in the exudations, they have also been found in the deeper organs.

3. They have been found in erysipelas, scarlet fever, diphtheria, and puerperal fever, and in each possess the same form, and show the same disposition towards fertilizing fluids and coloring matters.

4. Although it is very probable that different varieties do exist among these diseases, we as yet have no positive proof of the fact.

5. A differentiation according to size is an extremely difficult, perhaps hopeless task, but according to manner of growth it may be possible.

6. Vaccinations with cultivations of these micrococci from

different diseases has proved fatal to animals, but has given no typical or characteristic results.

7. Chain-like micrococci have also been found in infected wounds, and in the blood of pyemic patients.

8. The pathologico-anatomical investigations thus show that these clinically related diseases (puerperal fever, erysipelas, diphtheria, scarlet fever, and pyemia) possess similar micro-organisms.

9. Besides the chain-like form, other micro-organisms may be present in puerperal fever (*i. e.*, mixed infection).

10. The presence of these latter in the cadaver does not always prove that they existed in the living body; on the contrary, they are often the result of post-mortem decomposition.

11. It is probable that the processes of decomposition are sometimes present before death actually takes place, different varieties of micro-organisms therefore found, for instance, during the death-struggle may have nothing to do with the cause of the disease.

12. It is as yet impossible to classify puerperal fever, as regards course and prognosis, according to the varieties of micro-organisms found (Doleris), or according to their mode of invasion (Fraenkel).

13. In some cases, no micro-organisms have been found, but this does not prove that they did not exist.

In endeavoring to consider facts only, and leave aside as much of their explanation as possible, I thought the best general survey could be obtained of our present knowledge of the relations between micro-organisms and puerperal fever. Simultaneously I wished to attract attention to the difficulties which have still to be overcome before the question can be considered settled. I trust at the same time that these obstacles will not deter those who perhaps have opportunity, from the task of trying to overcome them. Should this paper give any impulse whatever to further inquiries and investigations, the work spent upon it and upon some of the experiments connected with it will certainly not be considered fruitless.

In conclusion I desire to express my thanks to Dr. John Vander Poel, of New York, at present in Berlin, for his assistance in rendering this paper into English.

QUESTIONS RELATIVE TO PUERPERAL FEVER.

BY

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IN his classical work on "Puerperal Diseases," Dr. Fordyce Barker remarks: "Very much more has been written on this" (puerperal fever) "than on any other disease. I find that more than twenty thousand pages have been published on this subject within the last twenty years, and a complete bibliographical catalogue of all that has been written on puerperal fever would fill many pages of an octavo volume." Since 1874—the date of the publication of these words—still further additions to the literature of the subject have been made, in monographs, papers, in medical journals, chapters in text-books on midwifery, and discussions at meetings of societies; in 1875, the subject was brought before the Obstetrical Society of London by Mr. Spencer Wells, and the debate extending over four meetings is published *in extenso* in the xvii. vol. of its transactions, and finally it was again brought prominently forward in the Obstetrical Section of the Academy of Medicine in Ireland and, in the same section of the British Medical Association in 1883.

One would naturally suppose that a subject which had received so much attention, that had been examined into, reported on, and discussed to so great extent, by all sorts and conditions of men, was thoroughly exhausted, its nature ascertained, its treatment defined, its causation definitely settled, and its place among diseases fixed. Yet one arises from the perusal of its literature with no fixed ideas about it, but only with the conviction that we are yet very much in the dark, very far from knowing what puerperal fever really is.

This being so, I have not the presumption to suppose that anything I could write would settle what so many men, leading authorities in medicine and surgery, have differed about; nor do I propose to lay down the law or dogmatize at all; but, having met in my practice cases of what we call puerperal fever, which seem to me not to come within the various theories put

forward, I propose to ask a few questions which I trust may be answered by abler men.

For it seems to me, if we are to arrive at the truth, if we can hope to come at some definite knowledge of the subject, if we expect to obtain a clear comprehension of the causation, prevention, and treatment, of that assemblage of symptoms which have been called puerperal fever, we must lay aside preconceived ideas and theories, and, not allowing ourselves to be carried away by the beautiful simplicity of any one doctrine, we must collect facts, carefully analyze each case; and remembering that we have to deal, not with fixed and immutable rules, nor inorganic bodies whose changing is slow and regular, but with living structures, ever varying with age and condition, no two of which are exactly alike; we must not take it for granted that, because in ordinary circumstances a given cause usually produces the same result, it must be invariably so. But we must examine carefully whether such causes may not, under different circumstances, be capable of producing other and unexpected results.

That a fever with tolerably definite symptoms, commencing with a chill or rigor, usually attended with pain in the abdomen and vomiting, frequently with perspiration, with affection of the lungs and kidneys, exacerbations of temperature, and often terminating fatally, attacks puerperal women we know. But the point of difference seems to be whether this morbid state is septicemia or one of the zymotic or infective diseases, the ordinary symptoms of which are masked or changed by the puerperal condition, or a specific fever "peculiar to puerperal women" produced, or "the determining causes of which," to use Dr. Barker's words, "may be either epidemic influences, contagious infection, or probably nosocomial influence," capable of being conveyed by infection to other puerperæ; or have we a fever peculiar to the puerperal state, distinctive in its course, which may be produced by varying and different determining causes, and which is capable of spreading by infection or contagion?

On these points, authorities differ.

That puerperal fever is septicemia has many advocates. Thus Mann¹ observes: "The opinion most generally expressed, and

¹Mann: Report on Obstetrics, AM. JOUR. OF OBSTETR., vol. x.

now held almost to the exclusion of any other, is that puerperal fever is caused by septicemia, autogenetic or communicated.

"The whole tendency of recent investigation is daily rendering it more and more certain that obstetricians have been led into error by the special virulence and intensity of the disease, and that they have erroneously considered it to be something special to the puerperal state, instead of recognizing in it a form of septic disease practically identical with that which is familiar to surgeons under the name of pyemia or septicemia."

No one at present denies the existence, nay more, the prevalency of septicemia in puerperal women; indeed, it is generally admitted that probably the majority of cases in which fever arises in the post-partum state are due to septic infection.

But no mean authorities hold that the septicemic theory does not cover the entire ground.

Playfair,¹ who, in his standard work on midwifery, excludes the term puerperal fever altogether and includes post-partum fevers under the head of puerperal septicemia, is constrained to admit that "there are many facts connected with heterogenetic infection which are very difficult to reconcile with theory, and of which it must be admitted we are not yet able to give a satisfactory explanation."²

Matthews Duncan³ states: "It is not uncommon nowadays to confine the name of puerperal fever to septicemia or pyemia occurring after delivery. It appears to me, however, to be better, on mere theoretical grounds, to include all the ordinary diseases of the lying-in chamber which are accompanied by fever, and which may be fatal. In practice, we cannot maintain the limitation to septicemia and pyemia, because in many cases we cannot be sure of our diagnosis, and because a case may begin as one disease and be complicated with another as it advances."

"In addition to septicemia and pyemia which are the result of the growth in the blood of certain micrococci having the power of rapidly multiplying, we have sapremia or mere poisoning by the chemical products of putrefaction, and we have simple or traumatic inflammatory fever."

The theory that septicemia is the cause of, or that it is in reality what is commonly called puerperal fever, is, no doubt, a

¹ Playfair : *Midwifery*; 4th edition, 1882, p. 328.

² Loc. cit., p. 335.

³ Dr. Matthews Duncan : *Puerperal Fever*; *Lancet*, 23d Oct., 1880.

most attractive one. It brings post-partum fevers into harmony with surgical fever and the germ theory of the day ; but whether it is true, or not, depends on what meaning is attached to the the word septicemia. Some evidently include not only a contagium vivum, but vitiation of the blood by products of decomposition. Others limit it to the former alone, and finally, to judge from the interchangeableness of the expressions used, some, too, seem to include blood-poisoning from any infection—whether strictly septic, sapric, or from zymotic disease.

If the latter was what was meant by septicemia, no doubt the the term would be absolutely correct when applied to puerperal fevers ; but if limited, as generally understood, to apply to fever resulting from inoculation with septic virus, or absorption of the products of decomposition, then it appears to me that the theory is not proven.

The division of puerperal septicemia into autogenetic and heterogenetic is practical and fairly good, though the distinction in practice is not always easy.

I find it laid down that the sources of autogenetic infection are decomposition of the tissues of the mother, of the matters retained in either uterus or vagina, or of a putrid fetus.

The channels through which the poison may be absorbed are given as the endometrium, the placental site, or some abrasion or wound of the genital tract.

From this, auto-infection would appear to be synonymous with what Matthews Duncan describes as sapremia. That the resemblance of the interior of the uterus to a stump after amputation, or an open wound, is more fanciful than real has been shown. It would be quite inconceivable to suppose that during delivery the entire mucous membrane (decidua) was torn off, leaving the muscular tissue bare ; whether, as maintained by Robin and Priestly, a new mucous membrane is formed under the decidua, after the fourth month of pregnancy, or, as Duncan and Spiegelberg, a superficial layer of decidua is expelled, and a deeper layer remains behind ; in either case, the surface of the uterus is covered, and therefore the channels for septic absorption appear to be chiefly confined to the placental site and wounds of the genital tract. And if Robin is correct in believing that even here (placental site) only a thin layer of the decidua serotina is thrown off, the loci for absorption would be still further limited.

It is not difficult to understand how auto-infection can take

place in the case of a putrid fetus, even prior to labor, and such cases have been recorded, but on the septic theory, how are we to account for cases in which, although the child is alive and afterwards born alive and healthy, symptoms of puerperal fever develop before delivery? In such there is no decomposition of either fetus or uterine contents, the mother has not been exposed to any zymotic disease, there has been no laceration or abrasion of the maternal tissue, the placenta has not been separated, its site cannot be a focus for infection, there are no open vessels through which septic or sapric matter can pass into the circulation.

Playfair suggests that the infection may pass through the unbroken mucous membrane, and so such cases may be accounted for, but this would apply more to heterogenetic cases, since for the poison to pass through the mucous membrane it must first get there, and in auto-infection I fail to see any uterine source but the products of decomposition. If the poison is imported from without, then the case ceases to be one of auto-genesis.

The existence of a specific puerperal fever having a period of incubation, has been denied. Dr. Atthill¹ states that it is "admitted by all writers of any weight that there is no such disease as puerperal fever, properly so called." I am not aware that Dr. Fordyce Barker has withdrawn his distinct statement to the contrary in his work on puerperal diseases, or in his lucid and argumentative speech at the London Obstetrical Society. I think Dr. More Madden² holds the same views when he says "provided we recognize (what some recent authorities have emphatically denied) that there is a specific infectious disease consequent on parturition." Which view is correct I do not presume to decide, but I do know that I have met in my own practice more than one case in which the disease commenced before labor, in which every possible source of contagion was carefully excluded, and this was the more easy as the patients resided in the country.

In such cases, a cause must obviously be sought outside the uterine system.

It has been sought to include under the head of autogenesis, post-partum fever arising in unmarried primiparæ. That it

¹Dr. Atthill: *Metria*. Trans. Irish Academy of Medicine, vol. i., p. 130.

²Dr. T. More Madden, meeting Brit. Med. Assoc., 1883.

unfortunately deserves the term enormous given to it by Dr. Atthill is too true. Its frequency is attested by the records of lying-in hospitals and the experience of obstetrical practitioners. Dr. Atthill endeavors to include these cases under the head of septicemia:

“When self-infection occurs under ordinary circumstances, we believe it to have been produced as the result of the decomposition of clots or of portions of the membranes or placenta, which have been retained in utero. But it is irrational to suppose that women the subjects of mental distress are more liable to the occurrence of these accidents than other primiparæ. And I think therefore that we must come to the conclusion that the remorse and anxiety to which these poor women are a prey, not alone renders them more susceptible of the action of infection, but that it interferes with that recuperative process which should take place rapidly in the uterus after parturition. The muscular fibres of that organ do not contract as they should, the blood supply consequently is not cut off. The mouths of the sinuses remain open; the denuded placental site, instead of becoming rapidly restored to its normal condition, becomes unhealthy, and the fetid discharge, which under these circumstances takes the place of the normal lochia, enters the system either directly through the open mouths of the placental sinuses, or is absorbed at the site of some fissure in the mucous membrane lining the genital tract.”¹

Now if this sequence of events took place, we should in the first place have post-partum hemorrhage. I cannot conceive how under such circumstances it could be avoided: “the muscular fibres do not contract as they should; the blood supply consequently is not cut off.” If the sinuses are open, would not the poison be rapidly absorbed and the symptoms quickly developed? Yet in these cases, we are told, that a couple of days after delivery the patient is reported fretting, the sole symptom of anything wrong, “a quick pulse.” After twenty-four hours more, a rise in temperature and “soon the regular phenomena which accompany puerperal septic fever.”

If these cases were truly septicemic depending on the sequence of events so graphically described, then many, if not most, of them should be far more rapid in development and termination.

In no way is septic virus so rapid in its effects as when intro-

¹ Atthill, *op. cit.*, p. 133.

duced directly into the blood. If the sinuses are open in an uncontracted uterus, if the tissues or discharges are decomposed or unhealthy, they would pass or be sucked into the circulation directly, and we should have many cases of septicémie foudroyante. I do not deny that such cases occur, I do not deny that auto-infection occurs in unmarried puerpera; but I question if this explanation accounts for the frequency of puerperal fever amongst that class, and I think it doubtful if they suffer from septicemia, in any very marked degree, more than other primiparæ. I believe, in most of these cases, that the mental condition and nerve influence are the originating cause, not by producing the condition of the uterus described by Dr. Atthill, but by interfering with elimination and causing the retention on the system of effete material which ought to be cast out, or, by promoting actual change in the condition or vital properties of the blood, a vitiation, facilitated by the pregnant or puerperal state. "Taking into consideration the condition of the blood of a puerperal woman, its condition verging on fever—is it not highly probable that mental emotion may produce a fever indistinguishable from puerperal fever?"¹

Dr. Atthill himself produces important evidence, in the valuable paper I have quoted from, as to the production of puerperal fever quite independent of septic poisoning, when he points out "that, if a woman who is in labor gets a chill while walking to the hospital, as the poor commonly do, even in the most inclement weather, and that, as a result, she is attacked with bronchitis or pneumonia, she will, if the attack does not subside in a few days, almost certainly exhibit those symptoms commonly observed in patients suffering from puerperal septic fever." He, it is true, gives this as an instance of the effect which the puerperal condition has on disease; to me it appears, on the contrary, strong evidence of how a puerperal fever may arise from interference with elimination, or how, in the peculiar condition of the blood, resulting, on the one hand, from the process of lactation and, on the other, from that of involution, a chill, with or without subsequent bronchitis, may give rise to a fever indistinguishable from septicemia.

In my own practice, I had a case which progressed favorably for seven days after delivery; uterus well contracted, lochia inodorous, temperature and pulse normal. The patient was

¹ *Obstet. Trans.*, vol. xvii.

accidentally wet, got a chill, and a most severe puerperal fever developed, the lungs, peritoneum, kidneys, and pelvic cellular tissue being successively attacked.

The condition of the bowels, too, is an important factor. Women suffer largely from constipation, a condition not improved by pregnancy. I have often wondered where or how the quantity of feculent matter could be stowed away. I have seen two cases at least where I could find no other solution of the cause of the fever, all other sources being got rid of by a process of careful exclusion and examination.

I attended Mrs. G. in her second confinement. She had two or three slight pains before I was sent for. I arrived in about an hour and found the child had just been born.

The placenta and membranes came away in a few minutes. I administered a dose of Long's solution of ergot, and did not remove my hand from the abdomen for over half an hour, until I had ascertained that a firm and permanent contraction of the uterus had taken place; an hour afterwards, just before leaving the house, I satisfied myself that the uterus continued well contracted and the discharge normal. Next morning, I found my patient doing well, uterus contracted, discharge perfectly free from odor, no tearing or laceration of the perineum or orifice of the vagina, and I was not surprised at this, as I had experience of the amplitude of the orifice of the vagina and the elasticity of the tissues in her first confinement. On inquiring as to function of bowels and bladder, I was informed that she had taken a dose of oil on the morning preceding her confinement, and that she had had six stools. "So I shan't need medicine, doctor," she remarked. The nurse confirmed the report. At my evening visit she looked flushed; pulse somewhat accelerated, lochia normal in quantity and free from smell (the vagina had been twice syringed out with Condyl's fluid and water). She expressed herself feeling well, but a little warm. Two hours after, I was sent for and found her in great pain; abdomen tender to pressure, pulse over 100, temperature 104, face crimson; patient restless.

I ordered a turpentine stupe to abdomen, gave ten grains of calomel, and administered a morphine hypodermic. The bowels moved five times; the nurse described the discharge as enormous, the first three motions being solid, the last two partly formed, partly liquid. By morning the patient was free from pain and tenderness, pulse fallen to 72, and temperature to normal level, and her progress to convalescence was uninterrupted. I am confident that, had I rested satisfied with the report as to the thorough cleansing out of the bowels and treated the patient for septicemia, or trusted to opiates, quinine, and nourishment, the case would have run on into puerperal fever.

In another case, to be recorded further on, after most careful

investigation, I could find no other cause but accumulation of feces. It appears to me that autogenetic cases, independent of septic or sapric infection through the generative tract, can occur; "that there is very cogent collateral evidence towards proof that the blood of a puerperal patient may be infected by other channels than by the genital canal. It is certain that puerperal fever may begin in a patient before her delivery."¹

Before assenting to the theory that all cases of fever, in the puerperal woman, indistinguishable in their symptoms from or resembling septic fever, and in which no infection can be traced, are the product of septic absorption, further investigation and the recording of carefully observed cases are needed.

The production of heterogenetic cases opens a wider field for speculation and theory.

Are they all, though showing almost identical symptoms, merely so many specific diseases, the distinctive features of each disease masked or altered by the puerperal state? Are most of them true septicemia, or is there a peculiar fever produced by various and different poisons? Or have we a specific puerperal fever? Finally the question of its contagiousness comes in: is it only inoculable or is it infectious, capable of being caught by others, and if so, to what extent?

At the outset it must be admitted that septic infection can be directly conveyed by instruments, sponges, the hands, and probably the clothes, and in the wards of a hospital by the atmosphere.

Two questions, however, naturally arise with regard to this inoculation with septic virus:

- (a) Does it always take place through a wound, or
- (b) Can it occur through the unbroken mucous membrane?

That septic infection most readily results from deposit of septic virus in a fresh wound must be admitted, yet that it can follow deposit of the virus on unbroken mucous surfaces can hardly be denied. The question is surrounded with difficulty. For those cases in which there has been no direct apposition of an infected instrument, finger, or sponge to a wounded surface, may be explained by contagion having previously been deposited in the vagina or vulva, or, that by the passage of the child, the mucous membrane was deprived in parts of its epithelial covering.

¹ Priestly: *Obstet. Trans.*, vol. xviii., p. 47.

Thus it would at first sight appear that, where septic poisoning followed a natural labor, in which instruments were not needed, and where clean sponges, etc., were used, but in which contact between the surgeon and some source of septic poison existed; for instance, a post-mortem examination, the presumption that the poison must have been absorbed through the mucous membrane, can be explained away. For although lacerations of the cervix and abrasions of its tissue only occur when the head is passing through, that is, after it had ceased to be felt by the examining finger; and similarly, lacerations of ostium vaginae and perineum take place after examinations have ceased, and, in most cases (except considerable laceration of the perineum), the abrasions are not touched at all. Yet the virus deposited in the vagina by the preceding examination may poison the fresh wounds, or may infect an abrasion when the child is passing through. But cases now and then occur which appear to be produced by direct transmission through the mucous membrane, and there does not appear to be any reason why they should not, when the virus of other diseases, such as syphilis, gonorrhea, small-pox, and diphtheria, is capable of being thus transmitted.

Another question needing solution is, Can septic poison infect in any other way than through the genital tract?

"It is the opinion of most of the recent writers, and it is mine," says Dr. Atthill, "that septicemia occurring in a puerperal woman is not capable of being conveyed to another puerperal woman by any means other than the direct transfer of the infectious matter to some portion of the mucous membrane lining the genital track."

While it is admitted that infection would more readily be absorbed by wounds or abrasions, or even by the mucous membrane of the genital tract after the rough usage it had received during the process of delivery, I see no reason why infection cannot be conveyed through other channels.

It is the condition of the blood and entire system of a puerperal woman which renders her so susceptible to septic poison, and, by whatever channel it is introduced, the same results will follow.

If infection could only take place through the generative tract, then we should find that it ought to be comparatively easy in Maternities to prevent the disease spreading—isolation of the

infected patient, not allowing other patients to be attended by the same doctor or nurse, the prohibiting the use of any articles with others, which had been employed with her, ought to prevent the spread of the disease.

Then we would also expect, in those cases in which the dose of the poison was not large or virulent enough to cause death prior to the development of appreciable lesions, that the local manifestations of the disease would be greatest and nearest to the place of infection, and spreading from thence to the neighboring structure. Although undoubtedly this is often the case, we find that sometimes the pelvic organs escape or are only secondarily engaged. But when they are the seat of apparently a primary attack, it does not follow that infection was conveyed through the genital tract, as, owing to the post-partum condition, they would more readily succumb to inflammation, no matter through what channel septic poison was introduced; and we would expect to find that septic poisoning should more readily occur where there had been greater interference and handling of the genital tract, yet, after severe obstetric operations, septicemia is rare.

We know that septic poison increases in virulence as it is reproduced, and becomes more infective, and, in this light, the observations that autogenetic cases do not appear to possess active powers of propagation are interesting, and it appears probably that, as the disease gains in virulence, it can be transmitted through other channels besides the genital tract.

If it be true that micro-organisms are the cause of septic and zymotic disease, then I fail to see why the action of some should be limited to one portion of the body.

But that the emanations from a woman suffering from puerperal septicemia can infect through the respiratory tract, I have no doubt.

Some years ago, I was in constant attendance on a case of puerperal septicemia. In it, as is usual, the odor arising from the body was plainly perceptible. After a time I became unwell, lost appetite, and could not lie down without breaking out into profuse perspiration, then my throat became inflamed, and I could not go near the patient without feeling the emanations from her body in my throat, and entirely losing my voice for half an hour afterwards. Loss of color and debility followed. It seems to me that the miasm which, in me, produced these

symptoms, in a post-partum woman would have caused puerperal fever.

The experience of the dissecting-room furnishes stronger proof of the possibility of septic infection through other channels. It is not uncommon to find students laid up with attacks of diarrhea with or without fever, or of fever with or without diarrhea, who have neither cut themselves nor have been inoculated with septic virus. I have seen these cases occur in men who have not touched a body, but merely sat by reading to a comrade who did the dissection.

Again, I do not think that a local inoculation can explain those cases that are recorded of puerperal fever dogging the steps of individuals, it may do so where the practitioner passes directly from a diseased patient to a healthy one; but where he takes precautions, uses disinfectants, and where two or three days intervene, then it is pushing the local infective doctrine too far to put it down as the cause of the resulting fever.

The local doctrine leads to this difficulty. We have dozens of cases recorded where the doctor, having attended a case of puerperal fever or erysipelas, attends a woman in her confinement, then another, and so on, and each woman is attacked by puerperal fever or septicemia, and it is asserted that the infection has been deposited by him in the genital tract. But the same man attends and examines during the same period of time other women who are not pregnant; in them, neither septicemia or erysipelas appears. Now, although admitting the greater susceptibility of the puerperal woman, yet the exemption of the non-puerperal is too general to be accounted for on this ground. The absorbing power of the genital tract is quite as great in the non-puerperal; and even when wounds or abrasions exist, an attack of septicemia is quite exceptional; consequently the obstetrician who feels bound to withdraw for a time from midwifery cases does not feel called on, although he would abstain from major operations, to give up his gynecological practice.

Dr. Barnes has shown clearly how the infection can be conveyed through other channels, though, I think, he was wrong to limit the source to the breath. Infection can be conveyed through the clothes and skin. He has pointed out that craniotomy and post-mortems produce diarrhea and foul breath capa-

ble of infecting another person. I have experienced the same. I attended a lady suffering from, and who eventually died of, fever—apparently a combination of typhus and typhoid. The smell of the fever was overpowering. So much so, that it could be perceived before entering the room. During the progress of the case, retention of urine occurred, and it became my duty to empty the bladder. On raising the bed-clothes to introduce the catheter, the effluvia was so great that the usual result was that I was obliged to vomit before I could leave the room. I took every precaution to avoid carrying the infection about, yet at various times of the day I was conscious of the smell. After thorough bathing and entire change of clothes, whenever I got heated and perspired, the smell would come back; on cooling, it would pass away, only to come back again on taking exercise and getting warm.

I have little doubt but that if I had attended a case of labor, I should have infected my patient, not through the genital tract, but through the lungs, and I think the explanation of the comparative immunity of the non-puerperal is that in gynecological cases, the time spent with the patient is usually very short, while on the contrary, our attendance in a puerperal case is generally long enough to enable a sufficient dose of the poison to be inhaled.

On the other hand, it may be urged that the results of antiseptic midwifery indicate a local infection. That the reduction of temperature and the marked benefit which follows, in many cases, intrauterine injection of antiseptic solutions demonstrates a local origin is quite true in some cases. I admit, as proved, that puerperal septicemia frequently originates from local infection, but I deny that it always does so. It seems to me that the good effects of intrauterine injections are principally shown where there is decomposing matter in the uterus or vagina, that is, in those cases that Matthews Duncan names sapric, and that in genuine septicemia it is not at all so clear that benefit follows, nay, more, that injury does not result.

Nor does it appear clear that, what has been cited on the same side, contagion from post-mortem examinations establishes the case.

Dr. Routh quotes the epidemic of 1846-47 in the Vienna Hospital as having been caused by the examinations made by students who came from the dead-house and coroner's courts.

The method adopted seems to have proved that at that time this was so. Yet prior to this epidemic, the students acted in precisely the same manner, and the effects do not seem to have been so disastrous. We also find that men in general practice, carrying on a large midwifery practice, frequently make post-mortems, yet seldom convey infection. Is it not probable that where it is carried it may be conveyed through and by other means than the fingers, and that the absorption or conveyance of the poison by the dissector depends on the body he dissects, atmospheric and telluric influences, so that in one case infection adheres to the hands, and is deposited in the genital tract; in another, it is absorbed and afterwards given off in the breath and perspiration, and inhaled by the lungs of the parturient.

Dr. Barnes, in eloquent and forcible language, pointed out that pregnancy is a grand experiment testing the soundness of the organization, producing such alterations of function and structure, such development of various organs, and such increased activity of the entire system, that we can learn therefrom most instructive lessons in physiology and pathology. The relation of puerperal fever to the zymotic diseases carries the lesson still further, and instructs us in etiology.

It is admitted that a puerperal woman (some even assert that a pregnant woman) is exceedingly sensitive to the contagion of the zymotic diseases, and that, having been exposed to the contagion, a fever is set up differing in symptoms and course from the original zymotic disease, yet indistinguishable from what is called puerperal fever. The question immediately arises, what is this fever?

Is it the zymotic disease, its symptoms masked and its course changed by the post-partum condition, or is it a fever varying from the zymotic, although produced by its infection or contagion, only affecting women recently, or about to be delivered, inducing a train of symptoms which differ *toto* from the original zymotic, and which is truly and really a puerperal fever? On these points opinions differ. Dr. Atthill¹ clearly states that held by many.

"I believe that these attacks of so-called puerperal fever, occurring simultaneously with epidemics of so-called zymotic disease, are nothing more than cases of that zymotic disease affecting a puerperal woman, the disease being modified and its

¹ Atthill, op. cit.

course altered in consequence of its occurring in a patient in whom a particular state of the system exists, the result of utero-gestation, and in whom the state of the blood is modified from the same causes, as well as being after parturition loaded with the effete material, the result of the metamorphosis which then takes place in the uterus."

This statement is based on the general opinion that zymotic diseases always breed true, that is, that the poison, or infection, or contagion, whether it be miasm or micro-organism of any given zymotic disease, is a specific poison, always acting in the same way, and invariably reproducing the same disease; that the virus of erysipelas, of small-pox, of scarlatina, of typhus, etc., will and can only and always produce erysipelas, small-pox, scarlatina, and typhus, and further, that the infection of any one of them cannot produce any other zymotic fever or any other disease whatsoever.

After labor, in all women, the condition of the system is pretty much the same. The high pressure; the nervous and vascular changes of utero-gestation; the effects produced by the expenditure of nervous and muscular force, with loss of blood during delivery; the metamorphosis of the genital structures; the circulation loaded with effete material; and the process of lactation setting in, I say these conditions are in all women pretty much the same, although to some extent modified by pre-existing disease, length and difficulty of labor, and loss of blood.

We have therefore a given specific poison acting on a given condition, and we should therefore have identical results, and in all cases the special action of the zymotic poison should produce the specific zymotic disease or be so altered or masked or overpowered by the puerperal condition as apparently to lose its specific power, and be able only to produce a non-specific febrile disturbance indistinguishable from puerperal septicemia. Thus, Mr. Spencer Wells says: "If you have a specific morbid poison, it must under all conditions produce identity of results."

Yet we have the undoubted fact shown, that apparently from different zymotic poisons springs one and the same disease, which, whether it results from erysipelas, scarlatina, small-pox, or typhus, has the same symptoms during life and similar anatomical changes after death.

Nor is this all, for if the zymotic infection invariably was followed by this result, the argument would have more weight. But what is the fact? We have evidence beyond all doubt that specific zymotic poison will, in a puerperal woman, produce the specific zymotic disease from which it springs. It, therefore, seems to follow, either that, in a given condition, the zymotic poisons do not breed true, or else that the cases of puerperal septicemia, apparently springing from zymotic infection, are not that zymotic disease with its symptoms modified or course changed, but a disease peculiar to the puerperal state, and which may rightly be called, however produced, puerperal fever.

From my own experience, and from medical literature, I find that, in the case of women exposed to the zymotic infection, some do not suffer at all, some are attacked with puerperal fever, and others contract the zymotic disease.

I have seen puerperal patients run through an ordinary attack of scarlatina, its course and symptoms unaffected by the post-partum condition. During an epidemic of small-pox in Galway in 1878 or 1879, a woman in the sixth or seventh month of pregnancy was admitted to hospital, suffering from small-pox. She aborted. The small-pox ran its regular course, unaffected by the delivery. About the same time I was called to see a lady the day after her delivery, and found the variolous eruption coming out. I attended her till she died. From first to last there was not a single symptom of puerperal fever, nor was the course of the disease different from what I had observed confluent small-pox to run in the unimpregnated, nor did it appear to be in the least modified by the puerperal condition.

In the sixth volume of the "Transactions of the American Gynecological Society," a case of erysipelas, running its ordinary course without any puerperal complication, is recorded by Dr. H. F. Campbell.

On the other hand, during the epidemic already referred to, I attended a lady whose husband died a short time before of virulent small-pox, and, although she had attended him and was confined in the same house, her recovery after delivery was rapid and perfectly normal. The case of Dr. Cordes,¹ of Geneva, is very remarkable, as, while the infant was born with scarlatina (evidently contracted through the mother while nursing her children suffering from that disease) she not only escaped con-

¹ *Obstet. Trans.*, vol. xvii., p. 218.

tracting it herself, but without "being taken with any kind of eruption, or erysipelas, or puerperal fever, or anything." Numerous cases of exposure to infection without contracting any disease are recorded.

Undoubtedly, however, the evidence showing a connection, in a large class of cases, between zymotic disease and that assemblage of symptoms called puerperal fever is too strong to be denied, and the question then is, How is it that, in some cases there is an entire escape from illness, in others a zymotic disease, and in others that puerperal fever results?

In all epidemics we find certain cases in which, either from the largeness or virulence of the dose of the poison, the patient dies rapidly, struck down at once before the disease has time to develop its symptoms; in others, the attack is slight, and were it not for the presence of an epidemic, we should have some difficulty in classifying the disease, and both these conditions occur in the puerperal state, and the woman may die rapidly with symptoms undefined beyond rigors, rise of temperature, acceleration of pulse, and more or less stupor, a condition likely to be mistaken for septic poisoning.

If the dose of the poison is, on the contrary, small, although sufficient in a healthy person to produce more or less *malaise* and some feverishness, but which would be thrown off by the system without developing into a specific fever, in the puerperal woman it would be quite sufficient to check elimination and to cause accumulation of effete material in the system. "The proclivity to be affected by prevailing febrile diseases, or to initiate them, is, no doubt, due to the peculiar constitution of the pregnant and puerperal woman, described at length by Dr. Richardson and Dr. Arthur Farre. The excess of fibrin in the blood, the diminution of salts exists even before parturition, afterwards the sudden arrest of the utero-placental circulation, and the consequent effect on the maternal blood-current, with the later irritation of the milk process, and the involution of the uterus, furnish, as it were, so much combustible material, all constitute so many disturbing forces, which, added to the susceptibility of the nervous system, must render even healthy women sensitive to febrile disturbance, and favor the development of rapid changes in the composition of the blood, if only a small leaven of ferment in any form be added to it."¹ In

¹ Priestly : *Obstet. Trans.*, vol. xviii.

this way the production of a certain number of cases of puerperal fever may be accounted for, but a wider question by far is opened, and that is: Is it true that a zymotic disease in all cases reproduces itself and nothing else? If this question can be answered in the negative, then we shall be nearer the true explanation of some of the causes of puerperal fever.

Researches into the germ theory of disease would seem to support this view. Thus it is stated that micro-organisms of one group may, by development, change into one of another group—a micrococcus developing into a bacterium, this into a bacillus, and this again into a spirillum. Buchner believed that, by changing the nutritive soil, the micro-organisms could be altered. His experiments are looked on as uncertain, because he used in some of them blood; but blood could hardly be a more impure cultivator than the fluids of a puerperal woman. According to Wernich, an innocent bacillus may develop in a typhoid one, and it appears probable that it is the condition of the virus which determines the form of the disease set up by the micro-organism.

The entire question of the relations of micro-organisms to disease is not yet sufficiently worked out to permit of definite conclusions being drawn, and not professing to be a skilled mycologist, I prefer to put forward proofs from observation on disease.

In ordinary surgical practice, and especially in hospitals, we find erysipelas, diphtheria, and scarlatina will give rise to pyæmia. "Whenever," says Mr. Solly, "a case of surgery in a house takes on a highly phlegmonous appearance, I am always sure to find break out in the inmates of the house either erysipelas or scarlatina.

"It is admitted that diphtheria occurs after scarlatina, after measles, in some epidemics of typhoid, after erysipelas, after the inhalation of sewer gas. What have all these conditions in common that they should each and all be liable to be grafted into diphtheria?"¹

Dr. Henoch, of Berlin, asserts that, in children, scarlatina seems often to result from an operation, and has been observed where the possibility of infection was out of the question.

In the *British Medical Journal*, January 4th, 1879, Dr. Braxton Hicks reports the case of his son, seized at school with

¹ British Med. Journ., May 24th, 1879.

"a very severe attack of pyrexia, and on the *second day* with delirium, with a rash all over him very like that of scarlatina. There was no peculiar redness about the throat, though it was somewhat flushed," the cause being a strain of the muscles and fascia, followed by deep-seated abscess. In the same journal, February 1st, 1879, Dr. Holland records three cases in the same house originating from scarlatinal infection, conveyed by two pupils who came to the school from another in which scarlatina had broken out. "A was a typical case of scarlatina with the usual throat and skin manifestations and subsequent desquamation. B, was similar, plus limited false membranes in the pharynx. C was equally typical of recognized diphtheria, with false membranes, albuminuria, sequential pallor and paralysis, but with no discoverable rash, and these varieties presented such a progressive fusion in their protean combination that I regarded the diseased states as the modifications of one process and the evolution of a similar germ in dissimilar pabulum." In the same journal, January 25th, 1879, Dr. Clement Dukes records diphtheria and typhoid fever occurring in the same houses at the same time and from the same infection. I have been sent the following interesting report:

"In a school in a country district an eruption broke out, which was called swine-pock by the doctor. It was particularly noticeable on a fine little girl, Fanny D. On that day (Friday) she complained of not feeling well, and was put to bed. Next day she was kept in the house. On Sunday she became worse. On Monday she was pronounced by the doctor to be suffering from scarlatina, and she died on the following Thursday. On that evening both a brother and sister took ill. The brother recovered, the sister died, and on the day after she died a third girl died. In all these fatal cases the throats were far worse than any other cases of scarlatina I ever saw. I suspected bad local sanitation had aggravated the disease.

"A fortnight after the third child died, the father cut his finger; in a short time the hand and arm became painful, and the man had bad rigors. The arm up to the shoulder became swollen, of a rosy color, and terribly painful (? erysipelas). I had him sent to the county infirmary, and, after a long illness, he eventually recovered. While he was recovering, his wife took ill, and I found her suffering from a low enteric or typhoid fever, from which she is now recovering. There was no scarlatina in the neighborhood at the time of its outbreak in this house. The sanitary arrangements were very bad. After the outbreak of scarlatina in this house, it appeared in several families in the vicinity, and among them, in a house of a woman near her confinement. After her confinement she was attacked with what the

doctor in attendance called puerperal mania; subsequently she died."

It seems clear that the origin of all was in the cesspool of D.'s house. Here was a curious combination: at first "swine-pock" well marked, in the course of which arose scarlatina, apparently without any infection or contagion communicating it, generating itself in three other children, in the father septicaemia or erysipelas, and in the mother typhoid fever, and the apparent source of all, the cesspool.

In the *Lancet* of January 19th, 1884, a most remarkable epidemic of scarlatina, for which no source of origin or contagion could be found, but which appeared to spring from defective sanitary conditions, is recorded. Dr. Playfair¹ records the case of a husband attacked with diphtheria and wife with septicaemia resulting from escape of sewer gas, and says, "Who could rationally disbelieve that those two diseases were produced by the same septic poison?" Dr. Fordyce Barker² reports, in the discussion on puerperal fever at the Obstetrical Society of London, cases of pyemia, typho-malarial fever, and diphtheria caused by sewage gas. Dr. J. de Gorriquer Griffith³ records in one house successive cases apparently taking the infection one from the other, of scarlatina, diphtheria, and typhoid. While Dr. John Harley writes: "My own conviction has long been that febricula, enteric fever, scarlatina, diphtheria, quinsy, puerperal fever, septicaemia, *et hoc genus omne* are natural links of one connected chain which the love of unnatural classification has disassociated."

It was held at one time that typhus and typhoid sprang from the same poison, and this is still maintained, I believe, by some. I have seen one case which began distinctly as typhus, mulberry rash, petechiæ, etc., which wound up with pea-soup stools and rose spots; while a couple of months ago, a doctor I met in consultation said to me, Have you observed the curious form of fever that is going now, in which typhus and typhoid are mixed up together? But a much stronger argument as to the change of form and symptoms of a disease depending on the soil in which the germs are planted is furnished by vaccinia.

The variolous poison introduced into the cow is so altered that it produces, when replanted in the human species, a disease

¹ *Obstet. Trans.*, vol. xvii., p. 203.

² *Ibid.*, p. 224.

³ *Obstet. Journal*, vol. vii.

differing entirely in natural history and symptoms from its original form.

"The consideration of these and similar facts leads me to take this opportunity of dwelling on the importance of recognizing the variability of diseases. There is a natural tendency, when engaged in defining diseases, to notice and adhere to the more distinct and typical forms, while we ignore all the less marked or anomalous kinds. But there are many diseases, particularly the exanthemata, which are not rigidly defined. Every practitioner has seen many cases which, either from possessing symptoms common to others, or failing in some of those proper to them, cannot be strictly placed under any absolutely limited head. These we dismiss from our minds as 'anomalous' cases, and then forget them. . . . What do we so much know of the poison of scarlatina and erysipelas that we can say *a priori* it shall only assume one mode of exhibiting itself?"

(To be concluded.)

A REPORT OF THREE CASES OF LACERATED PERINEUM TREATED BY A SINGLE SUTURE.

BY

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THE very interesting article in the January number of this JOURNAL by T. Johnson Alloway, M.D., has led me to report three cases treated in a similar manner. The ease with which the operation is performed and the perfect results given by Dr. Alloway are certainly sufficient reasons to warrant a further trial, and if found to result as perfectly in other hands, it surely should succeed the more complicated operation which appears so formidable to both patient and physician.

CASE I.— This case occurred some time in the summer of 1880, at the "House of Maternity" in this city. I was called in consultation by Dr. McMurray, the house physician; found the patient to be a healthy-looking girl of sixteen years. She had been in labor about twenty-four hours. Upon examination, the head was found to be well engaged, and parts well dilated. The patient being very much exhausted, forceps were applied, and,

¹ Braxton Hicks, Obstet. Trans., vol. xii., p. 50.

after nearly an hour's hard work, a living child was delivered that weighed nearly ten pounds. The perineum was torn down to the sphincter. Believing the immediate repair of a lacerated perineum to be the correct treatment, needles and silver wire were prepared for the purpose of uniting the torn surfaces, according to the old method of many sutures; but, upon placing the edges of the wound in apposition, I was struck with the perfect apposition that was secured when the edges were brought together just at the beginning of the laceration, and it occurred to me that, if a suture was introduced at that point, it would maintain the surfaces in apposition, and union would take place before involution should have sufficiently advanced to draw the surfaces apart. Accordingly, a silver-wire suture was introduced *on a level* with the commencement of the laceration; it was carried to the very bottom of the wound, and out upon the opposite side, including fully a half an inch of tissue on each side of the tear. The ends of the wire were twisted together, and the patient made comfortable in bed. The sister in charge was directed to give carbolyzed injections, and told to use her eyes in introducing the vaginal tube, and be careful to pass it above the suture. This she was very careful to do, and the wound united very nicely, and the suture was removed on the tenth day. There was not an untoward symptom during the whole progress of the case. I have recently examined the patient and she has a perfect perineum.

Notwithstanding the perfect result in this case, I did not fully recognize the principle upon which the success depended, and consequently hesitated to perform the operation the second time, and it was not until June 29th, 1883, that my second case occurred.

CASE II.—This was a strong, healthy young Irish woman of twenty-two years, primipara. Labor was perfectly normal, until the head descended upon the perineum, when a violent uterine contraction took place, and the head was expelled by a single pain, giving almost no time for dilatation to take place, and the result was an extensive laceration of the perineum and posterior vaginal wall. This I determined to repair with one suture. Accordingly, I armed a long perineum needle with a silver wire, and, after introducing one finger into the rectum, the suture was introduced, the needle being made to traverse the space between the rectum and vagina. The suture was introduced on a level with the commencement of the laceration, and when the edges were brought together, and the wire twisted, the wound appeared perfectly closed. This case was treated with carbolyzed vaginal injections administered this time by myself, as the patient was cared for by her mother, who had had no experience with puerperal cases. Recovery was rapid, as rapid as cases where there is no laceration. The suture was removed on the ninth day, and union was perfect, and when I made my visit on the tenth day, found the patient

sitting in a chair. I have examined this case recently and the perineum is all that could be desired.

CASE III.—This case was delivered on the 9th of December, 1883, was a difficult forceps delivery. Perineum was torn down to the sphincter. A silver suture was used in the repair of this case, as in my former cases, and was passed on a level with the beginning of the laceration; when the ends were twisted together, coaptation was perfect, and the parts healed at once. The silver wire was removed on the ninth day, and union was perfect. Patient recovered without an untoward symptom.

These three cases are the only ones upon which I have operated in the manner described by Dr. Alloway, and the result has been perfect in every case. I would not hesitate to operate in this way upon any case of perineal laceration where the sphincter is not involved.

It has always been my practice, when so unfortunate as to meet with a lacerated perineum, to repair it at once. This is in accordance with my early teaching, and subsequent experience has proved to my own mind conclusively that this is the best and safest way out of a serious difficulty, and it seems to me that on this subject there cannot at the present time be two opinions, and were I to neglect the immediate repair of a perineum lacerated to any considerable extent, I should feel guilty of exposing my patient to great and unnecessary dangers. When we think of the numberless and great dangers to which every woman is exposed during her confinement and for a considerable period subsequent to that event, it would seem that the physician would exert himself to reduce those dangers to a minimum, and at all events not add to those dangers, by allowing an extensive wound sustained during the parturient process to remain open all through the subsequent puerperal period, and thus afford an easy avenue for the entrance of all the poisonous and deleterious influences by which the patient may be surrounded, as well as those poisons peculiar to the puerperal condition. We avoid open wounds in other localities as much as possible, and are taught to close them as soon and as perfectly as possible, and this has been the teaching from time immemorial, and yet the obstetrician until recent years has been taught to look with perfect indifference upon wounds of the female perineum; and this is the teaching even now by some high in authority. We have been taught *not to close* wounds in this locality *immediately*, but to wait

until "involution has taken place" and "the lochial discharge has ceased" and "the patient is in a better condition," to undergo the operation. Now, why is this? Is there less danger from an open wound here than there is in other parts of the body?

Dr. Mundé has shown us conclusively, in his valuable paper in the October, 1882, number of this JOURNAL, that there is *more*. Then it would seem that even more care should be exercised to effect the immediate closure of wounds here than in other parts of the body. It is probable that this "let-alone treatment" of perineal lacerations was first advocated because the operation for their repair was thought to be a serious one, one that required so many sutures, and so much time in its performance, that the dangers to which the patient was exposed were increased instead of diminished. The danger of absorption of poisonous material was probably not as well understood then as now, consequently the patient was allowed to recuperate before the operation was performed. But since the use of chloroform has become so general in obstetric operations, and even in cases of normal labor, it has been found an easy matter to repair any laceration that may have occurred before the patient has recovered from the anesthetic, and the results are so much better than from the later operation that the immediate operation is now almost universally adopted and taught. But the operation as it is usually performed is too formidable to be done without an anesthetic. Several sutures are required and much time is necessary for their introduction, and very few patients will submit without an anesthetic. Consequently a skilled assistant is necessary, and altogether the operation assumes a very formidable aspect. Now all this can be avoided, and the operation reduced to the utmost simplicity, by the one-suture method described by Dr. Alloway. No anesthetic is necessary nor any assistant other than the nurse, who is always at hand, very little time is required, and any one of ordinary skill can be sure of a perfect result.

On one point I should differ a little from Dr. Alloway. He says: "The suture may be passed at any point between the beginning and the end of the laceration." But afterward he says, the "nearer the commencement the better." Now I

contend the suture should always be passed *on a level with the beginning of the laceration*. In passing the suture at this point the wound is closed at its commencement, and consequently is protected, throughout its whole extent, from the discharges from the uterine cavity; and it is to this fact that I have attributed the perfect results in the cases I have reported. I admit, as Dr. Alloway says, that the suture furnishes a fixed point where healing may begin, but I doubt if the healing process would go on perfectly if the lochia were allowed to pass over any part of the wounded surface. And then if any portion of the laceration is left open, sepsis is liable to occur, and one of the important ends for which the operation is performed is unaccomplished. It is only by passing the suture at the very beginning of the laceration, and to the very bottom of it, that the wound is perfectly closed. The suture passed in the way I have indicated closes the wound perfectly at its commencement, and consequently the wound is protected throughout its entire extent from the lochial discharges. And the edges of the wound below the suture lie in perfect apposition, and become agglutinated before involution takes place and draws them apart, and perfect union results. The method is so simple that it seems strange it was not thought of before. In all my operations I have used the silver wire, but I see no reason why silk would not answer equally well, and certainly it would give the patient less trouble and I think I shall use it hereafter. In regard to antiseptic vaginal injections I consider them of great importance in these cases, as also in all puerperal cases, and I agree with Dr. Alloway that they should be administered by the physician himself, for the reason that a nurse will not understand the importance of introducing the vaginal tube above the suture, and may do irreparable injury.

TURNING VS. HIGH FORCEPS.

BY

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THE question of the relative merits of these two operations having been the subject of frequent discussion among the physicians of this city during the past few months, it has been to me a matter of astonishment that so great a difference of opinion should exist, and that so little progress should have been made towards its solution. This being the case, it has seemed to me that a simple comparison of the dangers, the difficulties, and the advantages peculiar to each operation might, if it did not afford a definite answer to the question stated, at least be of some assistance by stimulating further discussion.

What then are:—

1st. The dangers,

a. Of high forceps?

b. Of version?

a. The application of forceps to the head arrested at the brim, of course, necessitates the introduction of a foreign body within the uterus and thus favors the carrying of septic material into its cavity. True, this danger is slight if thorough antiseptic precautions are observed, but still as this danger is prominently urged against version, it is no more than just to mention it as also inherent in the high forceps operation.

This operation favors the admission of air and consequent danger from air embolism and from the entrance of germs.

The forceps being applied without reference to the position of the foetal head, they will often (I think I may say usually) lie over its anterior and posterior aspects, that is, one of the blades will be exerting pressure upon the delicate structures of the face. Moreover, being applied to the long diameter of the head, the handles are so widely separated that firm traction cannot be made without exerting a dangerous degree of compression, which by shortening the longitudinal diameter, serves also to increase the lateral and vertical diameters, thus render-

ing still more marked any disproportion that may exist between the head and the pelvis and increasing the difficulty of subsequent extraction.

This application of the forceps to the long diameter has another and a greater danger, for not only are the handles widely separated, but the blades as well. Now what must be the effect of traction? There can be but one answer. Dangerous pressure upon the maternal parts with risk of subsequent sloughing or at least of serious inflammatory processes. This same mal-application of the blades favors slipping of the instrument with all its attendant dangers.

The last danger that I shall mention is due to the distance of the extremities of the blades from the operator's hands. This danger is partially but not wholly overcome by the axis-traction forceps of Tarnier or Simpson. In applying the instrument so far beyond our reach, we run a risk of bruising the maternal or fetal parts unless great gentleness be exercised. We even run a risk of including in the grasp of the blades a portion of the cervix, membranes or umbilical cord, and lastly, we cannot avoid in all cases pressure of the points of the blades against the bladder during traction.

Added to these dangers peculiar to the high operation, we must not forget those inseparable from every difficult forceps operations, and which are by no means lightly to be set aside. In this connection, Spiegelberg says, "It is demonstrable that in even the simplest forceps operations the mortality of the children is still a relatively great one." This mortality he states to be, according to Poppel, eleven per cent, and in his own clinic, where forceps are applied only when strongly indicated, the per cent of deaths in the last five years has been seventeen.

b. The first, and perhaps most strongly urged objection to version is the risk of septicemia, but the statement that was made concerning the same danger in forceps operations applies here as well. Indeed, so strong is my faith in the efficacy of antiseptic precautions in preventing this complication that I believe the factor of septicemia can be wholly thrown out of consideration.

The question of nervous shock I consider one of great importance, for there can be little doubt that the introduction

of the hand and arm into the uterus and the forcible turning of the child must be productive of more irritation of the nervous centres than the forceps operation. This is, however, reduced to the minimum by the employment of ether and the timely exhibition of alcoholic stimulant.

Closely allied to this is the risk of causing a rupture of the uterus or at least of setting up an inflammation of that organ, or of the peritoneum. These accidents are very little liable to occur, however, unless the operation be deferred until the waters have long been evacuated, and the uterus is strongly contracted about the fetus, and not even then need we fear them unless undue force be used.

Now as to the child, it must be admitted that the risks are greater in pelvic presentations than in those of the head, "but," as Schroeder puts it, "by the intervention of art at a suitable moment, the chances are greatly increased, so that turning, if performed under favorable conditions, certainly promises excellent results."

In other words, having the feet in our hands, we have the means of applying traction at any moment and, having the head in the most favorable position possible for delivery, we can, as a rule, terminate the labor so quickly that the risks to the child are really less than those arising from the compression of the head between the blades of the forceps and against the walls of the pelvis.

Again, under the head of contracted pelvis, Schroeder says, "A gentle attempt at turning is always harmless and often succeeds when the head is apparently impacted, thus affording the best chance for mother and child." Again, speaking of the operation performed under unfavorable circumstances, "as in a contracted pelvis after the waters have escaped for a long time and the uterus has firmly contracted," he says, "Even in these cases the act of turning is very rarely indeed hazardous to the child."

To recapitulate.

In high forceps:

1. Septicemia.
2. Entrance of air.
3. Injuries to fetal head.

a. As a result of direct pressure of the forceps upon face or scalp.

b. In consequence of compression of cranial contents by forceps.

c. Due to dragging the head against and through the bony pelvis.

4. Injury to maternal parts, which may be divided as follows :

a. Due to direct pressure of the widely separated blades during traction.

b. To pressure of the fetal head which we are attempting to drag through the pelvis in a faulty position.

c. To slipping of the blades.

d. To inclusion of some part of the maternal tissues in the grasp of the blades.

5. Pressure upon the umbilical cord and consequent asphyxia of fetus ; and I will add, as mentioned in some of the books,

6. Risk of causing separation or fracture of the pelvis during extraction.

In version :

1. Septicemia.

2. Admission of air.

3. Nervous shock and collapse.

4. Rupture of uterus.

5. Metritis and peritonitis.

6. Asphyxia from delayed extraction.

7. Injuries to fetus from forcible attempts at extraction.

The second section into which I have divided the consideration of this subject, namely, the difficulties of the two operations, is of far less importance, and will be discussed far less in detail. I say of minor importance, because, in the first place, the question of the difficulty or facility with which we can do any operation ought to have very little weight in determining our choice of procedure ; and secondly, because the difficulties in these two operations are so wholly unlike, and vary so greatly in different cases.

I think I am justified in saying that, if any physician of much experience in obstetrics were to be asked his opinion, he would hesitate very little in stating that, as a rule, version can be performed with far greater facility than the high applica-

tion of forceps with subsequent extraction. He would undoubtedly go farther and affirm that the latter operation is one of the most difficult that he is called upon to perform, and one requiring the highest degree of obstetric skill.

This being the case, I might leave the matter here, but, as the endeavor of this paper is to gain some accurate data by which we may determine our line of action, I will briefly state the more prominent difficulties met with in the performance of the two operations.

a. In the high application of forceps, the first difficulty we encounter is in the proper introduction of the blades. This may be due to several causes acting either separately or collectively. The instrument being applied simply with reference to the pelvis of the mother, the inconvenient position of the head may constitute the first of these causes. The head may be jammed so tightly into the brim that there is not room for the passage of the blades. The fact that the head is so high up necessitates the introduction of a large part of the hand into the cavity of the vagina, thus diminishing the amount of room for manipulation of the blade. Moreover, bearing in mind the distance of the extremity of our instrument from the controlling hand, and the readiness with which injury may be done the soft parts of mother or child, we must continually work under the disadvantage of using an instrument which is applying force beyond our reach and out of sight, and hampered by the fear of producing such injuries as are mentioned above.

The forceps being finally in position and locked, we arrive at the most difficult part of the operation—extraction. If the occiput be directed posteriorly, we shall prevent, rather than favor, its forward rotation, and, continuing our traction, shall extract the head in the posterior position. This, of course, is undesirable, and, consequently, “a posterior position of the head is a contra-indication to the high application of forceps, except in case of emergency” (Schroeder).

If the head be transverse, we shall experience not only great difficulty in applying the forceps, but shall find it almost, if not quite, impossible to drag the head in that position through the brim, even though the pelvis be of normal size. We shall also find ourselves unable to exert sufficiently power-

ful traction, because of the insecure hold of the forceps and the consequent liability to slip, and also because of the fact that such traction, being expended directly against the maternal soft parts, is liable to give rise to unpleasant results.

If, however, the position be an anterior one, and no great disproportion exist between the size of the head and pelvis, we shall probably find no very great difficulty in extracting the arrested head.

b. The difficulties in the way of the performance of version are of three classes: those met with in our attempt to reach and grasp a foot; those tending to prevent rotation of the body and retraction of the head; and those experienced during the subsequent extraction.

The first class need not be discussed at length, for they are seldom of any great importance. In fact, the skilful operator is rarely at a loss, even for a moment, in this matter, for under the profound anesthesia which is always necessary for this operation, the uterus is sufficiently relaxed to allow of the pushing back of the head and the introduction of the hand and arm. The rest is a mere matter of selection.

Our attempt to make the body follow the foot upon which we are exerting traction is quite another matter and sometimes it is impossible. In the vast majority of cases, however, we shall experience little difficulty in effecting rotation of the body, provided the anæsthesia be profound, and combined external and internal manipulation be added to the traction on the foot.

In some cases, as admitted above, we shall find ourselves foiled in the attempt despite the most persistent and skilful manipulation, and in such cases must fall back upon the high application of forceps and that failing, to more extreme measures.

The third part of the operation, extraction, does not differ from the ordinary course of pelvic presentations in which we are obliged to hasten delivery. The dangers are the same, the difficulties are the same.

Thus we have seen that the principal difficulties met with in the application of forceps to a head in or above the brim are as follows:

- 1st. In applying the blades, as those due to,
 - a. Faulty position of the head.

b. To jamming of the head in the brim.

c. To lack of room for manipulation of the blades, increased by presence of hand in vagina.

d. To distance of extremity of instrument, and consequent liability of causing injury if effective force be used.

2d. In extracting, as those due to,

a. Posterior, transverse, or extended positions of head.

b. To insecure hold and consequent liability of slip.

The difficulties in version are:

1st. In obtaining a foot, due to impaction of head or lack of skill in selecting a foot from the profusion of arms and legs presented to our hand.

2d. In rotating the body, due to impaction of head or firm contraction of the uterus.

3d. In extraction, due to extension of arms or of head, just as in the ordinary case of pelvic presentation.

Each of these operations possesses certain advantages over the other, but as these advantages are of necessity largely dependent upon the relative degree of danger and difficulty in each, it would be simply repeating what has already been stated, to discuss this phase of the third head of my paper. It will be treated of, however, in the closing argument, in summing up the results of the discussion, for is it not the object of the paper to determine if possible the relative value of the two operations as influenced by those factors? There are nevertheless certain advantages inherent in each operation which in no way depend upon these considerations, but arise from the cause that made interference necessary or from some peculiarity of the individual case.

Thus in case of severe hemorrhage, the head being still within the uterus, the forceps, by separating the lips of the os from the rounded cranium, would facilitate the flow of blood, while the hand and arm introduced for the purpose of version, and later the body of the child, would serve as a plug and check the flow.

Again in cases of contracted pelvis the preference should be given to version. Schroeder says, "In the contracted pelvis the use of the forceps is contra-indicated until the head has passed through the contracted brim." He says further, "In face presentations the forceps ought never to be applied when

the face is so high up that it is still possible that turning can be performed. Even in head positions, in cases in which both operations are practicable, turning is always to be preferred."

If interference be demanded because of the prolapse of the funis, version again has the advantage over forceps. In sudden death of the mother, the preference should be given to whichever operation offered a chance of the speediest delivery.

In twin labors, forceps would of course have the preference. In a case of putrid child, forceps might be inadmissible on account of the softening of the head; in premature labor, because of imperfectly formed head. Here version has an advantage.

Having thus pointed out some of the dangers, alluded to the difficulties most frequently met with, and indicated a few of the distinctive advantages of these operations, it now becomes my duty to sum up the results and draw the conclusions, which I hope will be obvious.

The dangers of septicemia and of the entrance of air are nearly equal in each, and hence may be thrown out of consideration.

Nervous shock and collapse are dangers more to be apprehended as a result of version, but they are so seldom noticed and are so little liable to give rise to any lasting symptoms that they may be considered more as inconveniences than as dangers.

Rupture of uterus, metritis, and peritonitis I have shown to be very infrequent and usually preventable accidents, whereas against them we may array:

Injury to maternal parts due to direct pressure of blades; to pressure of fetal head forcibly extracted in unfavorable positions; to slipping of the forceps, or to inclusion of some of the tissues in the grasp of the blades. I will not again rehearse the direful results of such injuries, but any candid observer will frankly admit that they far outweigh the corresponding dangers urged against version.

The child may be injured by forcible attempts at extraction after version, but this danger is altogether lost sight of as we consider the far greater liability to serious lesions, either with or without fatal result, in the other operation. Whereas, in the former case, the child may receive abrasions of the cuticle,

fracture or separation of the epiphyses of the long bones, or at times hemorrhage into the abdomen or cranium, in the latter case we not infrequently find transient impressions, abrasions or contusions of the face with facial paralysis, swelling and ecchymosis of eye, etc., more lasting injury to scalp or face resulting in sloughing or abscess, indentation or fracture of cranial bones, lesions of brain substance or intracranial hemorrhage due to compression of the head by the forceps or against the pelvic bones, or to the above-mentioned fractures of the skull.

In this connection my invaluable authority, Schroeder, says: "Unless the dimensions of the pelvic cavity are uncommonly great, the frontal bone of the child may, if powerful traction be made, be broken against the promontory of even a normal pelvis."

Again, the child is liable to asphyxia in both operations, in version because of delayed extraction; in high forceps because of the inclusion of the cord in the grasp of the instrument, and consequent arrest of the fetal circulation, or because of attempts at respiration produced by the pressure of the forceps upon the head. It must be admitted that the liability is greater in version, though not so much so as is generally thought and believed.

The foregoing I believe to be a fair and unexaggerated comparison of the dangers of the two methods of procedure, and it most assuredly does not result favorably for the forceps operation.

Now with regard to the difficulty of the two operations, as I have already stated, there is very little to be said, but here again the balance is in favor of version.

I have pointed out that unless the head be tightly jammed in the brim, it is usually an easy matter to secure a foot, and the process is so simple that few instructions and little practice are requisite.

Unless the case be a difficult one, such as has been described, the remainder of the operation is also simple; but in some cases great difficulty will be experienced, amounting at times to impossibility. It is then an operation requiring little skill, unless we except the latter part of the stage of extraction, and that does not differ from the ordinary course of a difficult breech presentation.

The high forceps operation, on the other hand, is pre-eminently one which calls for the greatest exhibition of skill and presence of mind. Not only must we know the exact position of the head, but we must continually bear in mind the precise object to be accomplished, we must be on the look-out to aid flexion and secure anterior rotation, we must be continually striving to avoid the various sources of danger already pointed out by guarding against slipping, against too firm compression of the handles or too strong traction or leverage, against working injury with the points of the blades—in fact we must be continually on the alert, in the first place to safely and accurately apply the forceps; in the second place to obtain by means of our instrument the most advantageous results, and thirdly to watch for and provide against injury to mother or child.

The latter operation is without question the one requiring the greater skill, and being attended by difficulties from its inception to its completion, it certainly is *not* the more easily performed.

The conclusion thus far reached (by the facts stated in this paper) is, therefore, that the high forceps operation threatens more dangers to mother and child, offers greater difficulties in its performance, and possesses inherent advantages of smaller number and less importance than version.

There are a few other considerations that are sometimes urged, and may be briefly stated, though I consider them rather as sentimental and imaginary than as weighty arguments. Such is the prevalent idea that forceps being applied so frequently and with such good results in low positions of the head, there is no good reason for drawing an arbitrary line and saying that beyond that they shall not be used. The instrument is the same, the operation is less formidable, the consent of the patient and family more easily obtained, and the child is extracted in the normal position instead of by the breech.

Again, some would object on account of the necessity for administering ether in the performance of version, they not using it in forceps operations.

These and other similar objections are not worthy of discussion, as they are incapable of affecting in the slightest degree the merits of the question before us.

I cannot do better than close the argument by a quotation

from the same authority that I have already called to my aid several times—Schroeder. He says, in speaking of the two operations: “Whilst the head is still movable above the brim, version is easy, and this should always decidedly be preferred.”

THE APPLICATION OF THE FORCEPS IN HEAD PRESENTATIONS WHEN THE OCCIPUT IS TOO FAR FORWARD.

BY

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I WILL first narrate the following typical case: The patient, Mrs. H——, was thirty-five years of age, a healthy woman of large frame, with considerable adipose tissue, and the mother of several children. All of her previous confinements were normal, possibly a little tedious, and were attended by midwives. About 10 P.M. November 19th, 1883, the husband of this lady came to me, stating that his wife was in labor, that a midwife was in attendance, but that, as the labor was continuing so long, he desired me to go and see her. On reaching the house, I elicited the following history: She had been in labor all day; at 4 P.M. the os was dilated (so the midwife stated) about as much as it was on my arrival. At that time also the waters were discharged. Although the pains had continued good, there had been no advance of the head. On examination, I found the head presenting at the superior strait and in the first position. The os was soft and but little dilated; however, on passing my finger around within it, I found it readily dilatable. It was one of those cases where the presenting part of the child was abnormally distant from the os internum, and gave the cervix that condition and shape that recalls to one's mind the mouth of a purse. After ascertaining the condition and the relation of the parts, I placed my finger against the presenting part and waited for a pain. When the pain did come, it made no impression upon the presenting part whatever. I then applied the forceps, locked them, not without some trouble, then with a pain I began making traction, when I felt the handles closing and the blades gradually slipping off. I removed the instruments and re-applied them, but with exactly the same result. Having had some experience with just such cases before, I was not at a loss to know what to do in this instance, and in a short time succeeded in the delivery. I recognized that the whole difficulty depended upon the fact that the head of the child was lying too far forward upon

the pubic bones, thus accounting for the apparent lengthening of the cervix, and instead of the pain causing the head to engage within the os, and thereby drawing the cervix tense around the presenting part, the efforts of the uterus were expended upon the child with its head pressing against the pubic bones.

In these cases the slipping of forceps is easily accounted for. In applying the blades, owing to the forward position of the vertex, either one or both are simply placed upon the cheeks of the child, somewhere anterior to the ears, and the natural result, when we make pressure and traction upon the handles of the instrument, is that we first feel the handles closing and then the blades slipping off. The most frequent position taken by the blades, however, is one, the first applied, passing on to the head properly, and then the second blade can only be gotten on the cheek, and it is when applied in this manner that so much difficulty is experienced in the locking of the instruments; but even if we succeed in locking them the result is the same, viz., slipping.

As I stated above, the first blade can generally be placed correctly, for it can be carried further forward around the pubis, within the pelvic cavity of the woman, than the second blade. When the second blade is carried along in conformity to the curve of Carus, if the presentation under consideration exists, the point of the second blade must be carried further forward at its completion than when the presentation is normal.

The object of this paper is to point out the proper manner of applying the blades (or more properly, the second blade, as it is the one generally at fault) of the forceps, so that they will take the proper hold upon the head, when they will not slip, and the child can be readily delivered if there is no other obstacle in the way.

After passing the first blade of the forceps, which can generally be done in these cases, the difficulty arises in the application of the second one. After the blades are locked and with slight traction slip off, in re-applying them to secure a better hold, we are apt, in applying the second blade, to pass the finger of one hand into the vagina, and place it against the blades, somewhere anterior to the lock, and use this finger as a fulcrum, while we have the handle of the blade in the other hand to use as a lever, and in this manner attempt to slide the

blade further forward upon the head of the child. About as often as we try this manœuvre we fail, because the handle of the first blade occupies the posterior commissure of the vulva, and our leverage or power is limited and obstructed in this direction, while the ear of the child is an obstacle or weight at the other end, preventing the sliding forward of the blade. The only way to place the second blade in its right position is to carry the hand, after introducing the blades, but before attempting to lock them, into the vagina with the index and middle fingers extended, and pass them up into the uterus, if necessary, until the anterior ear in L. A. O. positions, or the posterior ear in R. O. A. positions, is felt, then lift the blade on to or over the ear until it lies in the proper position over the ear, after which the handles lock with ease, and we are not further annoyed with the slipping of the forceps. That the forceps may sometimes be applied without the above manœuvre in these presentations is true, but it is the exception rather than the rule. It matters not what presentation of the normal head exists, if we get the proper hold with the forceps they cannot slip.

This presentation occurs in two classes of persons, namely, those who have naturally, not pathologically, too much anterior curvature to the vertebral column simulating lordosis, and in those who have borne a number of children, and the lordosis is gradually produced by the gravid uterus causing the abdominal muscles to pull forward the "small of the back," and causing the abdomen to appear pendulous.

The patients in whom this dystocia has occurred, in my experience, are ones with rather large frames and a tendency to embonpoint. The pelves have generally been sufficiently capacious and the vaginæ ample.

These cases are among those where craniotomy is often resorted to, and I am sure that if the small modicum of experience that I try to lay before you were more generally appreciated, craniotomy might many times have been avoided. In illustration, I will cite a couple of cases.

I was called some years ago to attend such a case as has been here described. The cervix was long, but soft and dilatable, although its whole internal surface, or canal, was in apposition—the purse-mouth cervix. As no advancement of the head was made, I applied the forceps, but they gently lost their hold, and gradually slipped. This was repeated more than once with the

same result, although I used the greatest effort to get a hold on the head nearer the pubic bones. I then sent for assistance. The physician who came brought his instruments, differing from mine, with him. The patient was now anesthetized, when I applied his forceps, but with no different result. Then he applied them and they slipped; he applied mine and they slipped also. I then, with the forceps removed, passed my hand into the vagina and up to the head of the child, and found that I could pass my fingers all around the presenting part; why the instruments did not prove efficient I could not devine, when there appeared to be such ample room. After working for four hours, with the patient under an anesthetic most of the time, as she declined to permit any interference without it, we came to the conclusion that there was nothing to do but perform craniotomy. Before doing so, I made one final effort with the forceps, but before attempting to lock them, I passed my hand as above described, and found the second blade in front of the ear and upon the cheek, when I lifted it over the ear, locked the forceps, and delivered in ten minutes.

About two years ago, a practitioner sent for me to assist him in performing craniotomy. A midwife had been in attendance hours before he was called; he had applied the forceps many times before I saw the case, but they had slipped off each time. I recognized the case to be one such as is the subject of of this paper, adjusted the forceps over the ears of the child, and delivered it in a short time living.

I believe that it is in just such cases as these that face presentations spontaneously take place; the occiput becomes engaged on the pubes, and the face is gradually forced down. It is also possible for us to convert the vertex into a face presentation when we get hold of the cheeks with the forceps.

A critic might say with regard to the first case of the two last reported here: Why did you not deliver by the feet? I would reply that early in the case it would be quite proper, but, after the cervix had passed over the head of the child, it would be a very dangerous proceeding, for if, in the effort at turning, the occiput should catch on the anterior lip of the os, you would have to double up the child in the vagina, or tear the vagina across at its junction with the uterus before you would succeed.

PUDENDAL HEMATOCELE.

BY

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THIS affection has been known under a variety of names. It has been designated hematocele of the vulva, hematoma of the vulva, thrombus of the vulva, varicose tumor of the vulva, etc., etc., by different authors. The name at the head of this paper was first employed by McClintock, and, in the opinion of the author, it is to be preferred to the others, inasmuch as it is concise and explicit, both as to the seat and the character of the accident. The disease was recognized and described so long ago as 1524 by Rueff, of Zürich.

As indicated by its various appellations, the disease is a bloody tumor of the vulva, or rather an extravasation of blood into the pudendal tissues. The disease may be divided into two great classes, according as it occurs in the pregnant or non-pregnant condition. When it occurs in the non-pregnant woman, whether she be a virgin or a woman who has already borne children, it is *always* of an *extraneous* traumatic origin. It can, of course, also occur traumatically in a pregnant woman, but among this class it most usually happens during the process of parturition. Strictly speaking, these latter cases are also traumatic, but in this paper I desire to limit the use of the word traumatic to those cases of pudendal hematocele which are produced by violent causes external to the patient, and entirely disconnected from pregnancy.

It is but recently that two cases of traumatic pudendal hematocele, in rapid succession, fell under my notice; one case occurring in a virgin and the other in a lady who had already borne one child. I subjoin the clinical histories of both of these cases.

CASE I.—On December 3d, 1883, I was called to see Mrs Fl., æt. thirty-two years, married, and the mother of one child. She gave the following history. She was standing upon a high-backed chair, in order to reach for something on an upper shelf. She had her arm outstretched, when she lost her balance, the chair

suddenly lurched, and she fell backwards, with very great force, upon the back of the chair, violently striking the pudenda in her descent. She at once suffered acute pain, and an enormous swelling presented itself in the left labium majus, including a portion of the labium minus, and extending down from about the clitoris to the perineum. It gradually enlarged, until it was equal in size to a small fetal head. I saw her within fifteen minutes after the accident, and advised the use of ice-cold compresses and bandage, with an evaporating lotion added to the ice-cold water.

Inasmuch as the tumor was of such large size, I considered that absorption of its contents would be most improbable; and to wait till suppuration should be established, before evacuating the sac, seemed to me to involve too much risk and danger from absorption of septic matter which might be generated in the extravasation. Hence I thought it most judicious to incise the tumor at the earliest moment which would admit of safety, as regards the cessation of hemorrhage into the sac. In accordance with this plan, on the fourth day after the occurrence of the accident I made a free incision in the sac, turned out all the loose clots, and washed out the cavity with a disinfectant solution. I then introduced a certain amount of powdered iodoform into the cavity, and applied a firm compress of carbolated absorbent cotton to the wound. The wound was irrigated with antiseptic solutions each day. For about a week, the dressing was continued, as it had been applied in the first instance, and by the end of which time the wound had completely healed.

CASE II.—On December 29th, 1883, H. M., æt. twenty-four years, unmarried, domestic, whilst engaged in her household duties, fell forcibly backwards and struck the back of a low chair behind her in such a manner that her thighs became engaged on either side of the chair-back, hitting herself violently in the vulvar region. She immediately suffered great pain, which continued for some time, and was only relieved by opiates. Simultaneously, an enlargement was developed in the left labium majus of about the size of a goose egg. It was very tender and soft, and of a bluish color. Ice-cold water affusions were ordered to be applied to the tumor with a firm bandage. It did not grow any larger. After a week had elapsed, no indications of absorption could be detected, whereupon hot fomentations were advised, in order to encourage suppuration in the sac. Suppuration was soon established, and in about one week later, while the patient was making some muscular exertion, the swelling suddenly burst spontaneously. After it had been evacuated, the cavity was thoroughly cleansed with disinfectant lotions for the next two weeks, at the end of which time it had completely healed.

In both of these cases, the healing process was very much facilitated by the natural contractility of the parts involved.

Of the various authors, each devotes a different degree of

attention to this malady. Some ignore it altogether, others pass it by with a short paragraph, and a few only describe it somewhat at length.

Etiology.—Because in pregnant women the venous circulation in the lower portions of the body is interrupted, and because at the same time a dilatation and a thinning of the walls of the blood-vessels occurs, one would be apt to conclude from *a priori* reasoning that the pregnant woman would be specially predisposed to this accident. A few writers hold to this view. It has, however, been proven, and the fact has been corroborated by nearly all writers on obstetrics, that the occurrence of pudendal hematocele even in the pregnant woman is rare, and it is not to any material extent favored by the anatomical and physiological changes which the parts in question undergo during gestation. Statistics also bear testimony in the same direction, for, if the circulatory changes of pregnancy specially predisposed to this accident, we ought to meet with it very frequently, in fact much oftener than we do. Yet all who practise obstetrics to any great extent must acknowledge that, even in a large number of cases, they have met with it quite infrequently. Further on in the course of this paper, I shall submit some figures which will sustain the position here assumed that the circulatory changes accompanying pregnancy do not necessarily predispose to pudendal hematocele. Here, like everywhere else, those cases which do occur as complications of pregnancy and parturition are but the exceptions which prove the rule.

It is very rare indeed in the earlier months of pregnancy. It occurs usually during labor or after delivery, and especially just as the presenting part is about to clear the vulva. In these cases, a large child, a narrow pelvis, a delay at the inferior strait with the subsequent exertions to overcome the impaction, may each be an immediate cause of the catastrophe. The abuse of the forceps in unskilled hands has been a cause at times. Lawson Tait mentions a case in which the forceps slipped at least twenty times, the parts were frightfully mangled and macerated, and the vulva was infiltrated with an extensive blood clot. The parts took on sloughing and in a few days the patient died.

¹ Diseases of Women, by Lawson Tait, F.R.C.S., etc. W. Wood & Co., 1879, page 19.

As a complement to the two cases of extraneous traumatic pudendal hematocele previously narrated, I will take the opportunity to introduce in this connection the histories of two cases illustrative of this affection when it occurs as a complication of the puerperal state.

CASE III.—Mrs. L. F. was taken in labor March 1st, 1881, and was attended by an ordinary “midwife.” During the progress of the labor the right labium majus became swollen to the size of the fetal head at full term. The midwife became alarmed and sent for me. I, however, was absent from home, and the case was referred to Dr. A. M. Jacobus. Dr. Jacobus saw the patient and attended her, and to him I am indebted for this history of the case. I will quote from the notes furnished me by Dr. Jacobus. “There was no history of previous local injury or trouble. As the labor pains became stronger the swelling became discolored and very hard. By this time the vagina was so nearly occluded that the labor was interfered with and very much delayed. Upon my arrival (the pains seemed to have become stronger) the head had descended and, by its great pressure upon the swelling, had caused the rupture of the tumor. The discharge was sanguineous in character. The child was then speedily delivered *per vias naturales*.”

CASE IV.—Mrs. B., æt. thirty-three years, was confined of her sixth child on February 23d, 1884. She had strained much at labor. Shortly before the child was born, a swelling showed itself in the vulva on the right side, involving both the labium majus and the labium minus. It grew to the size of a small orange. The patient suffered striking pains in that region. The tumor interfered very much with micturition. On the third day the swelling became livid and showed signs of ulceration on its most prominent part. Warm fomentations were used and on the fourth day the sac discharged itself spontaneously. The contents consisted mostly of decomposed blood and but very little pus. The case was then treated with antiseptics and iodoform, similarly to Case I. Complete recovery was established in four days afterwards.

In the extraneous traumatic cases, direct violence to the parts, as blows, falls, kicks, violent attempts at coitus, as in rape, are usually the causes of the disaster. My friend, Dr. Leale, related a case in which the injury was produced in a singular manner. The patient was gored by the horns of a cow in the pudendal region, causing a complication of wounds, among others pudendal hematocele and hernia. The patient many years after died in consequence of strangulation of this hernia.

As the vulva is so largely made up of areolar tissue, it offers

very little resistance and hence extensive bleeding results. It appears that in the majority of cases the left side is affected. In the opinion of the author, this is but a coincidence, for no special reason exists, so far as he could determine, why the accident should have a predilection for one side rather than the other.

Symptoms.—When the accident happens, a sudden sharp pain is experienced, with the instantaneous development in the pudenda of a large and growing tumor. This swelling is, of course, the effusion of blood into the surrounding tissues, which may in rare instances continue until the dissolution of the patient occurs. Usually the labium majus only is involved, though it has been seen in the labium minus, as in two of my cases. It has even occurred symmetrically in the labia on both sides of the vulva. It has extended to the space between the superficial and middle fascia of the perineum. Cazeaux¹ cites a case in which the extravasation extended into the right hypochondrium to the false ribs and to the attachment of the diaphragm. It may also press on the neck of the bladder or upon the rectum, thus causing retention of urine or feces. It varies in size from that of an egg to that of a fetal head. The hemorrhage may be venous, or arterial, or both. The ruptured vessel is usually seated in the lower part of the vagina and less frequently in the vulva.

Pain may sometimes be absent, though very rarely indeed. The vagina may be scarcely patulous from the large size of the effusion.

The most frequent point of spontaneous rupture is at the junction of the larger and smaller labium. Sometimes when a rupture occurs a fistulous tract may result. Pudental hematocoele also frequently accompanies the complete rupture of the uterus and vagina.

Diagnosis.—The diagnosis of pudental hamatocoele is easy. The sudden nature of the affection is generally sufficient to distinguish it from edema or abscess of the labium, from pudental hernia whether of omentum or intestine, and from inflammation of the vulvo-vaginal glands, or from other local

¹ A Theoretical and Practical Treatise on Midwifery, by P. Cazeaux, revised by Tarnier, 1878, page 689.

troubles. It must also be differentiated from varix of the vulva and vaginal cystocele.

It is by no means a common affection, in fact it is quite infrequently met with. This statement applies more particularly to those cases having an *extraneous* traumatic origin.

¶ Prof. T. G. Thomas, of this city, in a recent edition of his book on "Diseases of Women,"¹ says that "in an extensive special practice of twenty-seven years he had met with only four cases of pudendal hematocele of a traumatic origin in the non-pregnant condition."

Velpeau,² however, states that he has seen six traumatic cases in the non-pregnant woman in the course of one year, which is, indeed, a very remarkable experience.

Scanzoni³ observed fifteen cases occurring during the course of labor—eight before the child was born, six during the delivery of the placenta, and one in the interval between the births of twins.

Playfair⁴ remarks that various French authors have collected one hundred and twenty-four cases, of which forty-four proved fatal.

But two cases (extraneous traumatic) of pudendal hematocele were reported among all the recorded cases under treatment at Bellevue Hospital during a period of seven years, from 1876 to 1883, and both of these occurred in the latter half of the last year. The tumor in each case was as large as an egg. One was produced by a kick, and the other by a fall astride of the edge of a pail. One of these cases was treated with hot fomentations from the outset, and was finally opened, discharging a sanguineous purulent material. The other was immediately evacuated. During the same period of time, but a very few cases were treated in the hospital which had any connection with the parturient state. These cases occurred during the services of Drs. Fuller and Lewengood, of the house staff, and to them I am indebted for these figures.

In a continuous series of six hundred and forty-six obstetrical cases in my own practice, covering the period of the last

¹ Diseases of Women, ed. 1880, page 132.

² Diseases of Women; Thomas, 1880, loc. cit.

³ Puerperal Diseases, Prof. Fordyce Barker, 1878, page 55.

⁴ Playfair's Midwifery, edited by Harris, page 357.

nine years, I met with this accident only once. This *one* case is Case IV. reported in this article.

Dr. E. F. Ward,¹ in a practice of two thousand obstetrical cases, *never* met with it. In a recent private communication to the author, Dr. P. F. Mundé writes that he has seen but two (2) cases of pudendal hematocele occur among three thousand labor cases. One case was conjointly external and pelvic in a primipara. Dr. Mundé has never seen a case of pudendal hematocele in the non-pregnant woman.

These statistics I referred to in a previous page, as confirmatory of the fact that the changed condition of the circulation in the lower portions of the body of the pregnant woman does not specially predispose to this accident.

Prognosis.—The accident is less dangerous in the non-pregnant than in the pregnant woman. At one time, it was considered as a very unfavorable and almost fatal complication. Suppuration or decomposition of the contents of the sac may take place in the effused blood, and, if not promptly evacuated and properly treated, auto-sepsis may take place from the absorption of the contents of the tumor. This is more likely to happen in the pregnant condition. Improved methods of treatment at present in use have reduced the large mortality which formerly seemed to attend this affection, particularly in connection with the parturient state.

The amount of blood effused may be so great as to cause collapse of the patient. Deveux reported twenty deaths in sixty cases, Winckel six deaths in fifty cases, Barker two deaths in twenty-two cases, and Scanzoni one death in fifteen cases.²

Prof. Barker³ reports nine cases in private practice, all resulting in recovery. Of thirteen hospital cases, two died of puerperal fever.

Hewitt narrates a case which Mauriceau mentions, in which a blood tumor had existed for twenty-five years in the left labium majus, which, on being opened, gave issue to matter like the contents of an aneurysmal sac.⁴ This is, indeed, a

¹ Minutes of Discussion of North-Western Med. and Surg. Society, vol. ii., page 118.

² The Science and Art of Midwifery, W. T. Lusk, A.M., M.D., etc., New York, 1882, page 580.

³ Puerperal Diseases, by Prof. F. Barker, 1878, page 60.

⁴ Hewitt's Diseases of Women, vol. ii., page 418, edited by H. Marion Sims, N. Y., 1883.

very exceptional case, and I believe the only one of the kind on record.

Pudendal hematocele is very variable in its course and termination. It may end by resolution or absorption, like a thrombus in any other situation. Especially when it is small, it may disappear spontaneously by absorption, or else the clot may become encysted, remaining innocuous in the tissues for an indefinite length of time.

Treatment.—As regards treatment, there seemed to be some diversity of opinion among authors, for the directions laid down by them are not uniform, yet a general rule of procedure can be deduced therefrom. In both of my traumatic cases, I was in the outset, from lack of experience, at a loss how to treat them; I became only more perplexed after I had consulted the views of various authors, and I finally decided to act as I did in the cases mentioned.

Of course, it is a well-settled principle in surgery *never* to incise a recently-formed effusion of blood, lest uncontrollable and fatal hemorrhage ensue. Hence, when seen early, or when effusion of blood is still going on, our efforts should be directed to the arrest of the hemorrhage, by the application of cold and pressure. As already stated, small effusions may become absorbed or encysted, and all the treatment required in these cases is to keep the patient quiet and to apply evaporating lotions with pressure. A good method of applying internal pressure in these cases is that suggested by Prof. Lusk.¹ It consists of this: A rubber bag or a large Barnes' dilator filled with iced water should be adjusted in the vagina, and then, by the double action of cold and pressure, the hemorrhage may be checked. Opiates may be given to relieve pain.

In those cases in which the effusion is so large that absorption is improbable, it will be necessary at some time in the history of the case to incise the tumor. If we wait till suppuration shall have set in, we will run the risk of sepsis. Hence it seems to me that, after waiting a reasonable time after the occurrence of the extravasation, and when we think that the hemorrhage has ceased, it is preferable to incise the tumor,

¹ Science and Art of Midwifery, by W. T. Lusk, A.M., M.D., etc. N. Y., 1882, pages 580 and 581.

clear out the clots, wash out the resulting cavity with an anti-septic solution, and make use of iodoform with firm pressure.

If the bleeding should continue, however, after thus incising the swelling after a reasonable period of time shall have elapsed, then the bleeding vessels should be secured by ligature. It has even been recommended to apply the thermo-cautery to the bleeding cavity, after the swelling has been opened in order to arrest the flow of blood.

If the effusion occur during labor, and it be sufficiently large to impede the birth of the child, it is proper to make a free incision at the most dependent part, and the advancing portion of the child will act as a tampon to control the hemorrhage. If it occur before the presenting part has descended, pressure must be applied. If it form after the birth of the child, it should be treated as one of an extraneous traumatic origin. At whatever period the incision may be practised, it is not advisable to remove all the clots at first; leave those which seem to adhere, and they will gradually come away with the subsequent dressings. The incision should be free and not merely a puncture and after the clots have been extracted the natural contractility of the parts will cause the wound soon to close up. As regards the part of the tumor where we are to operate, we must always be guided by the fact that it is necessary to secure free drainage from the wound. Hence it should always be opened at its most dependent part, even if there should be indications of pointing elsewhere. It is usually preferable to cut on the inner or vaginal side of the labium majus.

If, when the effusion is small, no sign of absorption occur after a reasonable period of time, then it will be proper to employ hot poultices to encourage suppuration, and as soon as pointing takes place, the pus should be liberated. The discharge will sometimes emit a distinctly stercoral odor which might possibly lead to the erroneous opinion that the hematocele is complicated with a recto-vaginal fistula. It has been well demonstrated by surgeons that, in abscesses near the rectum, it is quite usual, without any communication being present with the intestines, for the purulent matter to possess a fecal odor.

Since the great mortality in former times was most probably due to septic infection from absorption of the purulent and de-

composing sanguineous constituents of these effusions, it is very necessary and important that the antiseptic method should be rigidly carried out in the frequent irrigations of the cavity with disinfectant lotions, preferably of the bichloride of mercury 1 part to 2,000, or of phenic acid of a two-per-cent strength. Then all the indications for treatment will have been met, and a favorable result may be, in very nearly all cases, confidently expected.

DYSTOCIA FROM DORSAL DISPLACEMENT OF ARM.

BY

B. G. LONG, M.D.,

Buffalo, N. Y.

DYSTOCIA, on account of the forearm being back of the head in vertex presentations, was first described by Sir James Simpson in 1850. It is an exceedingly rare complication, as is evinced by the few cases on record, and the description of it being absent from a majority of our text-books.

I here present the history of a case from my own practice, with a review of the cases recorded in English literature:

I was called at 8.30 A.M., November 17th, 1883, to attend Mrs. J. N. G., American, aged thirty, in her second labor. This was my first acquaintance with the patient, as I was summoned during the absence of her family physician from the city.

Her first child, with which she had had an easy labor, was ten years of age; her health, she said, had been very good prior to and during this pregnancy.

Pains had kept her awake all of the previous night, and were now moderately severe at intervals of about twenty minutes.

On examination, I found the os well dilated and soft, the membranes intact, with vertex presenting.

At 9.15, while making an examination, the membranes were ruptured. Pains now came on rapidly; and with such a roomy pelvis I felt justified in assuring my patient that she would have a short and easy labor. On examining again after several hard pains, I was very much surprised that no progress had been made; the position was the second (R. O. A.).

By abdominal palpation, I was enabled to map out the position of the child, and detected the fetal heart just to the right of the umbilicus. A very peculiar, hard, angular protu-

berance above the symphysis pubis was discovered, which seemed to be the cause of delay. After a very careful examination and a great deal of puzzling, I decided that the tumor must be the elbow of the left arm, with the forearm thrown back of the neck.

Failing, after persistent efforts, to reach and dislodge the arm through the vagina or by external manipulation, as mother and child were both in excellent condition, the labor was allowed to go on for a time with the hope that delivery might be accomplished by natural efforts.

After two hours of waiting, with no apparent progress, the pains became weaker, and the mother somewhat exhausted. I then applied the long (White) forceps at the superior strait, and, by very forcible traction during pains, succeeded in delivering her, without anesthesia, at 1 P.M., of a large boy, who was very active, and none the worse for the protracted labor.

While manipulating the instruments, it was impossible for me to watch the course of the displaced arm, but I think that it slid down the child's back. My diagnosis was strengthened by the fact that the left arm remained blue for several days, showing that pressure had been greater on that member than on any other part of the body.

In Sir James Simpson's case,¹ the true condition was undetermined until the patient was thoroughly anesthetized, and the hand introduced far up into the vagina. He recommends bringing the arm down, and converting it into a head and arm presentation. This was done in his case, but was unsuccessful, as he was obliged to perform podalic version. It would appear eminently more proper in such a condition, with the patient anesthetized and the hand introduced far enough to reach the hand, to deliver by turning than to convert it into a head and arm presentation, which is anything but a desirable one.

Mr. J. Jardine Murray reported a case² in 1859. He also delivered by podalic version without first bringing down the hand.

In 1862, Mr. Fox, of Sudbury, records a case³ of convulsions, where the head presented "with the forearm placed across the back of the head."

Two cases⁴ are reported in 1868 by Dr. Angus McDonald of this variety of dystocia.

¹ Edin. Med. Journal, 1850, p. 389, and Selected Obstetric Works, p. 381.

² Med. Times and Gazette, 1861, vol. i., p. 627.

³ Dublin Quar. Journal of Med. Science, vol. xxxiv., p. 281.

⁴ Edin. Med. Journal, 1869, vol. i., p. 59.

Dr. C. H. Brockway, of Lynn, Mass., reports a case¹ occurring in his practice on May 26th, 1878, which was left entirely to nature until the head was born, when the real cause of delay was first ascertained; the arm was then replaced, and delivery of the shoulders was followed immediately. The management of this case is severely, though justly, criticised in the same journal a month later.

A case² is reported by Dr. Lewis, of Tenn., in which flexion, rotation, and extension occurred before the condition was discovered.

Playfair reports a case³ under his care where version was performed after the forceps failed to accomplish delivery.

A. R. Simpson,⁴ in addition to four of the above cases, and several others where the condition was somewhat analogous, reports a case where delivery was easily effected by the use of the Tarnier forceps, being the first on record where forceps were successfully employed. Diagnosis was made by examination per vaginam.

Remarks.—In this condition, it is very important that the diagnosis should be made out as early as possible. In very large pelves, this may be done by internal examination, but the best and most satisfactory manner is by external palpation, by which, after a little practice, the exact position can be determined in all but very obese patients.

This method is too much neglected by the general practitioner, and, in fact, has been but recently brought prominently forward by Mundé and Lusk in America, and Pinard and Tarnier in France.

Which is the better way to deliver in such cases, forceps or version? From the literature on the subject and my own experience, it would seem to me that where the pelvis is large and the head small in proportion, forceps delivery is safer for mother and child; but where the contrary exists, version is preferable on account of the liability to rupture of the uterus, where such an unnatural angle presses upon the uterine walls. The fact will be observed that two of the above cases were safely delivered by version after the forceps failed.

¹ Boston Med. and Surg. Journal, 1879, vol. c.

² AMERICAN JOURNAL OF OBSTETRICS, vol. xi.

³ The Science and Practice of Midwifery, p. 326.

⁴ Contrib. to Obstetrics and Gynecology, p. 130.

It seems very probable that there are cases of this description which are undiscovered, because of their causing only delayed labors and not actual dystoeia.

FOUR CASES OF PUERPERAL CONVULSIONS TREATED WITH
HYPODERMIC INJECTIONS OF MORPHINE.

BY

WM. C. WANNAMAKER, M.D.,
Orangeburg, South Carolina.

CASE I.—I was summoned to attend M. F., primipara, aged eighteen, on the 14th of March, 1882.

The messenger said she had been delivered but a few hours, and that she was having convulsions, one after another, in rapid succession. Arriving at the bedside a short time after receiving the message, I found the patient suffering with a violent convulsion. I learned from her father that she had had seven convulsions besides the one I witnessed. She was comatose—administered one grain morphine hypodermically at once—remained one hour, instructed the friends to let me know if the paroxysms returned, and left.

I called next morning and learned that the convulsions had not returned. She had some fever, and was drowsy, but did well from this time on.

CASE II.—On the 31st of July, 1883, was called to see S. E., primipara, aged twenty-five. Patient was in deep coma when I arrived, had had but two convulsions, the last about one-half hour before my arrival, child had been born before the advent of the convulsions. Administered one grain morphine hypodermically. Patient slept for ten hours, and remained dull and drowsy for as many hours more, but she had no more convulsions, and made a nice recovery. The most trouble and inconvenience she had after the morphine was given, was caused by a badly bitten tongue.

CASE III.—Was called to see E. R., primipara, aged sixteen, on the morning of 31st September, 1883. Patient had a severe convulsion as I entered the room. Her mother stated that she had been in labor eight or ten hours, that she had had seven convulsions before. I arrived, that the convulsions returned every half or three-quarters of an hour at first, but the last three recurred at intervals of fifteen minutes.

An examination revealed a rigid os, dilated as large as a silver quarter dollar. I inserted one and one-third grain morphine un-

der skin. Ten minutes after giving the morphine, patient had another severe convulsion. After this, she slept nicely, only groaning and turning about in bed when the labor pains were severe, and was delivered of a fine boy four hours after morphine was administered.

She made a good recovery, although her temperature ran up to 104° at one time.

CASE IV.—L. M., primipara, aged twenty-four, sent for me about four o'clock on the morning of the 23d of December, 1883. Her husband said she had a "spasm" which woke him up, that she had made water rather often during the night, but did not complain; he did not expect her to be confined for a month at least.

Having but a hundred yards to walk, I reached the bedside in a few minutes, just in time to see her go off in a second convulsion. I inserted two-third grain morphine under the skin, and made an examinations which revealed a slightly dilated os, large enough to admit the end of my index finger. No contractions were felt when this examination was made. She slept most of the day, but about eight P.M., labor pains came on pretty severely and thoroughly aroused her. Everything went on smoothly until three o'clock on the morning of the twenty-fourth when the child was born, immediately after which event the patient was seized with another convulsion.

Inserting two-third grain morphine under skin, it kept her quiet until six o'clock, when she was seized with the fourth convulsion. Again I repeated the two-third grain dose of morphine which controlled the paroxysms till eleven o'clock, when she had the fifth convulsion, with temperature 104° , pulse 160. Fearing serious brain lesions I gave gtt. ij. ol. tiglli on tongue and applied cold water to head. Patient had no more convulsions, but gradually grew worse, and died at eight P.M. the same day.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF NEW YORK.

Meeting, December 18th, 1883.

LARGE INTRAUTERINE MYOMA REMOVED BY LAPAROTOMY.

DR. J. B. HUNTER related the following case: A woman, thirty-four years of age, came to him with an abdominal tumor which her physician had pronounced ovarian. It had first been noticed eight months before, and had grown rapidly, having reached about the size of a child's head. It seemed too hard for an ovarian tumor and too soft for a fibroid. The patient, who was unmarried, had always had scanty menses until since the appearance of the

tumor, when the flow increased in quantity at the regular periods, and there was some flow during the intervals. Several of the surgeons of the Woman's Hospital examined her, and opinions were divided between a fibro-cyst, an extrauterine fibroid, and an ovarian tumor. Laparotomy was performed on Saturday, December 15th, and, after a free incision had been made, a large tumor, believed to be a fibroid, was encountered. Several turns of an elastic ligature were thrown around the neck of the entire mass, and an incision was made, when it was found that the mass was composed of the uterus greatly enlarged by the presence of a tumor within. The tumor was enucleated and withdrawn through an incision made into the uterus. A clamp was then put well down on the uterus, which was cut away and the abdominal wound closed around the pedicle. The patient had since done well. The tumor had not yet been examined by the pathologist. It contained a considerable amount of bloody fluid in cavities, but not a sufficient amount to give distinct fluctuation. The elastic ligature controlled hemorrhage perfectly. Before the operation the probe had been passed into the uterus a distance of four inches, but could not be swept around.

DR. G. T. HARRISON thought there could be no question but that the proper method of removing such a tumor was by laparotomy; the principal question which could arise was with regard to the method of treating the stump.

DR. H. T. HANKS asked whether, had the exact nature and condition of the tumor been recognized, it would have been worth while to try treatment by ergot.

DR. H. J. GARRIGUES did not think that ergot would cause a tumor of such large size to disappear.

A TRACHELORRHAPHY NEEDLE.

DR. HANKS presented a needle for repairing the cervix. It possessed the advantage over that in ordinary use of having the concave side rounded and the convex angular, rendering it stronger and more suitable for introduction through cicatrized tissue. The smallest portion of the shaft was at the eye, thus admitting of easy passage of the needle when once the point had been passed and seized with the needle-holder.

DR. HUNTER and DR. BACHE McE. EMMET had used a needle answering to the description given by Dr. Hanks with advantage; it was probably the same needle.

CERVICAL MYOMA COMPLICATING PREGNANCY.

DR. HARRISON had been called to see a woman, thirty-four years of age, who had been married eleven months. During the first five or six months of menstrual life the flow had been regular; it then became irregular, and during the last few years she had suffered from lumbar and pelvic pain and difficulty in urination. The pain grew in intensity. She was under the impression that she

was pregnant, as during the past two months menstruation had been absent. On examination, an abortion was found to be in progress, and a large mass was felt in the vagina, the os uteri lying behind. The os was crescentic and distorted to the left. The ovum was expressed. After two months the tumor was removed by means of the scissors and the vulsella. Considerable hemorrhage took place. Had pregnancy gone to full term, it was probable a good deal of difficulty would have been experienced in expelling the fetus.

SUBPERITONEAL MYOMA REMOVED BY LAPAROTOMY.

DR. HARRISON also related this case, which was specially of interest from a diagnostic point of view. The patient was first seen by him eighteen months ago, at which time he could discover no neoplasm. In June last, however, he found a large mass filling Douglas' cul-de-sac, encroaching upon the rectum, and pushing the uterus forward against the pubes. It simulated an hematocele, but the history did not correspond; the patient had only suffered a short time from obscure pain. Afterwards she was unable to pass water, and it was necessary to use the catheter. The tumor increased in size rather than diminished. He then diagnosed ovarian tumor, and, as it was rapidly increasing in size, advised an early operation. Dr. Thomas coincided in the diagnosis and the necessity for an operation. At the operation the tumor was found to be a myoma, springing from the posterior face of the fundus uteri, and completely occupying the posterior cul-de-sac. Great difficulty was experienced in extracting the tumor from its wedged position in the pelvic cavity, although it had no adhesions. The abdominal wound had to be re-opened the next day in order better to secure the pedicle. The patient died of septicemia and peritonitis.

DR. BACHE MCE. EMMET referred to a case similar to the first one related by Dr. Harrison, in which he refused to remove the tumor, which was of large size and situated in front of the cervix, at the utero-vesical junction on the anterior lip of the cervix, through fear of leaving a large cavity and of subjecting the patient to the risk of septicemia. The late Dr. Peaslee also refused to operate in the case.

DR. HANKS said that he once removed a fibroid tumor from the posterior wall of the cervix, of about the size of the one presented by Dr. Harrison, and the pain and distress referred to it before its removal were very marked in that instance, being greater than in cases of tumor situated in the walls of the body of the uterus.

DR. POLK referred to a case similar to the second one related by Dr. Harrison, which illustrated the impossibility of lifting the tumor out of the pelvis. Before he got ready to remove the tumor the patient died of dysentery, which was apparently due to pressure of the growth upon the rectum. At the autopsy the adhesions were found to be so extensive that any forcible attempt at reduction would necessarily have resulted in tearing out nearly the whole pelvic viscera.

UTERINE FIBROID; INVERSION OF THE UTERUS; DIFFICULTY OF
DIAGNOSIS.

DR. ROBERT WATTS related the case of a woman who entered the Roosevelt Hospital with the history that for several months she had been losing a good deal of blood at irregular intervals, and that recently hemorrhage had been quite profuse. On introducing a finger into the vagina, a tumor resembling a mushroom was encountered, the lower portion being rounded and the upper portion presenting a distinct shoulder. After the patient's condition had been improved, ether was administered, and a thorough examination was made, *per rectum* and *per vaginam*, when it was easily determined that the uterus was inverted and a tumor was attached to the inner surface of the fundus. After apparently enucleating the tumor, reduction was attempted, but unsuccessfully. It was then found that only the shell covering the tumor had been removed. The entire growth was now easily enucleated and the uterus reduced without difficulty.

DR. HARRISON spoke of the difficulty of diagnosis in these cases, and of limiting the operation to the growth itself. The late Professor Spiegelberg found, after supposed ablation of the tumor in a certain case, that he had taken away the fundus of the inverted uterus.

DR. CLEMENT CLEVELAND believed it was maintained that this accident could not happen if the spoon-saw was used.

DR. POLK related a case which he had seen several years before, in which the cause of inversion—namely, the presence of a submucous fibroid tumor—was not recognized until after death. The patient was rhachitic, and had a deformed pelvis. The vaginal tumor presented the exact appearance of an inverted and slightly enlarged uterus; not the least irregularity could be detected on its surface. This was a point which had been noticed by several physicians who saw the case in consultation. All attempts at replacement failed. The patient died of exhaustion, and at the post-mortem the submucous fibroid was recognized. He believed that the only way in which the tumor could have been recognized during life would have been by microscopical examination, but even this might have failed.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF PHILADELPHIA.

Thursday, May 1st, 1884.

The President, R. A. CLEEMANN, M.D., in the Chair.

DR. HENRY BEATES, JR., read a report of a case of

CYSTIC LEIOMYOMA UTERI,

the fluid of which contained the *Drysdale corpuscle* and other characteristics of typical ovarian cystoma. The early history of this case, owing to the inability of the patient to state with preci-

sion a few minor details, is somewhat incomplete, yet sufficient accuracy is available to supply a clear unequivocal clinical record, and distinctly demonstrate that the Drysdale corpuscle, where found in considerable number in fluid derived from the abdominal cavity, is not pathognomonic of cystoma ovarii.

Mrs. L., æt. 51 : housewife; a mother, noticed, four years prior to admission to the Philadelphia Lying-in Charity Hospital, a lump in the lower abdominal region. The menopause had not yet occurred. The tumor was round, painless, and occasioned no inconvenience. Its development was comparatively slow, and not until it had attained large dimensions did it occasion constitutional derangement. When admitted, the patient was markedly asthenic, and suffered from mechanical dyspnea, gastric irritability, sub-acute proctitis and pyrexia, the temperature ranging from 99° to 100.5°.

The facies ovariana and the peculiar atrophy of the soft tissues of the supra-thoracic region, so constant an attendant upon ovarian cystoma, were present in conspicuous degree. The notes of measurement are unfortunately lost, but an estimate of the size can be formed when it is remarked that the tumor completely occupied the abdominal cavity, everting the xiphoid cartilage and inferior ribs, bulging far over the lumbar regions, and extending over the pubes down and between the thighs. The circumference at its largest portion was about fifty-one inches. The superficial abdominal veins were conspicuous, and the cellular tissue from the mammary zone to the feet very edematous. Palpation and percussion detected and revealed signs of ovarian cyst. The fluctuation was more perceptible in the longitudinal than in the transverse diameter. In the sub-hepatic region a decided resistance to pressure was noticeable, dependent upon a thickening of the cyst-wall. This mass was attached to the liver at the outer two-thirds of the inferior border. At the inner third there intervened a space which emitted a percussion tympany, the note being that of colic resonance. The urine afforded negative evidence. As the asthenia was so profound, it was deemed advisable to tap the cyst, in order to obtain an opportunity of improving the general strength and rendering ovariectomy bearable. Dr. Albert H. Smith, who had charge of the case, tapped, employing the ordinary curved trocar and canula used in tapping, per rectum, the urinary bladder, and withdrew twenty-seven pints of a dark, muddy-brown, grumous fluid possessing a neutral reaction, sp. gr. 1.018, and containing blood and paralbumin. Microscopically, I found red blood-corpuscles, leucocytes, endothelium in various degrees of retrograde metamorphosis, *i. e.*, the corpuscles of Bennett, Nunn, and Gluge, the ovarian cell of Drysdale, cholesterolin, and amorphous detritus. It did not coagulate spontaneously. These micro-chemical properties, coupled with the physical signs, determined a diagnosis of ovarian cyst. The grumous character of the fluid and the existence of the sub-hepatic mass inclined to the belief that we were dealing with a cyst in which carcinosis had become established, and that either metastasis to the liver occurred, or there co-existed carcinoma hepatis. The idea of ovariectomy was abandoned, and analepsis instituted with the effect of improving the general condition sufficiently to enable her being removed to her home, where she remained under the care of Dr. L. Brewer Hall. In about five months the tumor had refilled, and during

my absence Prof. James B. Walker, at Dr. Hall's request, tapped the second time. The fluid was clear, and presented the appearance of ovarian fluid. In the winter of 1882 I tapped a third time, employing an aspirator. The gentlemen assisting me inadvertently applied the exit nozzle of the air-pump to the vacuum-jar and when the trocar was introduced there occurred an inflation of the cyst. The air apparently inflated a series of various sized cysts that were arranged circumferentially, and emitted percussion notes of different pitch. From this circumstance, which occasioned no evil consequences, we concluded that there existed a number of small cysts communicating with the principal. As the evacuation of the fluid progressed, the umbilical region sank in, disclosing a circumferential mass, presenting the appearance of a wreath, as it were, underlying the abdominal parietes. This ridge could be firmly grasped and moved to a limited extent. This peculiarity determined me to carefully examine the fluid. I also had Dr. Formad examine it, and he pronounced it ovarian. The class at the University of Pennsylvania was supplied, and the ovarian cell of Drysdale demonstrated. The cyst refilled more rapidly, and I tapped a fourth time, removing a large bucketful of clear fluid, containing the same corpuscle in greater number than the previous specimens; large flakes of coagulated lymph were also evacuated. The circumferential mass had undergone great development, as had also the sub-hepatic induration. The re-accumulation of fluid was more rapid and the deterioration of health steadily progressing, death terminated suffering about six years after the first manifestations. During the last days of the patient's life, Prof. Walker, in the absence of Dr. Hall, was in attendance. His letter details the mode of death and results of the autopsy. "Mrs. L. died on the Sunday night after your departure. Her bowel trouble rapidly disappeared, but the symptoms of cerebral anemia deepened, with hallucinations and convulsions until death.

"At the autopsy the tumor was found to have membranous walls over an area of nine square inches above the navel in the middle line, elsewhere the cyst-wall was thick as per sample. In some localities, notably in the hypochondria, the wall was quite thick. Firm attachments existed over the anterior wall of the sac with the parietal peritoneum, and the intestines were carried far up under the liver and stomach, and were attached to the tumor. The liver was also attached. The entire abdominal cavity was occupied with the tumor, and it dipped into and occupied the pelvis. No attachments existed at the sides nor behind. The uterus was forced downward. One ovary, the right, was normal, and attached loosely to the pelvic brim. The other is presumably occupied by the tumor. The cavity of the tumor was filled by a brownish gelatinous fluid resembling soft soap; it was transparent, but had a sediment consisting of detritus from the inner wall of the sac. There was but one cyst, and the entire interior was similar in its lining, being apparently undergoing erosion. The contents of the cyst measured over a Yankee bucketful. As the tumor was too immense to even contemplate removal, and as the variably thick wall was everywhere similar save in the pelvic portion which was darkened from hypostatic congestion, I removed the uterus and the portion of the tumor immediately surrounding it, extending the section through the cyst-wall." (This specimen was presented to the Society.) Sections for microscopic study were

prepared from different portions of the cyst-wall and were all demonstrative of leiomyoma. Those from the sub-hepatic portion resembled spindle-celled sarcoma, but were clearly differentiated by the elongated nuclei and want of sarcomatous relationship of cells to capillaries. The identity of the clinical phenomena of this neoplasm with those of ovarian cyst is a matter of special attention. In the early stage, a marked peculiarity consisted of the sub-hepatic enlargement and induration; a morbid resistance to pressure noticed in the inferior lumbar regions after the first tapping, and attributed to the edema, is now seen to have depended upon a thickening wall, which at that time was insufficiently developed save in the hepatic region to attract special attention. That all doubt regarding the nature of this neoplasm may be removed, attention is directed to the left ovary, which has undergone marked atrophy, and is to be seen in the specimen. I submitted some sections to Dr. Formad, who pronounced them ovarian. The origin of the cyst from the fundus uteri is evident upon examination. With the facts before us, I think it is conclusively proved that the ovarian corpuscle of Drysdale, while a valuable aid to diagnosis, does certainly not possess pathognomonic value.

DR. DRYSDALE regretted that Dr. Beates had not sent him a specimen of the fluid removed from this tumor, especially as he had more than one opportunity of doing so. While having the highest regard for the opinion of the gentlemen who examined it, still so many errors had been made in these investigations that it would have been a satisfaction to him to have examined it himself. But, apart from this regret, he considered it by no means proven that the cyst in question was not ovarian. The portion of tumor left attached to the uterine wall in the specimen resembled a closely adherent ovarian cyst such as he had met with repeatedly. The little mass lying close to the uterus, described as an atrophied ovary, did not present any resemblance to an ovary, nor did it occupy the usual position of that body. In the description, no reference had been made to the color of the tumor, which has an important diagnostic value, the uterine fibro-cyst being usually livid or purplish in color, while the ovarian had a white, pearly hue. It was especially in cases like that of fibro-cyst of the uterus where the importance of the ovarian cell in diagnosis was well-marked. In his investigation of these tumors he had never met with the cell which he had described as ovarian. Mistakes were very easily made in the differential diagnosis of such tumors, and, in fact, in many cases the diagnosis could not be established except by the examination of the fluid or by opening the abdomen. For want of this examination of the fluid he had seen Spencer Wells make the abdominal section to remove a tumor which he had diagnosticated as ovarian, but which proved to be uterine. Dr. Marion Sims had sent him on three different occasions, and without telling him that they were from the same patient, specimens of fluid, which he thought was ovarian, obtained from a cyst in the abdomen. Dr. Drysdale assured him that the fluid was not ovarian; but, after the examination of the last specimen, Dr. Sims, still doubting, determined to operate, and found a uterine fibro-cyst. The history and all the characteristics of the tumor described this evening were ovarian, and the specimen and autopsy were not sufficient to establish the diagnosis of uterine cyst.

DR. B. F. BAER remarked that the specimen seemed to be a section of a fibroma arising from the uterus, but it is too small to be satisfactory or to prove its origin. His personal experience has taught him the diagnostic value of the Drysdale corpuscle. When Dr. Formad reports finding this cell in the fluid removed from an abdominal tumor, he feels strengthened in his diagnosis of ovarian cyst. He has not trusted to the cell alone, but has been greatly influenced by its presence or absence in making up a diagnosis in doubtful cases. In every instance in which Dr. Formad has reported finding the ovarian cell, operation has proved the tumor to have been of ovarian origin. He would like to ask Dr. Drysdale if he still considers the cell pathognomonic.

DR. DRYSDALE still believed in the pathognomonic value of the ovarian cell. In his investigations he had met with but one exception to the rule, and that was in renal cysts. To prove that the cell could be relied on to establish a diagnosis, he would give one or two instances where it was impossible to do this except by its aid. In a review by Dr. Harris of the Transactions of the American Gynecological Society in the *American Journal of Medical Sciences* will be found this statement: "On one occasion, Prof. D. Hayes Agnew gave Dr. Drysdale a fluid for examination in which he found the characteristic cell. Upon stating what he had found to Dr. Agnew, he was told that the fluid had been taken from the abdominal cavity, upon which he immediately said that the fluid must have escaped from an ovarian cyst, for it was ovarian. In this he was correct, as the cyst had a small hole in it, as if made with a punch, and the fluid had escaped as claimed." In another case, Dr. D. received a letter from Prof. Matthew D. Mann, of Buffalo, N. Y., with a specimen of fluid consisting of eight or ten drops, which the doctor stated was all that he could obtain by aspiration. The tumor had been diagnosed by other surgeons as one of uterine fibroma, and consisted of a large solid mass which filled the pelvis and abdomen as high as the umbilicus. The history and symptoms all pointed to a uterine fibroid, and the patient was in such a precarious condition that an exploratory operation was considered unjustifiable. An examination of the fluid by Dr. D. showed the presence of the ovarian cell. This determined Dr. Mann to operate. He found two ovarian tumors which he removed successfully, and the patient recovered: "without the microscope no certain diagnosis could have been made except by resorting to an exploratory incision." These cases—and he could give many others like them—were sufficient to show the diagnostic value of the cell.

DR. BEATES remarked that it was a matter of extreme regret to him that Dr. Drysdale did not have an opportunity of examining the fluid: it was due to the fact that the neoplasm was regarded as ovarian and the specimens of fluid were not preserved. At the autopsy, the growth was presumed to have arisen from the left ovary, and none of the fluid was kept. Later study of the specimen disclosed the amygdaloid mass situated in an atrophied membranous structure closely approximated to the uterus. The microscopic examination of this determined its ovarian character to the mind of Dr. Formad, which conclusion dissipated a doubt in my mind, and confirmed my belief of its being the left ovary. I thoroughly appreciate the strong probability of error liable to occur in positively diagnosing by differentiation the Drysdale corpuscle from similar bodies, as the pyoid body of Lebert, but the

fact that treatment with acetic acid only had the effect of rendering the whole corpuscle very slightly clearer and disclosed no nucleus: that ether added to the fluid and the mixture thoroughly agitated for several minutes had the effect of simply rendering the corpuscle paler, convinced me that the bodies were the corpuscles in question. There is, by very extended experience, developed a capacity to differentiate by a varying degree of opacity. Dr. Formad believed these corpuscles to be those of Drysdale. The striking phenomenon, if this be an ovarian cyst, is in the fact that primarily it was purely cystic, typically so, and that during the last year of its existence the cyst-wall throughout its posterior seven-eighths assumed a myomatous development. If this did not occur, the myomatous wall must have originated at the fundus uteri, and gradually permeated the cyst. Either of these processes is almost incredible and certainly exceptional to known clinical facts. The true uterine origin must not be forgotten. That a proper conception of the tumor may be formed, it should be compared to a large pumpkin with a wall varying in thickness from one to three inches. For an area of about nine square inches at the umbilical region, the ordinary cyst-like structure formed, as it were, a drum head. This wall is not fibromatous, but purely myomatous. Dr. Goodell had seen this patient, and diagnosed the tumor as ovarian.

DR. B. F. BAER exhibited a specimen of

HYPERTROPHIED UTERINE MUCOUS MEMBRANE.

R. H., æt. thirty, married twelve years, sterile; puberty occurred at age of twelve, slight dysmenorrhea from the first, and since her marriage the difficulty has been increasing, so that during the last few years the pain has been very severe. The menstrual flow, which has always been rather profuse, especially since her marriage, has for more than a year been irregular in time and quantity: sometimes it continues two weeks very freely, when she would be so prostrated as to be compelled to remain in bed to regain strength. She complained of a severe, sharp pain in the region of the left ovary radiating to the groin and anterior part of the thigh, and to the precordial region and side of the head to the top-head. She had great dragging in the pelvis and pain in the sacral region. During her periods the mammary glands would swell and become very tender and sore. Coition had been rendered almost intolerable on account of pain during the act, and because it increased the pain in the left ovarian region and induced a sensation of nausea and faintness. She had such dread of sexual congress that an interval of months would sometimes elapse between the acts. Her weight had decreased from 146 to 117 pounds, and her appetite and digestion were poor.

Examination showed the cervix uteri to be near the vaginal orifice, somewhat elongated and conical. The os was patulous, the body of the uterus very much hypertrophied and retroverted. The left broad ligament was contracted and the corresponding ovary prolapsed, larger than normal, and very tender to pressure. The sound indicated a uterine depth of three and one-half inches and

the cavity was large and soft. The organ was mobile. Ether was administered, the cervix dilated by means of Ellinger's dilator, and the endometrium was carefully curetted, removing a large amount of the most enormously hypertrophied mucous membrane. Nitric acid was applied. Under a regulated diet with rest, complete relief followed, with freedom from hemorrhage and pain.

DR. W. H. PARISH would like to hear the result in Dr. Baer's case after the lapse of three or four years. Two or three years ago he had reported before this Society a similar case in which, after dilatation by sponge-tents, he had removed a large quantity of endometric growths, and applied nitric acid. The treatment was followed by an apparent cure which lasted for some months, after the lapse of which the previous condition returned. The same treatment, followed by relief and subsequent relapse, has been repeated several times. Good microscopists have pronounced the growths benign. Dr. Goodell has, however, given it as his opinion that it will ultimately become malignant. Dr. Parish has been gradually coming to the same opinion.

DR. BEATES has treated a woman, æt. thirty-three years, who suffers from anteflexion of the uterus, menorrhagia, and granulations of the endometrium. The microscopic appearance is benign. After treatment by means of the curette and nitric acid no hæmorrhage occurred for four months; the treatment was repeated, a laceration of the cervix was closed, and seven months later the patient became pregnant, abortion occurred at two and a half months, and the granulations and hemorrhages have returned.

DR. BAER remarked that these cases are very common; they are seen every week at the clinic. They are usually benign, but sometimes become malignant from loss of blood and a run-down condition of the system. Adhesions or some other obstacles to the free return of the venous blood from the uterus may exist, or the ovaries may be diseased, and these causes will bring on the relapse, no matter how perfect the relief may be. In many cases the cause is flexion, the effect is sterility. It is an exaggeration of a purely physiological process. It may be benign in its incipency, but may become malignant later on.

TRANSACTIONS OF THE OBSTETRICAL AND GYNECOLOGICAL SOCIETY OF WASHINGTON.

Stated Meeting, held April 18th, 1884.

DR. S. C. BUSEY, *President, in the Chair.*

The subject for discussion was

THE MANAGEMENT OF THE PUERPERIUM,

and was opened by Dr. S. C. Busey (Vice-President Dr. J. T. Johnson in the Chair).

DR. BUSEY said his selection as the first speaker was probably due to the fact of his being the senior in practice, and having had

a longer experience; also, because it was supposed he had seen more of the changes and advances made in the obstetric art than any other member. He still adhered to much that was taught in the University of Pennsylvania by Hodge, and to the details demonstrated by Warrington in the Obstetric Institute of Philadelphia.

The methods then taught were antagonistic to those employed by many practitioners, and he came to Washington with advanced ideas, and was perfectly drilled in the details of the lying-in room.

The question to be discussed seemed, at first sight, very simple, yet close examination showed it to represent a vast field with numerous details. For this reason his remarks would be a mere narration, as it was impossible to reduce these details to definite limits. He would define the puerperal period as beginning at the end of the third stage of labor, and ending at the close of the fourth week. Viewing it in this respect, the question was, Should we discuss the condition and treatment of the mother alone, or also include the management of the infant? He would confine his remarks to a description of his individual experience and methods of conducting the woman safely through the puerperal stage, *i. e.*, through the retrogressive and developmental changes occurring after-labor; the means adopted to guard her against complications; to protect her from accidental conditions; to prevent trouble, and to favor the processes of nature as far as possible. By care and attention he could surmount most, if not all, the difficulties likely to arise after a natural labor. We must bear in mind the conditions existing during pregnancy; ascertain whether or not hygienic conditions have been observed; remember the influence of age; pay attention to the appetite of the patient, the condition of the bowels, the character of the food, to the exercise she has taken; see to it that the functions of the skin, lungs, kidneys, etc., are normally performed. Was the woman a primipara or a multipara; and was the labor natural or preternatural? would be the next questions. These considerations opened up a wide field for the exercise of experience, discretion, and observation.

But suppose that the third stage of labor has been completed, and the womb well contracted, then the first thing to do is to apply the binder. This is of the utmost importance. The binder preserves the figure, fixes the womb, keeps up pressure, and obviates repletion of the abdominal vessels. It is not only a sanitary measure, it is a remedy to protect the woman from serious complications. He always uses it and has had no experience without it.

The next thing was to cleanse the patient's body from blood and other discharges; remove all bloody cloths, and the clothing, if soiled; in fine have everything as clean as possible, *i. e.*, antiseptic. Ergot is sometimes called for by hemorrhage or imperfect uterine contractions. He was in the habit of measuring a dose of the fluid extract of ergot, and leaving it with the nurse, with instructions when to give it. He then had the patient put to bed and let her go to sleep. The baby was put to the breast when the mother had recovered from the exhaustion of the labor. He preferred to give the patient broths during the first hours after labor. Before leaving, he counted the pulse and made pressure on the uterus. The pressure, indeed, gave pain, but saved trouble by preventing after-pains; but, if the latter came on, he left a prescription to relieve them. The time thus occupied was from one hour to one hour and twenty minutes; and he never left this patient sooner. His next visit was made as early as convenient during the same or the fol-

lowing day. At this visit, he investigated the case thoroughly, took the temperature, counted the pulse, noted the condition of the tongue, the bladder, womb, ascertained the character and amount of the flow, examining the napkins himself for that purpose. Special attention was paid to the amount of urine passed. Here we were apt to be deceived by nurses who would look upon a dribbling as a full flow while sometimes retention existed. He had seen a case where death resulted from just such a condition. The case was seen in consultation on the fourth day after labor, and the physician in attendance reported that the patient had urinated freely, yet Dr. Busey drew off a quart or more by the catheter.

If all was found to be satisfactory at this visit, he directed vaginal injections of a one-per-cent solution of carbolic acid to be given as often as he deemed them necessary. And in directing them, he inquired whether the nurse knew how to give them, and if not, he taught her. He generally ordered a quart of hot water, preferring hot to tepid or cold. He also satisfied himself that the syringe was a safe one.

He would assume that the baby had been put to the breast, and this brought up the importance of taking care of the nipples before labor, neglect in this direction being very apt to lead to trouble. He never permitted the baby to have artificial food during the first two days. If it needed fluid, he gave water. When he first started in practice, the custom with nurses was to give the baby goose-oil or sweet oil. He never saw any reason for this practice and did not permit it.

The diet of the mother should be liberal and of a liquid form: no toast and tea, but eggs, soft toast, etc., until the bowels were moved, which usually occurred on the third day. He interfered sooner, if headache, flatulence, or a foul tongue existed, and held that a liberal diet promoted the early evacuation of the bowels. After the movement of the bowels, he allowed the patient to have solid food, such as chops, steak, fish, oysters, etc. He avoided all fermentable substances and acids. All this, of course, referred to a normal case, and the plan sketched out was pursued to the ninth day, the patient, as a rule, not rising before that time. This rest for nine days had been inculcated in his practice by Hodge and Warrington. He afterwards departed from this custom, but had good reason for returning to it again. The longer the patient was kept in bed during the puerperal month, the better would be her getting-up. The retrogressive changes took place better during this period of rest. He gave intrauterine injections for cause only, while the vaginal injections were kept up for a week or longer. To keep the bowels open once a day, there might be a necessity for drugs, but usually warm enemata only were needed. On the third day, he expected a little fever, which might, however, come on as early as the end of the second day, or be deferred to the fourth. This was the period when we become apprehensive of possible complications and look for something more serious than the ordinary milk fever. There might be a chill, and this occurred more frequently in primiparous cases: hence the importance of taking the temperature so as to make sure that we are dealing with ordinary milk fever. If the cause was suspected to be in the breasts, they should be carefully examined. If these were found to be painful and tender, and if the child refused to take the nipple, or the mother could not support the pain of suck-

ing, manipulating the breast might evacuate the milk; but a better method was that of emptying the breast by the mouth of the nurse. If tender, saturnine lotions with opium were applied by means of a soft pad. If the soreness and swelling were circumscribed, local hot-water compresses, with absorbent cotton, were the best remedies. But the great point was to get the breast emptied. Dr. Busey showed a woodcut of a nipple shield which was the only one he had found serviceable. It was made of glass and soft rubber, just fitting the nipple. Fissures of the nipple were best treated by comp. tinct. of benzoin. If abscess formed, it should be opened at once. In mechanical obstruction, means should be taken to arrest the lacteal secretion, of which the official ointment of the lead iodide gently rubbed on the breast, together with a saturnine lotion, was the best. Dr. McArdle could bear witness to its success in one case seen in consultation.

After the ninth day, the conduct of the case became more simple. Exercise, sleep, rest, diet, etc., should be properly regulated. The mother must have the proper amount of sleep; hence, the baby should be nursed late at night, so as to afford at least five hours of uninterrupted sleep to the mother, and this practice should be inculcated at once, for, by so doing, patient and physician are saved a great deal of trouble. Nurses were too prone to break the discipline of the lying-in chamber; in fact, all of them spoil the babies when given a chance. Babies were not naturally bad; their badness was due to bad management. The room allotted to the mother and child should be absolutely clean, well lighted and ventilated, and all unnecessary upholstery should be banished from it. Quiet should be maintained, by keeping the mother and child free from visitors, none being admitted during the first week. It was not necessary to preserve absolute silence, but the physician should not make his visits too long, or sit and chat; he should make the necessary investigation and go about his business. All depressing or exciting influences should be kept off; no bad news, such as ladies were fond of communicating to the lying-in woman, should be permitted to reach her. Above all, there should be a competent nurse. This rule was absolute with him, and if he found the nurse not fully competent, he had her discharged at once; and in this matter he never hesitated, as soon as he discovered that the nurse was not doing her duty, or disobeying or deceiving him. The best nurse was the one who strictly followed the directions laid down for her, and was able to give an enema or, if need be, use the catheter; and not the one who was in the habit of administering medicines to mother and child of her own volition. The old grannies of twenty years ago were fond of giving remedies, and he had seen bad results from their interference. It is a good plan to get definite information about the nurse beforehand, and also about the patient's habits during pregnancy, as such knowledge might prove useful in the conduct of the case. Dr. Warrington saw to all these details as just recited, and in the service of the Obstetric Institute personally superintended the students in carrying out his rules. At that time, Dr. Busey thought that too much attention was paid to these details; but experience had taught him they were correct, and he now thanked Dr. Warrington for having trained him as he did; for he believed that his good fortune with his obstetric cases was mainly due to his early training, for he had had

but one case of death in the puerperal state where he had the exclusive management of the patient from the beginning.

In conclusion he said that he had simply endeavored to sketch his method of dealing with the puerperium, and hoped that other points not referred to would be taken up by those to follow in the discussion.

DR. C. E. HAGNER agreed with Dr. Busey as to the use of the binder, and always applied it, although some physicians had discarded it; but he desired to speak especially of a method of applying it which he had learned from the late Dr. W. P. Johnston. A woman during labor would not object to exposure or to be touched by the physician's hand, but she, generally, did object after labor. To prevent exposure he laid a napkin over the abdomen of the patient, spreading it like an apron, and then drew the binder over it and pinned it, thus proceeding neatly and without exposing the patient's person. As to the management of the breast in case of irritation, he objected to pressure and manipulation. The best plan was to put a large breast into a sling as soon as the milk made its appearance. This prevented dragging. He used two broad bands crossed in front of the thorax, the lower ends passing under the axilla and meeting those passed over the shoulder. After convalescence, he prohibited the use of the sewing machine for at least three months. In a case under his care, the use of the sewing machine six weeks after labor induced uterine hemorrhage. He also prohibited sexual intercourse for some time.

DR. SMITH felt under obligations to Dr. Busey for pointing out the importance of inquiring into the conditions antecedent to labor. Many cases of disease had been produced through neglect and inattention to the secretions and excretions during pregnancy. Many women entered on labor with the lower bowels filled with fecal masses, as all know from experience, and would suffer from the effects of this condition after labor. He believed that the fever of the third day was due oftener to the condition of the bowels than to the milk. He rarely sees this fever now, although in his early practice it was his custom to expect it. Always gives a dose of oil on the third day unless the bowels move sooner, but if attention is paid to this matter during pregnancy, the bowels will continue to act regularly after labor. Sometimes a threat to administer a dose of oil if the bowels are not moved by a given time, acts well by creating an impression on the nervous system. Did not believe in the use of carbolized vaginal injections unless there was a reason for giving them. This was not orthodox, but he believed the bulk of practitioners were with him in this respect. He always removed the soiled clothing and pads as soon as possible, for if women are permitted to lie too long in the mess, great danger is entailed from absorption of putrescent matter. The bed should be well ventilated as well as the room. The necessity for this was apparent, for the raising of the bed-clothing permitted an offensive odor to escape, which was readily detected. By folding the bed-clothing longitudinally on the side of the bed not in use, ventilation would be insured while the patient occupied the other side. Dr. Busey's remarks on the police regulations for the sick-room were excellent.

DR. TARKINGTON inquired of Dr. Busey what he gave to relieve after-pains.

DR. BUSEY.—Morphia in camphor water and solution of acetate of ammonia.

DR. BROMWELL attributed his own success in obstetric practice to attention to the details as laid down by Dr. Busey.

DR. J. T. JOHNSON would like to emphasize a few points. Too much attention could not be paid to the condition of the woman during pregnancy. The habits of our women during pregnancy differed from those of all other civilized countries. They disliked to expose themselves in the streets, and thus failed to take sufficient exercise in the open air, while in other countries they walked about freely. As to the care of the breast previous to labor, he used alum and brandy to toughen the nipple, especially in primiparae. There was one point of importance not mentioned by Dr. Busey; this was the post-partum chill. It was due to the difference between the internal and external temperature of the woman. Labor being really what its name implied, labor, the woman was often fanned to keep her comfortable; after labor this cooling-off process showed its effects, producing the chill. Sometimes we meet with a pulse as low as 50 or 75 after labor, and when he first saw it he became alarmed, but experience had taught him that a low pulse immediately after labor was a favorable omen for the patient, and he now looked upon it as a natural condition.

With regard to the binder, he held that it was abused. In the Vienna lying-in wards they simply used a sheet previously baked in an oven, and applied warm. The binder did harm when applied so as to press the uterus against the vertebral column. It should be pinned tightest above so as to keep the uterus from rising in the abdominal cavity. He thought it generally did as much harm as good. With reference to Dr. Smith's plan of ventilating the bed, he cited the old practice in Bellevue Hospital of delivering the woman on what was called a "pony bed," and afterwards removing her to the bed she was to occupy during the puerperal period. To obtain sleep was of the utmost importance. If the baby cried, it should be removed to another room, so as to insure quiet to the mother. He favored the early nursing of the baby, and insisted on the need of good nourishing diet for the mother, to counteract the exhaustion after labor. He objected to feeding the child before the milk appeared, and said if the good Lord had intended that the baby should be fed at once, He would not have the milk appear as late as the second or third day. The temperature was, in a measure, kept up by a binding of the bowels, but he did not give castor oil, which had a tendency to act injuriously in cases where piles existed, but preferred some other laxative. He did not approve of keeping the patient in bed too long, as the recumbent posture favored retroversion, and the collection of discharges in the vagina.

DR. MCARDLE said he could corroborate the efficacy of the method given by Dr. Busey in the drying up of the milk. A mammary abscess was a terrible complication, and he had heard women say that they would rather have three children than one abscess. Rather than have an abscess occur, it was better to dry up the milk and feed the child from the bottle.

DR. MAGRUDER said that the iodide of lead ointment was a favorite in his practice. He also spoke highly of the belladonna plaster, which acted as an anodyne to the painful breast, and at the same time arrested lacteal secretion, and cited a case proving its efficacy.

DR. BUSEY, in closing the discussion, commended the suggestion of Dr. Hagner, to sling the breast, as very valuable.

He had long since abandoned castor oil in his obstetric practice. Years ago, when his cases were chiefly among the poor, he could force them to take it; but now, with a higher class of patients, he found he could not coerce them to take the nauseous dose. It was sure in its action, but generally caused pain. He generally permitted the patient to take the laxative she had been in the habit of using. He had no special time for opening the bowels, but, as a rule, waited until the third night. Referring to Dr. Smith's non-use of vaginal injections, he thought that the fact spoken of in relation to ventilation of the bed, showed the necessity for vaginal injections so as to remove the putrescent matter which caused the offensive odor referred to. As to the want of exercise, referred to by Dr. Johnson, his rule was to make his pregnant patients take exercise, walking being preferred, believing that young women, who did take exercise, had less trouble than those who lolled about the house.

He looked upon the post-partum chill, spoken of by Dr. Johnson, as suspicious, and always carefully examined into it, lest it might be due to some more serious cause. A slow pulse was also suspicious, and thought one of its causes was incomplete application to the binder favoring a flow of blood to the abdominal veins. If might also be due to shock. A frequent pulse within the first hour was highly suspicious. Dr. Hagner's method of applying the binder was a good one, but Dr. Busey always applied it himself and never allowed the nurse to perform that duty.

He did not believe in three and nine months' colic in babies; it was due to mismanagement. Generally the outcries were due to under- or improper feeding. In cases of severe colic, a drop of chloroform afforded relief. As to rest in bed, he did not wish to be understood as keeping the woman on her back, he simply required her to stay in bed, but permitted her to change her position and raise her shoulders on pillows. He did not rely on belladonna to arrest mammary secretion, and thought simple adhesive plaster as good as belladonna plaster. He had used the tincture internally and the extract externally, and failed. Iodide of potassium was better. As to the means of increasing lacteal secretion, the best was abundance of good food. He had tried electricity, which had restored the secretion, but he had been forced to abandon it because it set up inflammation. Castor bean had also been used, and fomentation by the leaves, but without satisfactory results.

The early return of the menses was disastrous to the nursing child, the vast majority of cases of rickets being traceable to this cause. Early and frequent sexual intercourse also, while not having as marked an effect, still deteriorated the milk.

REVIEWS.

ETUDE SUR LES LOCHIES DANS L'ETAT NORMAL ET LES ETATS PATHOLOGIQUES. PAR LE DOCTEUR EUSTACHE.

A STUDY OF THE LOCHIA UNDER NORMAL AND PATHOLOGICAL CONDITIONS. By DR. G. EUSTACHE, Professor of Clinical Obstetrics and of Gynecology at the Free Faculty of Medicine at Lille, etc., etc. Paris: A. Delahaye and E. Lecrosnier, 1884, pp. 136.

The author of this essay, after a few general considerations on the lochia, describes the rôle which they play under both normal and pathological conditions. The main question he attempts to answer is whether the lochia enter as a pathogenetic factor in the production of puerperal affections. M. Eustache is a believer in the septic origin of these affections. He cannot grant the existence of an essential puerperal fever. He considers the temperament and diathetic conditions of his patients powerful factors in insuring a normal puerperium. The less the resisting force of a given patient, the greater her liability to some one of the many complications which may ensue after any delivery, however simple. In his opinion, when the puerperium takes an abnormal course, this is not due to "the presence of a micro-organism which has penetrated into the system of the lying-in woman from without;" on the contrary, the pathogenetic factor results from a dynamic modification of the system itself, which, under given conditions, gives rise to one or another complication, whose manifestations are at times connected with the general condition of the patient, and again with certain local conditions." Germs, hence, play but a secondary part in the production of puerperal affections. And why? Because the germs which are found in numbers, as well in the normal as in the pathological lochia, exist there in great part as the result of the breaking up of the liquids and solids of the organism itself. These germs are in themselves, therefore, harmless; they may become harmful, however, under certain conditions of the organism, conditions engrafted on it by, at times, hospitalism, again physical wear or mental distress, again local lesions. Such is the gist of our author's argument. He has reached his conclusion after making nearly three thousand examinations of the lochia from two hundred and ninety lying-in women. He has found the micro-organism in many forms uniformly present under the most normal as well as the most pathological states. In part, these organisms come from the surrounding atmosphere, especially after manual or instrumental interference, and they develop and multiply in the lochia; in greater part, however, the origin of the organisms is to be traced to the change of the elements of the blood and of the epithelial cells of the uterus and vagina into molecular granulations, which are living organisms, or rather "the germs of organisms which, under certain conditions, acquire an intense vitality, with the power of acting either as ferments or as agents of organization or disorganization. This is the theory which Béchamp propounded, and which M. Eustache accepts.

This origin, then, of puerperal affections is strictly from within the system, it is not necessary to go outside in search of an infectious agent. The lochia play but a secondary rôle in the production of disease. They simply contain micro-organisms which, under certain given conditions of the system, may become noxious, and, in addition, they may receive from without an accession of organisms which, under like conditions, may also become harmful. The corollary as to treatment is to place the woman about to be confined amidst such surroundings and in such a healthy systemic state as will prevent the implanting of noxious properties into these organisms. If, notwithstanding, she becomes sick, the lochia alter, and the contained micro-organisms acquire irritative and infectious properties, the aim of all treatment should be to prevent the accumulation of these organisms and to modify their irritative properties as soon as possible. This is to be accomplished by means of antiseptic vaginal and uterine injections. In short, the object of treatment is to modify the ferments arising from within the system, not to attack atmospheric germs.

The most that can be said of this essay is that it presents a logical argument in favor of the author's views. Where so much is theory, however (and as yet it would be premature to claim the cause of those affections to which the lying-in woman is subject as definitely settled), it would not be wise to dogmatize as to the truth of M. Eustache's conclusions. It has simply been the aim here to give a résumé of his views touching a subject of such interest and importance. In so far as he practically rejects heteroinfection, he is at variance with the belief entertained by the vast majority of accoucheurs. His views are based on the evidence afforded by the microscope, and there are doubtless some who will question, and with justice, the stability of data afforded by a method of research which control experiments have in other supposed germ diseases shown to be fallible. For the elaboration of the argument on which M. Eustache's conclusions rest, the reader must be referred to the essay itself.

EGBERT H. GRANDIN.

SHAKESPEARE AS A PHYSICIAN. By J. PORTMAN CHESNEY, M.D.,
Ex-Secretary Medical Society of the State of Missouri, etc., etc.
J. H. Chambers & Co., 1884.

Whilst the marvellous versatility of Shakespeare has ever been granted, it is questionable if the average reader of his works has any idea of the extent of his knowledge in matters medical. In the present work Dr. Chesney aims to supply the void by gathering into short compass the medical thoughts and phrases which occur in the complete works of Shakespeare. To limit our remarks to the pages devoted to obstetrics, the author accomplishes his aim right well. Shakespeare is shown to have been familiar with a variety of topics connected with the phenomena of pregnancy, labor, etc., though, of course, this familiarity was of the superficial kind, such as the average untrained midwife possesses to-day. So far, then, these pages are purely matters of curiosity and not at all open to criticism. The work subserves still another purpose, however, it gives Dr. Chesney an opportunity to collate his own knowledge of obstetrics as well, a knowledge which appears to be of a varied nature, in certain respects rather peculiar, in others a trifle crude. We find, for instance, in connection with appropriate or suggestive quotations from Shakespeare, his views in regard

to abortionists; his plea in favor of satisfying the longings of pregnancy because, whilst not open to proof, it may be that "the strange and unusual articles of diet sometimes so longingly sought by the mother do contain ingredients essential to the elaboration of some of the tissues of a new being" (!); a résumé of the chief causes of miscarriage, to prove that Shakespeare in making fatigue and worry a cause, "*might* have been correct," "but then the chances are as one in a thousand he *might* also have been mistaken;" a brief lecture on teratology, suggested by the statement that Richard the Third was born with teeth. We might multiply instances, but enough has been said to make good our statement that Dr. Chesney's knowledge is of a varied nature. As an instance of the peculiarity of this same knowledge we could note the elaborate argument he makes in favor of his opinion that infants should be nursed for nearly three years, even as the nurse in "Romeo and Juliet" tells us Juliet was. He agrees with the nurse and protests against "the murderous habit of depriving the babe of its natural aliment at an earlier age" as is the custom today. Reference to the woodcut on page 59, illustrates this marvellous retrograde step Dr. Chesney would have us take; for there is pictured the infant (certainly of three summers) standing on tip-toe to obtain its nutriment from the over-distended breast the mother gracefully offers. As for the crudity of Dr. Chesney's knowledge, we could simply refer to page 40, where the statement is made without qualification, "there are no means yet known to the medical world by which pregnancy can be *positively* known." Before reading this statement, we had at times felt positive as to the existence of pregnancy in a primipara even in the early months, to say nothing of the certainty granted in the later months by the fetal heart. If we accept Dr. Chesney as a guide, however, we must give up even the fetal heart, for on the same page he tells us that "the click of the fetal heart, at a later period, is of some value." One word more, and this to give an instance of Dr. Chesney's style, which, often involved and ungrammatical, occasionally verges on the obscure. On page 35, when speaking of old age, he says, "Asperity is not, in general, a concomitant of this period of human existence—a pensive realization of the fact that the spring-time of life has passed—the seed has been sown, the hey-day of existence has been reached and the harvest gathered in, and the husbandman has nothing more to do but set thoughtfully by through the autumn and winter, with his hands resting on the top of his staff, contemplating the shadows as they silently fall around him."

We could commend this work to those curious about "Shakespeare as a physician." We cannot, however, conscientiously indorse Dr. Chesney's share in the work except in so far as it is an example of patient toil and research.

EGBERT H. GRANDIN.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY . . . GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

NOTES OF A CASE OF INCONTINENCE OF URINE FROM MAL- FORMATION OF RIGHT URETER.

BY

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INCONTINENCE of urine, although so common, and usually curable in children, is still by no means undeserving of more consideration than is accorded to it. In many instances within my own experience this affection has proved intractable to medical treatment. Nor does time, that great panacea for so many of the chronic diseases of youth, here as universally affect a cure as some writers contend. In such cases this complaint may become of very serious consequence; and I have myself known instances in which the lives of persons thus affected have been rendered miserable, and their prospects of success or happiness completely marred by this unfortunate ailment.

On the present occasion, however, I desire to confine my remarks to a cause of incontinence of urine which, as far as I am aware, has not been noticed by any writer on the subject, and which is exemplified by a case now under my care.

In this instance the patient is a young lady in her sixteenth year, about whom I was first consulted some five years ago. She was then leaving Ireland, and as the complaint was only casually and imperfectly described to me when visiting another member of her family, I supposed it to be one of ordinary incontinence of

urine, and merely prescribed the customary routine treatment, viz., iron with small doses of belladonna, and blistering the sacrum, which I generally find useful in such cases. I lost sight of the patient, as she was sent to school on the continent. I learned afterwards, however, that no benefit had been derived from the treatment prescribed, and the urinary incontinence became so troublesome to herself, and so unpleasant to those about her, and exposed her to such derision from her companions, that within a couple of years she had to be removed from several schools to which she was sent successively. She was then brought over to London, where she was treated by several eminent physicians and surgeons, one after another, without any apparent result. Ultimately she returned to Dublin and was again placed in my care. As the flow of urine was now incessant, although at the same time she had power of retaining and voiding a small quantity of urine at regular intervals, I suspected some malformation, and accordingly proposed an examination. In this I had the advantage of Dr. Kidd's advice and assistance. She was placed under chloroform, and we convinced ourselves, first, that the bladder had power of remaining a considerable amount of urine; secondly, that there was no fistulous communication between the bladder and vagina; and lastly, we ascertained, after a long and careful examination, that about half an inch above the meatus urinarius there was a minute orifice, from which a small stream of urine was continually escaping, but which was almost completely concealed by the rugæ of the vaginal mucous membrane. We then traced this passage by a No. 1 catheter introduced as far as it could be passed.

The length of the canal, and the constant dribble of urine through it, were sufficient to distinguish this from either what is generally described as the efferent duct of the Wolffian body, or Gærtner's duct, or from the peri-urethral duct recently discovered by Dr. Schüller, of Berlin. The latter states that between the longitudinal folds of the urethral mucous membrane that converge towards the meatus, numerous pores and depressions may be found. On each side of the middle line, posteriorly, lies a ring of mucous membrane surrounding an orifice into which a sound of from one to four millimeters in diameter can be passed for from one-half a centimetre to two and a one-half centimetres upwards. This duct, which was found by Dr. Schüller in subjects of every age, on horizontal section appears as a slit with a contour more or less irregular, and is traceable to its termination above at a gland made up of a collection of small short tubules. In the case now reported, however, there could be no question that the orifice discovered

near the meatus was quite distinct from the duct of Schüller. This was proved by the anatomical character of the parts as well, as has been already observed, as by the length and permeability of the duct, and the escape of urine through it.

The conclusion arrived at by Dr. Kidd and myself, as the result of our examination, was that we were dealing with an instance of malformation: malposition of the right ureter. Accordingly we suggested attempting by a plastic operation to establish a communication between the ureter and bladder above, and at the same time close the external orifice of the former. In making this suggestion we, however, placed fully and fairly before the patient's friends the risks as well as the chances of success of the proposed operation. As this is still *sub judice*, I venture to bring the case forward, not only on account of the rarity of the condition described, but also in the hope of eliciting any counsel or hint which the experience of others may suggest, and by which the treatment of this puzzling and distressing case may be facilitated.

A CASE OF UNIVERSAL ERYSIPELAS IN A CHILD AGED TEN MONTHS; WITH RECOVERY.

BY

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THIS case is interesting for the following reasons: In the first place, the entire surface of the child became gradually involved, and, in the second place, as illustrating the value of sustaining treatment.

My first visit was made to the little patient on 8th of May. I was informed that the disease commenced on the day previous, as a reddish swelling on the right labium. When I saw the patient for the first time the vulva was greatly swollen, reddish, slimy, and very tender. The inflammation had also extended on to the pubes and slightly on the lower part of the abdomen.

From this time the disease marched upwards and downwards over the surface of the body, requiring about four days from its appearance at any part till it became nearly normal again in color.

When the erysipelatous inflammation extended to the feet there was very marked edema. The chest, back, and arms were also taken in turn. The neck and head were next invaded. The eyelids and lips were the last points of attack. In this manner every portion of the entire surface of the body—not omitting the palms of the hands and the soles of the feet, which had a tinge of red—was affected at some time during the course of the disease.

The local treatment consisted in frequently and thoroughly anointing the skin with the following:

Acid. carbolicæ....	gr. v.
Ext. belladonnæ.....	gr. xv.
Ung. petrolei.....	3 i. M.

Soft cloths were kept next the skin and child loosely and comfortably covered.

The internal treatment consisted in the administration of one teaspoonful every three hours, in water, of:

Quin. sulph.....	gr. iv.
Acid. hydrochlor. dil.....	℥xxxij.
Tr. ferri chloridi.....	f 3 iv.
Glycerini.....	3 iv.
Syr. simp.....	ad 3 ij. M.

As the child was nursing, no other form of food of any kind was given, nor alcoholic stimulants.

The duration of the attack, from its commencement at the vulva to its disappearance at the eyes and mouth, was fifteen days.

FATAL HEMORRHAGE FROM SUPRA-RENAL CAPSULE IN A YOUNG INFANT.

BY

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WITH the consent of Dr. Paul F. Mundé, my visiting surgeon, I report the following case, as being unique and of interest principally from a pathological point of view, and as showing a possible cause, not likely to be suspected, of death in a young infant.

Susanna Janick, æt. twenty, single, Bohemian, domestic, on May 17th, gave birth to a child. The labor was normal in every particular. The length of the first stage was fifteen hours; of the second, one hour and thirty minutes; of the third, twenty minutes. Child male, weight eight pounds; at birth was in excellent con-

dition and cried lustily, and for twelve and one-half hours after birth was, to all appearances, perfectly healthy.

At the expiration of that period, while lying quietly beside its mother, it was seized with vomiting. This lasted only for a few minutes, and was not repeated. Rapidly following the vomiting the extremities became cold, the features very pale and sunken, the respiration shallow and rapid, and each expiration was accompanied by a short sharp cry. A hot mustard bath, with alcoholic stimulants, were given, but called forth no response, the mustard bath producing no visible effect upon the superficial capillary circulation, and the child gradually sank and died one hour and thirty minutes from the time when the vomiting began. The child had suffered no violence subsequent to delivery.

Autopsy. The only pathological condition discovered was the following: The intestines were displaced somewhat to the left by a firm blood-clot. This clot was about the size of a man's fist, entirely enveloping the right kidney and supra-renal capsule. The source of the blood was a rupture in the supra-renal capsule. A large quantity of fluid blood was found within the peritoneal cavity, and an opening was found in the peritoneum through which this had passed from the seat of hemorrhage in the supra-renal capsule.

In my search through the literature bearing upon the diseases of the new-born I have been unable to find any reference to a similar case. Dr. E. A. Maxwell informs me that he performed an autopsy at this hospital, about ten years ago, upon the body of an infant, in which he found a hematoma of the supra-renal capsule. In this case the lesion was bilateral. Unfortunately, the volume of the pathological records of that year has been mislaid, so that I am unable to obtain the details of the case. This is the only parallel case of which I have gained any knowledge.

As to the etiology of this lesion I am able to give only a probable explanation. Similar hemorrhages occur from the mucous membrane of the alimentary tract, from the pleura, pericardium, peritoneum, and pia mater of the brain. The generally accepted belief as to the cause of these hemorrhages is as follows: A congenital deformity exists, giving rise to a weakening of the veins of a part, with subsequent rupture, if the venous circulation be subjected to undue obstruction from any cause. Such cause may arise during labor from pressure on the funis due to an unusual shortness of the cord, to its several times encircling the child's neck, or to its becoming compressed by the uterus, as in breech presentations; or else the

cause may arise after birth from atelectasis. Such hemorrhages would naturally occur on the mucous or serous surfaces, as being the point of least resistance.

Dr. J. H. Ripley and Dr. J. Lewis Smith, to whom I have mentioned the case, suggest that the hemorrhage from the suprarenal capsule may be due to the same cause, the site of the rupture being, for some unknown reason, this unusual one.

The mother of the child was a primipara and seemed to be entirely healthy, making a rapid convalescence. On examination I was able to find no evidence of specific disease.

The offspring of apparently healthy women are by no means exempt from a liability to the vascular weakness referred to, and the explanation of the case above given is the best that has come to my mind.

TRANSLATIONS.

THE INHERITANCE AND TRANSMISSION OF SYPHILIS.¹

BY

DR. M. KASSOWITZ.
of Vienna.

Translated from *Jahrbch. f. Kindhikde.* xxi. B., 1 u. 2 H.

By J. FEWSMITH, JR., M.D., Newark, N. J.

DURING the eight years which have passed since my monograph (1875) on the subject of the inheritance of syphilis, many publications have appeared upon this theme, and it has been actively discussed. I have lately carefully arranged the works of the various authors, and it is now my object to ascertain what progress the subject has made, and to state as accurately as possible the present standpoint of our knowledge upon this most difficult question.

First of all we find that the questions in dispute are much more exactly formulated than before the publication of my article. It is now almost universally recognized that when a child is born syphilitic or shows symptoms of syphilis after birth without infection either pre or post partum, it can, theoretically, have become infected in one of only two ways, either at the moment of conception, through a paternal or maternal germinal cell containing the contagion, or after conception, by a transmission of the

¹ On account of the importance of this subject, and Dr. Kassowitz's extensive labors in connection with it, we have decided to publish a translation of the article in full.—ED.

poison through the blood or secretions of the diseased mother to the organism in utero. Side questions arise in connection with these two principal points. One of the most important of these is, whether a fetus, infected by the father, transmits the disease per placental circulation to a healthy mother. These different points will be taken up in order, and first we will consider the statements of those authors who believe in the transmission of syphilis to the fetus *only* through the fluids of the mother, and deny the *direct* influence of the father.

I. THE OPPONENTS OF INHERITANCE FROM THE FATHER.

There were always only a few authors who took this stand, and during the last eight years the only ones of note who have opposed the theory of the *direct* influence of the father on the fetus are Lewin, Sturgis, Flindt, Wolff, and Roth. Zeissel (in edition of 1880) states that this method of infection is now universally admitted. Of these five authors Roth stands alone in that he denies the spermatic and ovular transmission of syphilis on purely *theoretical* grounds. He says, in short, that all hereditary infectious diseases are transmitted to the fetus in its intrauterine life, therefore this is also true of syphilis. Now this premise is certainly not true. There is one disease whose parasite has been definitely found in the ovum and the semen. This is a disease of the caterpillar of the silk worm, in which the parasite has been clearly demonstrated by Kohn, Lebert and Nägeli, and Pasteur has proved experimentally that the disease results either when a diseased father impregnates a healthy mother and she, remaining healthy, lays diseased eggs, or when the mother is diseased and lays diseased eggs, in which the parasitic cocci may be directly shown. There is also no proof that other infectious diseases are *not* transmitted through the germinal cells. There is great doubt on this point both in regard to tuberculosis and lepra. The *theoretical* exclusion of inheritance from the father is therefore not justified. In expanding his theory, Roth seeks to explain the so very frequent occurrence of the birth of syphilitic children from mothers who apparently remain healthy—a fact in direct contradiction of his theory—by saying, “it is not strange that in an old syphilis of the father, the infection is so weakened that the infection of the mother is confined to the fluids and shows no tendency to outward manifestation.” In reply to this it is enough to say that it is extremely striking and strange that the mother, who receives the poison direct, out of its first source, should show no “external” symptoms of syphilis, while the child, who receives this weakened poison only second hand, from the infected mother, should frequently have the most intense and virulent manifestations on the skin, the mucous membranes, and internal organs. The attempt to bring this theoretical view into unison with established facts is a failure.

We come next to Lewin, who, without paying any attention to the present standpoint of the controversy over the question of inheritance, states it as a simple fact “that the infection of the

fetus can only take place through the blood of the mother," and then goes to draw further conclusions from this statement. He says "if the fetus becomes infected eight or ten weeks before birth the eruption appears at birth, if infection occur later the child may be born apparently healthy, but the symptoms appear later. If the mother was infected before the pregnancy or in its early part, the child, if it does not die in utero, will be born with advanced syphilitic dermatitis and perhaps also show specific visceral disease." From the many objections to which these few sentences give rise I will mention only the following: (1) Lewin has simply ignored the whole very important statistical argument of the mothers, absolutely without symptoms of syphilis, who have borne evidently syphilitic children. (2) We cannot understand why an incubation period of eight to ten weeks must be passed through after a placental infection just as after the entrance of the syphilitic poison at some point on the general surface. On the contrary, it is much more probable that when the poison enters the fetal circulation directly, the stage of incubation will be short or absent. (3) Experience teaches that even in those cases in which syphilis of the fetus is without doubt due to disease of the mother (the father being healthy), the more that the disease of the mother, by time or by treatment, has become weakened, the slighter is the affection of the children, and the later is the appearance of the symptoms after birth. Now, since we may accept the fact that in the later births the infection, if it took place per placentam, must surely have occurred in the first months of pregnancy, so according to Lewin the later births should develop their symptoms surely in utero, and the fact universally established by all observers, that in the later children the skin symptoms make their appearance after a longer period of extrauterine latency, appears, according to the theory of Lewin, to be a complete paradox.

The other recent supporters of the placental theory, Sturgis and Wolff, use the same arguments as Cullerier and Oewre, which we have answered in our first monograph. They report observations according to which men who have had syphilis have had healthy children from healthy women; and believe that they have thus excluded the influence of the paternal syphilis. In this these authors have left out of view: (1) that syphilitic men, after thorough mercurial treatment, may even in the first year of their syphilis beget healthy children. This fact is recognized by the best authors and confirmed by numerous observations. I have already reported such cases, and Fournier has reported not less than eighty-seven. (2) The universally acknowledged experience that the capability of inheritance of syphilis is spontaneously lost after a series of years. This point is reached earlier, of course, when mercurial treatment is used. (3) The fact acknowledged on all sides, that individuals affected with well-marked so-called tertiary forms of syphilis may beget healthy children after energetic treatment, or when their syphilis is many years old. Out

of Fournier's eighty-seven cases, for instance, there are thirty-five in which, *after* the birth of healthy children, the fathers showed late forms of gummy syphilis.

If we now, in the light of this experience, carefully review all the cases cited by the above-named authors against the influence of the paternal syphilis upon the descendant, we are easily convinced that every case may be classed in one of the above-mentioned categories. But even if they should succeed in producing one case in which, after a short duration of the disease in the father and without treatment of the latter, a healthy child was born (an occurrence belonging to the greatest of rarities), they would still have in no way destroyed the great importance of the extremely numerous observations in which apparently and confessedly syphilitic men have married healthy women, and the latter, without ever showing a symptom of syphilis, have borne, not one, but a whole series of syphilitic children, so that in this vast number of cases the direct transmission of syphilis from the father to the child by way of conception must be considered as proved. But since exactly this question, whether mothers who have never shown symptoms of syphilis may bear syphilitic children, is of fundamental importance for our whole discussion, I therefore consider it not superfluous to strengthen the proofs already laid down in my first article by the facts which have become known during the past eight years.

II. MOTHERS WITHOUT SYMPTOMS OF SYPHILIS MAY BEAR SYPHILITIC CHILDREN.

For me personally, from my own observations and careful study, this fact is definitely established, and I have nothing to add to what I wrote before in regard to my own observations, except that of all the mothers of syphilitic children then reported as free from syphilis, not one has since shown any symptom of it, though most of them have remained under my observation. The observations of others during the last eight years may be divided into three groups: 1. Those of the home or family physicians, including pediatricists; 2. The immense material of the obstetricians, and 3. The experiences of syphilis specialists.

The observations of family physicians are of great worth, because they can observe these mothers through many years, and nothing is concealed from them. This is especially the case when a mother knows the cause of the disease in her child, and watches anxiously for any sign of the same trouble in herself, hastening to consult her trusted physician on the slightest suspicious sign. I recall a case of my own, in which the husband was infected four years before marriage; the first child died of syphilis soon after birth, the second child (the father having undergone treatment) was but slightly affected and lives, and now, at nine years, is healthy and blooming. After this the father again was actively treated, and six years later the mother bore a beautiful, perfectly healthy child, now three years old. The mother, a clear blonde, learned the cause of the disease at the birth of the first

child, and has watched for the slightest possible sign of syphilis, but we have never discovered anything whatever pointing in this direction. It is a pity that these observations of family physicians are so rarely published. Inquiry among my professional friends has shown me that almost every one of them could report one or numerous cases in point, but a quite general answer to the question of why they did not make such a report was that they supposed that question was already settled beyond doubt.

As a type of these cases, we may notice one reported by Rosenberg, in which the mother of many syphilitic children was watched and examined year after year, and finally died of (hereditary) phthisis, without ever having shown a symptom of syphilis. The pediatricists, both in dispensary and private practice, also have frequent opportunities of observing the combination of syphilitic child and healthy mother, and a careful revision shows me that all important authors on children's diseases, without exception, believe in the transmission of syphilis from the father to the child, without the intervening infection of the mother. I mention only Bednar, Vogel, Steiner, Gerhardt, Henoch, West, Parrot, and my honored teacher, Prof. Politzer, who tells me that, in his rich experience, extending over more than forty years, he has had *very many* such cases, where the observation of the mother was continued long enough to remove all doubt. Three cases have lately been published by Orth, most carefully detailed and in every way apparently trustworthy. In all three the observation of the mothers was continued for many years without finding a symptom of syphilis, yet one had had seven and the others each four syphilitic children.

The second class of observations, those of obstetricians, have a different character. They are made principally in institutions, and extend in each case over only a short time. This shortness of observation is, however, compensated for by the immense mass of material and the accuracy and care with which the examinations of the women are made, both before, during, and after confinement. To this may be added the fact, emphasized by many celebrated syphilodologues, that, during pregnancy, and especially in the last month, caused probably by the congestion of the sexual organs, syphilitic skin eruptions in the genital region are peculiarly intense and reach "colossal dimensions," while, during the weeks following labor, they rapidly disappear. There is, therefore, scarcely a chance that syphilis, in a mother having a syphilitic child, should escape observation at this time. Now we have recent reports from three great lying-in asylums. Mewis reports from Dresden that, during the last five and one-fourth years there have been 109 syphilitic children born from 108 mothers, in whom no sign of syphilis could be found. Anton reports from Berlin for thirteen months 70 births of syphilitic children. The mothers of 15 of these were *certainly* free from syphilis. From Munich, Hecker reports 53 cases where mothers of syphilitic children showed absolutely no symptom of the disease. This mass of tes-

timony agrees completely with my report from the Vienna Foundling Asylum, published in my previous article. Weil has objected to the latter that the examination of the mothers was, as a rule, made *after* the birth of the child ; but this is not quite so, for only cases which had been confined in the lying-in hospital were admitted afterward to the Foundling Asylum, and in the hospital they were carefully examined. If a single trace of syphilis was found, they were sent to the syphilitic division. Therefore, those who entered the Foundling Asylum had shown no sign of syphilis up to the time of confinement. But even granting that, in one or two of all these cases, the mothers did have some symptom of syphilis which escaped notice, yet such a mass of testimony from four such institutions shows without question that the birth of a syphilitic child from a mother who shows no signs of the disease is not only possible, but may be considered a frequent occurrence.

The third group of observers, the syphilidologues, are peculiarly placed in regard to this question. They do not have much opportunity, either in private or hospital practice, of seeing this combination of healthy mother and diseased child. Their wards and dispensaries contain many syphilitic men and women, but no healthy women, and only occasionally a syphilitic infant. When a mother is herself healthy, she does not bring her syphilitic child to a syphilis clinic, for she has no idea or suspicion of the nature of the disease, and the consciously guilty father generally is very careful not to direct his wife with the sick child to the clinic, probably so well known to himself. It will be even more rarely that a renowned syphilidologue will be called to a case of syphilis in a child born of a healthy woman, for this would be immediately divulging the fatal secret to the wife, and worse still, the wife's family. And so it happens, as I have said before, that distinguished syphilidologues are almost the only class who either deny the theory of direct paternal inheritance or consider it a great rarity. But this renders all the more important and weightier those observations which, in spite of such unfavorable circumstances, have been made by syphilidologues. Their testimony as to the absence of all symptoms of syphilis in such mothers should serve to convince those obstinate sceptics who claim that, in other cases, even long-continued and frequently repeated examinations are not of worth, because not made by a specialist (Vajda). In my former article, I stated that Ricord, Drysdale, Vidal, Hutchinson, and other syphilidologues had come out as decided believers in the direct transmission from the father, whereby, of course, the immunity of the mother is implicitly conceded, and that frequently enough the latter point was especially mentioned. I will now give some strikingly positive observations which have been published by well-known syphilidologues during the last eight years.

Casparry reported in 1875 a case, which he used for argument on the point of whether a mother can be infected from a fetus (a

question we will come to later), in which the mother never showed any sign of syphilis, and another case where a child infected a nurse, yet the mother never had a symptom of the disease.

Güntz (1876) reports cases which he considers absolutely free from all doubt.

Taylor, of New York, a recognized authority in the domain of syphilis, reports (*Arch. Clin. Surg.*) two cases which, with the detailed description given, are extremely convincing. [I omit the cases.—*Translator.*] Taylor also states expressly that he has observed many cases of the same kind.

J. Nevins Hyde reports several cases, from which I extract only two [omitted from translation]. Hutchinson, in an article (1877) on the infection of a mother from the fetus, makes the statement that in *at least half* of the cases in which a healthy woman bears a syphilitic child it is impossible to find any symptoms of disease on the mother. Diday is also a believer in "syphilis par conception," but at the same time he declares that in the *majority* of cases in which healthy women bear syphilitic children, they remain free from the disease. Drysdale has also reported cases in point. Fournier too, certainly one of the most experienced and skilful observers in this field, states that he has known cases where healthy women have borne syphilitic children. There are fourteen such cases mentioned in his book, in which the continued freedom of the mothers from symptoms of syphilis is especially noted. In 1881 Behrend published some observations on hereditary syphilis, one of which is specially interesting. A woman had borne twelve syphilitic children. In spite of this, and fifteen years cohabitation with a syphilitic man, she had no symptoms of syphilis. "She had never any trace of an eruption, never dolores osteocopi or disturbances of the membranous or bony systems, and never any emaciation; on the other hand she is extremely well and has a good color." Behrend says further, "Here is a case where a woman, in spite of bearing so many syphilitic children, shows not only no syphilis, but no effect upon her general health or good appearance." T. Neumann has recently reported a similar case and it comes with the greater force from him because he believed from theoretical grounds, that such women must have a latent syphilis. Finally, Michelson states that from his own experience he can increase the already large list of this class of cases. It appears, therefore, that even the syphilidologues have arrived at about the same result as the family physicians, pediatricists, and obstetricians, and perhaps this collection of the facts may bring the case into the view of even obstinate sceptics. And this, it seems to me, will prove to every careful thinker that *in these cases* the syphilis of the children descends in a direct line from the—usually well-established—syphilis of the father. And I do not hesitate to say openly, that *direct paternal inheritance of syphilis may now be classed with the most decidedly established facts of science.* The continued opposition of some writers to this fact affects it as little

as the protecting power of vaccine is affected by the libraries of volumes yearly written against it.

The transmission of syphilis from the father to the unborn child can only be explained by the presence of the poison in some form in the seminal cells, and since we have no reason to suppose any difference between father and mother in the capability of transmission through the germinal cells of physiological or pathological characteristics, since also we must take it for granted that a syphilitic woman imparts the poison to the still unimpregnated ovum, this establishes, therefore, the possibility of the transmission of syphilis to children by way of conception, through the germinal cells of either father or mother. It remains to show whether this is the *only* method, or whether an originally healthy fetus may be infected *intra uterum* from the mother, through the placental circulation. Before discussing this, we must first clear up another disputed point which is closely connected with it, namely, the question of the infection of the mother from a syphilitic fetus. This is not only a point of great practical value, but also would throw a clearer light on the controversy which has woven itself about the question of placental infection of the fetus; for here, as there, though in an opposite direction, it is necessary for the accomplishment of the object that the syphilitic poison should pass through the dividing walls of the placental and uterine circulations. It is best, therefore, to settle this question so far as possible before proceeding further.

III.—THE INFECTION OF A HEALTHY MOTHER FROM A SYPHILITIC FETUS.

It has been shown in the preceding paragraphs, not only that a mother may remain healthy up to the birth of a syphilitic child, and that in these cases the syphilis is transmitted directly from father to fetus, but also, by a large number of observations, that the mothers frequently enough remain free from symptoms of syphilis afterward; that is, the syphilitic poison, although contained in the fluids of the fetus and in many cases surely circulating through all its tissues, yet throughout the whole period of pregnancy meets with some obstacle between this circulation and that of the mother, and hence is not transmitted to the latter. Not only do these observations, therefore, show that the mother of syphilitic children *may* remain free from symptoms of syphilis, but on this point all authors are agreed, except the few principal opponents of the theory of direct paternal inheritance. The controversy turns in another direction, which may be summed up in the three following points:

1. It is claimed that in a certain small percentage of cases, the mother of a syphilitic child, though healthy at the time of conception, and not afterward infected from without, may in the course of the pregnancy or soon after delivery show symptoms of general syphilis, and that the latter, with the exception of the

absence of the primary lesion, runs its course in exactly the same way as after an ordinary infection from external sources.

2. It is said that such mothers may show no symptoms of syphilis during or soon after pregnancy, but after the lapse of a long time the disease may manifest itself, not with symptoms of the condylomatous (virulent) period, but with the later, gummy manifestations, or especially only in the form of a syphilitic cachexia.

3. There are some authors who claim that even those mothers who, under their own observation, have remained entirely free from every manifestation of syphilis, yet are not absolutely free from the disease, on the ground that they, the mothers, are probably, as a rule, not capable of being infected with the disease. They are, in the first place, not infected from their own children, in spite of the frequent opportunities offered; and in the second place, a few attempts at inoculation in such cases have given negative results. Such authors suppose a symptomless syphilis which is expressed simply and only in immunity against syphilitic infection.

Let us look at these three claims.

1. *Syphilitic disease in the mother running the usual course with the exception of the primary lesion.*—This is the occurrence which was so strongly defended by Ricord and his pupils under the name of *choc en retour*. And strangely enough, the first notices of this form of infection of pregnant woman are cotemporaneous with Hunter's theory of the non-infectiousness of the secondary eruptions on skin and mucus membranes. On Ricord's authority, this doctrine for a time held full sway, and with it the theory of *choc en retour*, while in all the publications of the two previous centuries there is no mention made of such a method of infection. It appears to me that this coincidence was not accidental. The Hunter-Ricord theory of secondary symptoms would have soon met with complete shipwreck in the numerous cases in which men with long-cured primary lesions afterward infected their wives, had not the *choc en retour* theory given an anchor hold, because, at least in cases where conception had taken place, the infection of the mother was traced, not directly to the man, but indirectly to the syphilitic fetus. We might almost say that this theory had to be created to support the other. We know today that the theory of the non-infectiousness of constitutional syphilis is false, and that probably many more cases of infection take place from secondary lesions (*plaques muqueuses*) than from ulcerated indurations; but the theory of retroinfection of the pregnant woman from the syphilitic fetus remains with us. Yet it is not impossible that a law which was formulated on false premises may afterward find confirmation in facts. Such facts, therefore, as seem to support this theory we must now carefully examine. In my earlier article, I showed that all observations made on this point up to that time were weak and wavering, especially in the important point of excluding the usual sources of infection. Let us now closely and critically examine the obser-

vations reported during the last eight years. One of the most important of these comes from Diday. He publishes a table of twenty-four cases, in which he claims that infection took place in this manner, but, alas, only one point can with certainty be gleaned from this table, namely, that twenty-four women at different periods of pregnancy showed symptoms of syphilis. The period of the outbreak in each case is given, but it is not stated in what way this point is fixed, nor how the absence of a primary lesion was made sure, nor whether relapses of old syphilis were excluded. It appears that most of the cases had not been examined by Diday, or any one trustworthy, before the outbreaks, and that therefore the very unreliable statements of the women themselves had to be accepted, and we have no guarantee that the syphilis may not have begun before pregnancy, or that the external point of infection was not overlooked by the patients. There are no statements as to the condition of the fathers, not even whether at the time of impregnation they had primary or secondary lesions. The report of the condition of the children is also very incomplete. In only four cases did they show clearly marked syphilis at birth or shortly after. In the other twenty cases, it seems hardly fair to speak of a transmission of syphilis from a fetus in which no syphilis can be demonstrated, and we surely cannot all agree with Diday when he says in one case, in which the child was born healthy and showed no syphilis while under observation, that this child had a "syphilis imperceptible." It is also strange that in the three cases in which the disease did not show in the child till some weeks after birth, the mothers were infected in the first months of pregnancy. Taken altogether, Diday's cases cannot be considered as very convincing.

Fournier also believes in syphilis of the mother *par conception*, and boldly defends his position. In his crisp style, he describes a type of this form of infection. "A young and healthy girl married a man with a still uncured syphilis. When called to her some months later, you find her with the symptoms of secondary syphilis, but you find no trace of a primary lesion or primitive adenopathie. The man also has no lesion of the skin or membranes which could cause such an infection, and he stakes his existence on the denial of such an *accident* during the whole time of his married life. And the key of the riddle is that the woman is either pregnant or has shortly before been delivered or aborted." We at once note one contradiction here, in that while he says we may be called after a few months, he afterwards admits that this may mean enough months for a normal pregnancy, or probably a year. In this case, the absence of the primary lesion and the bubo would not be strange. But I must confess that the positive tone in which this experienced specialist reports the result of his observations cannot fail to make an impression. But for those who will hold to exact facts, his premises turn out to be very unsatisfactory. His reports run about as follows:

VI. observation. Man syphilitic. Pregnancy since commencement

of married life. Syphilis through conception. Papulo-squamous syphilide. Syphilis of vulva and mucus membrane of cheeks. Treatment by mercury. Child, born with syphilitic eruption, dies on eleventh day.

Here we can ascertain neither when the observation of the woman began, nor at what period of the pregnancy the first signs of constitutional syphilis appeared.

XVII. observation. Syphilis through conception. Erythemato-papular syphilide, syphilis of the tonsils, headache. Treatment for several months. Delivery in the seventh month. Apparently healthy child, which died after some days.

Here also there is no statement of the condition of the father, the date of the mother's first symptoms, or of the first observation. The syphilis of the child is also not proven. It is fully as possible that the woman was infected during pregnancy, and gave birth to a non-syphilitic child. The same may be said of the following observation:

LXVIII. observation. Syphilis through conception. Long treatment. Birth of a fully-developed, *probably* syphilitic child, which died of convulsions when a month old.

In Fournier's whole list of one hundred and twenty-three cases of infected pregnant women, only five are mentioned—including the three we have given—in which he claims that the infection *par* conception was proved or probable. In only one of these cases is the date of the outbreak of the secondary symptoms given (in fifth month), and in this case also there is no statement of any scientific observation of the woman in the first months of pregnancy. Fournier will not commit himself in regard to the actual method of the infection in these cases. He thinks it is just as possible that the contact of the diseased ovum causes the syphilis as that the poison is transmitted through the placental circulation.

Individual cases of the same sort have been reported by Mason, Frank, and Lutaud. Mason (Osgood) has reported two cases of supposed infection from the fetus, but only one belongs in this category, because in the second the woman showed late forms of syphilis when pregnant with her *second* syphilitic child. But in these cases also no sufficient guarantee is given for the absence of the primary lesion. Frank's case (omitted in translation) is given here simply to show with what frivolous carelessness material is used for proof of this at least doubtful method of infection. It is not worthy of comment. But we must consider more earnestly a case reported by H. Zeissel as conceptional syphilis. It is as follows:

A man who was treated for constitutional syphilis in 1865 was married in 1867. In 1868, his wife gave birth to a syphilitic child and, after the first puerperal week, showed a maculo-papular syphilide and a psoriasis palmaris specifica. The absence of primary lesion Zeissel claims can be absolutely affirmed, as he had made *almost daily* examinations of the woman up to the outbreak of the syphilitic symptoms.

(To be continued.)

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ORIGINAL COMMUNICATIONS.

A GYNECOLOGICAL STUDY OF THE ONEIDA COMMUNITY.

BY

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THE Oneida Community was in some of its relations a great physiological experiment. As such it has a commanding interest to medical men, and especially to the gynecologist. Here were tried elaborate experiments in sexualism, and an act that is done crudely, passionately, or by reason of blind instinct elsewhere, was reduced to an art. With this strange people, the sexual relation was made to realize, in a certain sense, an artistic fulfilment, to conserve their general social relations, and to contribute to their most refined pleasure.

That sexualism formed the warp in the texture of their religion in nothing concerns us as scientific men; nor is it at all singular, for in all ages religions have existed in which this function entered as a rite.

And here, for the first time in the history of the race, was a deliberate attempt made to apply the rules that govern scientific breeding to an entire community of men and women. A new science was discovered, or rather created, that of "stirpi-

culture.”¹ Its laws were formulated upon those which govern the skilled breeder of short-horns, or the still more delicate art of the bird fancier who breeds to a feather; its practice consisted in combining known conditions of temperaments and mental aptitudes in the men and women who were “combined” in accordance with these traits, in order to produce given results in the children. The Community lived long enough to bring its fine art of coition to something like perfection; but, unfortunately for stirpiculture, it was too brief in existence to reach results. Here, under the rule of male continence and scientific propagation, was made the first attempt to apply the laws of Malthus to human increase. If, in the Community, the art of coition was sometimes a failure, it simply proved that all were not artists; and if scientific propagation resulted in unexpected and undesirable “combinations,” it simply proved that human love and passion were eternal factors that mock alike at prison bars and scientific laws.

It is necessary to say something now about “male continence.” The sexual practices of the Community were those usually understood under this term, plus male continence. Mr. J. H. Noyes, who invented, or discovered—it is difficult to decide which is the better word—this refinement of sexualism, has written of it without reserve. He says frankly that “the Oneida Community in an important sense owed its existence to the discovery of male continence, and has evidently been the Committee of Providence to test its value in actual life.”² As this gynecological study of the Community is made only with reference to the sexual practices which prevailed there, it is important that we understand just what is meant by the term. It is better to let Mr. Noyes describe it:

“We begin,” he says, “by analyzing the act of sexual intercourse. It has a beginning, a middle, and an end. Its beginning and most elementary form is the simple presence of the male organ in the female. Then usually follows a series of reciprocal motions. Finally this exercise brings on a nervous action or ejaculatory crisis which expels the seed. Now we insist that this whole process, up to the very moment of emis-

¹ Essay on Scientific Propagation. By John Humphrey Noyes, Oneida. N. Y.

² Male Continence. By John Humphrey Noyes. Oneida.

sion, is voluntary, entirely under the control of the moral faculty, and can be stopped at any point. In other words, the presence and the motions can be continued or stopped at will, and it is only the final crisis of emission that is automatic or uncontrollable. Suppose, then, that a man, in lawful intercourse with a woman, choosing, for good reasons, not to beget a child or to disable himself, should stop at the primary stage, and content himself with simple presence continued as long as agreeable? Would there be any harm? It cannot be injurious to refrain from voluntary excitement. Would it do any good? I appeal to the memory of every man who has had good sexual experience to say whether, on the whole, the sweetest and noblest period of intercourse with woman is not that first moment of simple presence and spiritual effusion before the muscular exercise begins? But we may go further. Suppose the man chooses for good reasons, as before, to enjoy not only the simple presence, but also the reciprocal motion, and yet to stop short of the final crisis. Again, I ask, would there be any harm, or would it do no good? I suppose physiologists might say, and I would acknowledge, that excitement by motion might be carried so far that a voluntary suppression of the commencing crisis would be injurious. But what if a man, knowing his own power and limits, should not even approach the crisis, and yet be able to enjoy the presence and the motion *ad libitum*? If you say that this is impossible, I answer that I know it is possible, nay, that it is easy." Further on, Mr. Noyes gives the following illustration of male continence which is picturesque, to say the least, and deserves quotation: "The situation (male continence) may be compared to a stream in three conditions, viz., 1, a fall; 2, a course of rapids above the fall; and 3, still water above the rapids. The skilful boatman may choose whether he will remain in the still water, or venture more or less down the rapids, or run his boat over the fall. But there is a point on the verge of the fall where he has no control over his course; and just above that there is a point where he will have to struggle with the current in a way which will give his nerves a severe trial, even though he may escape the fall. If he is willing to learn, experience will teach him the wisdom of confining his excursions to the region of easy rowing, unless he has

an object in view that is worth the cost of going over the fall.”¹ The reader has now both the theory of male continence and some practical instruction as well. There are some arguments in favor of the practice which the author calls Bible arguments, and we will remain just as wise if we omit them. One argument is so forcible, and gives the reader such a clear idea of the author’s style and method, that I cannot resist the temptation to insert it. “It is seriously believed by many that nature requires a periodical and somewhat frequent discharge of the seed, and that the retention of it is liable to be injurious. Even if this were true, it would be no argument against male continence, but rather an argument in favor of masturbation; for it is obvious that before marriage men have no lawful method of discharge but masturbation, and after marriage it is as foolish and cruel to expend one’s seed on a wife merely for the sake of getting rid of it as it would be to fire a gun at one’s best friend merely for the sake of unloading it.”² As a scientific study of the subject, we have nothing to do with Mr. Noyes’ arguments, and must concern ourselves only with results. For thirty years the Community existed under the rule of male continence. “Two hundred and fifty sober men and women have lived together in constant observation of its tendencies and effects.”³

Having said so much about the peculiar sexual habits of these people, it is necessary to say something upon the other side. The illumination must be direct and oblique to give us the lights and shadows—the good and evil that exist in it. There are no persons so well qualified to give the subject this oblique illumination as the women themselves. A lady of whom I asked some questions upon this matter requested me to write out those points upon which I wished information and she would answer them. I did so, and she returned home with the questions. The following is the result, and is just as I received it, except that some parts are omitted which contained repetitions.⁴

¹ Male Continence, p. 10.

² Ibid., p. 21.

³ Ibid., p. 20.

⁴ In order that no eye of suspicion should rest upon any lady at present resident of the Oneida Community Co., Limited, I will state that this

1. "The Community, or Mr. Noyes, who represented it, thought that girls usually had, as they termed it, 'amative desires,' when quite young, and that they would get bad habits unless these feelings were satisfied in the way of sexual intercourse, and so of course they were looked after and introduced into the social system *certainly* at the age of puberty and in quite a number of cases before.

2. I am knowing particularly of at least four women of my own age who had sexual intercourse at ten years of age, and one case at nine years of age. One of these cases did not arrive at the age of puberty until five years after, another not until two years after, and the other two were unwell very soon after, before they were in the least developed. This was not confined to the girls; boys of thirteen and fourteen years old were put with old women who had passed the change of life, and instructed all about such things before they had begun to think of it at all.

3. The sexual relations were encouraged very much. The young women were always instructed that the more unselfish they were in giving the men all the satisfaction they could in that respect, the nearer they were to God. They were encouraged so much that those in office would advise and urge it to both men and women if they thought they did not care much for it.

4. In theory this relation was under a rule, and to a certain extent in practice. Still there was a *great* deal of rule-breaking in regard to it.

5. There was a great deal of complaint by the young women and girls, a few years before the breaking up of the system, of too frequent demands upon them by the other sex. Ten years before, they *felt* just the same, but partly in bondage to their religious beliefs about it, and partly from fear of criticism and the knowledge the relation with a loved one would be broken up, they were quiet, and submitted. I have known of girls no older than sixteen or seventeen years of age being called upon to have intercourse as often as seven times in a week and oftener, perhaps with a feeling of repugnance to all of those whom she was with during the time. She would do this with-

paper has been in my possession several years, and was written by a lady who had left the O. C. never to return.

out complaint simply to gain the confidence of those in charge of such things so that she would be allowed to associate with some one she loved.

6. Sexual relations did occur clandestinely, but were nearly always confessed and the parties criticised and separated; by this I mean the more common people. Those who held office did as they pleased, only they made some show of always having a 'third party.'

7. A lady might refuse at one time without incurring criticism, and at another time be severely criticised, and, too, it made a difference who the person was that she refused. If it were one of the leading members she was just as likely to be taken out of any responsible position she held at the time, and not be allowed to do anything until it was thought she had a good spirit and was humble.

8. Pregnancy was sometimes accidental. Ever since I remember anything about it there have been at least from six to eight pregnant women during the year, and perhaps one or two of these by accident, and in some cases no possible way of telling who the father of the child was. This, of course, was in accidental pregnancy.

9. Abortion was never practised while the social theory was in existence to my certain knowledge. What was done after people were married I will not attempt to say.

10. Love affairs were frequent and caused a great amount of trouble, sometimes causing one or both of the parties to leave the Community (of their own accord). It was generally like this: If a young couple loved each other and were intimate, so much that they did not care for others, they were severely criticised and separated, one being sent to Wallingford, and all correspondence forbidden. It was frequently the case with those who had children that they were getting too "special" to each other, and to the child. The consequence was that the child would be put into other hands, the father and mother separated, and one or both to have children by others.

11. I cannot say that there was any *special rule* governing the ages of the parties to the sexual relations. It was very seldom that a young man under twenty years of age associated with a woman who had not passed the change of life, or who was not so near it that she would not be likely to become pregnant.

Of course there were some exceptions to this. As to young women and girls—girls, after they were twenty or twenty-five years old, were allowed to associate with men who were not very much older than they were, but with the older ones, too. Girls under those ages did not, as a general thing, associate with men who were much under forty years, and then very seldom. They were considered better off, morally and physically, if they were sought after by men fifty and seventy years of age, and in fact were put under moral pressure about it.”

If this investigation into the health of these people has any scientific value at all, it comes from the light thrown upon the physiology of the sexual relation. What they are physically must be understood in the light of what they do sexually.

It seems proper that I should say something about my connection with this investigation of the sexual health of the Oneida Community. In the autumn of 1877, Dr. Theo. R. Noyes, with whom I had been acquainted at that time nearly a year, spoke to me about the feeling of dissatisfaction, then growing in the institution, concerning the effect of their peculiar sexual practices upon the health. As the subject was one of great physiological interest, I expressed a willingness to undertake the necessary investigation. He returned to the Community, and in about a week after I received a letter inviting me to Oneida, to make a study of the subject upon the lady inmates. At that time, I have been since informed, there already existed the two factions, one in favor of, and one opposed to the sexual habits that were then practised, and which division finally resulted in breaking up the Community. Whether the examinations were allowed after consultations with one or both parties I do not know, but that visit was the only one I ever made for this purpose. About one-fourth of the lady inmates were examined when the investigation was stopped by, as I have since learned, the interference of the venerable head of the Community himself, Mr. John Humphrey Noyes, whom, by the way, I have never seen.

I commenced my work directly after breakfast, and continued until day-light began to fail. Each lady was brought into a small steam heated room, the dormitory of Dr. Noyes, who was present and assisted at the examinations. From the order and manner in which they presented themselves, I am

quite confident that there was no attempt to select cases by Dr. Noyes; but those, young or old, were brought in who were willing to submit to the examination. They were bright and intelligent women, and were modest and lady-like in their manner.

The lady, whose report I have included in this paper, says: "In theory this relation (the sexual) was under a rule, and to a certain extent in practice." It is but justice to the Community that I state what I know upon this subject, in contradiction to the extraordinary stories about drawing lots, and the ungoverned license which have been related by newspaper correspondents. I sought information upon this matter as a preliminary to my investigation. My informants were Dr. Noyes and Dr. Cragin, the then resident medical member. I have every confidence in the truth of these gentlemen. As the lady reporter says, these rules may have been violated, as all laws and rules are in sexual affairs, but such violations did not pass without criticism on the part of those in authority. In the Community, as in the world everywhere, the sexual approach came from the man. This was not made directly to the subject, but through a third party, and by whom the wishes of the gentleman were made known personally to the lady. She was at liberty to decline or accept, as she thought best. All reasonable grounds of objection were respected, but what those in authority did attempt to overcome were those objections which originated in too warm feeling toward any party other than the one making the advances. All those sexual solicitations made, as one may say, through the official channels were properly recorded so that the history of each individual was known to every one. Certain advances, such as known to the authorities, were discouraged if for any reason they were believed to be inexpedient. For instance, two individuals of very warm and impulsive temperament were not allowed relations for fear of the consequences; or when both were too young and inexperienced. There were probably other regulations governing the sexual affairs of the Community, but which were not confided to me. Many of them are incidentally referred to by the lady reporter. The sexes roomed separately.

In the table of antecedent conditions every item of interest in the history of each individual that seemed to bear upon the

subject under investigation has been tabulated. It will be observed that about one-half the women examined were originally from the rural population. This accords with what has been observed concerning heterodox religious movements.¹ The morbid indwelling and religious inquiry necessary to those who depart in erratic religions seem to be fostered by the quiet and isolation of country life. Another point of interest to be noted is the early age at which a large proportion menstruated for the first time, namely, one at ten years, eleven at twelve years, and twelve at thirteen years. It follows that about fifty-seven per cent menstruated nearly two years in advance of the average age for girls in this latitude. Other causes may have operated to produce this, but the one most evident is the mental and physical stimulation due to the peculiar sexualism that surrounded them. Sixteen of these instances of early menstruation were exposed to communistic marriages from ten to thirteen years of age. By comparing the column of weights, opposite these cases of early sexual intercourse, we find that they correspond to the average. By following out the numbers which identify these cases in Table II. in the columns of chest measurements, we find an average of thirty-four inches in chest expansion, a bust measure that, if anything, is in excess of the average for the adult woman. In some examinations I have made upon this subject, I have concluded that the girl at thirteen years of age has about four inches to add to her height, and twenty pounds to her weight, before she reaches the average development of the adult woman. However repugnant it may be to our sense of manhood, we cannot resist the conclusion that sexual intercourse at this tender age does not arrest the steady tendency to a fine and robust womanhood. From what we all have observed of the stunted appearance of women who have borne children prematurely, it would seem that the extraordinary care with which impregnation was prevented in the Community was a redeeming feature of Mr. Noyes' system of sexual intercourse, in its humanitarian and physical relations. As a gynecologist, I think I may say further that in no other way than by male continence could impregnation be insured against.

In contrast to this phase of their sexual life, by a further

¹ See Hepworth Dixon's "Spiritual Wives."

analysis of Table II. we observe the comparatively advanced ages at which communistic marriages were first indulged in by several. Among five of these, we have an average age of forty-eight years, ranging from the extremes of forty-three to fifty-two years. These subjects all contracted regular marriages, before entering the community, rather later than is usually the case, the average being about twenty-seven years. One would suppose that these women joined the community and conformed to their practices when they were ill-prepared to change their sexual habits; yet if we examine their physical state in the column of weights and chest measurements in Table II., we perceive that they enjoy a vigorous old age, and, upon a careful questioning concerning the symptoms at the change of life, they testified that they had passed this trying period without any unpleasant results. All, except No. 6, had an average of 3.5 years of menstrual life after joining.

Continuing our examination of Table I., we observe an aggregate of fifty-eight children, forty of whom were born of mothers who contracted regular marriages before entering the community, while the remaining thirty women exhibit a fecundity of only eighteen children. Those women who had contracted previous regular marriages confess to two miscarriages, while the remaining four miscarriages were distributed among the thirty communistic marriages. These figures prove conclusively, if any other proof were needed, the thoroughness with which impregnation was interfered with, and probably by the method peculiar to these people, namely, male continence.

Remarks.—No. 9 only conformed to sexual habits eight times. No. 20, child stillborn. No. 23, age at communistic marriage and length of time resident are blank in my notes. No. 27, stirpiculture was attempted in this case, but she proved sterile. No. 31 ends the series undertaken for the hygienic investigation. The others are added from case book and pencil notes. As the original memoranda of the investigation are in the hands of parties in the community company, limited, the initial numbers are transposed, so that it is impossible to identify the subjects. The key, which was never in my possession, is thus rendered useless.

Keeping before the reader's mind the fact that this is not a study of a group of women having a promiscuous and regular

Table I.—Antecedent Conditions.

No.	Age.	Parent's occupation.	Age when menstruation began.	Age when married.	Age at communistic marriage.	Length of time resident.	Length of time resident under comm. relation	No. of children.	Age of oldest.	Age of youngest.	Hereditary tendencies.	Complexion.	Weight.	No. of miscarriages.
1 59		Mechanic.	15	22	29	30	30	4	36	29	Good...	Dark...	130	0
2 31		10	..	10	29	17	2	..	4	do.....	Blonde	115	0
3 31		Printer...	13	..	13	29	16	2	8	5	do.....	...	1	
4 30		Farmer...	18	..	18	28½	10	3	5	6 wks	do.....	Blonde..	154	0
5 18		Agent....	12½	..	12½	18	6½	0	Rheumatic....	do.	135	0
6 60		Physician	14	30	49	11	11	2	29	..	Consumptive..	do.	97	1
7 44		Mechanic.	12	16	16	29	13	1	15	..	Good.....	Brun...	145	0
8 54		Farmer...	13	19	43	11	11	6	34	15	do.....	Dark...	127	0
9 37		do.....	15	17	36	1	1	4	19	4	Consumptive..	Blonde	143	0
10 80		do.....	14	30	52	28	28	6	50	30	Children consumptive..	Dark...	120	1
11 18		do.....	15	..	17	1	1	0	Consumptive..	Blonde	145	0
12 62		do....	16	32	32	30	30	2	30	20	Scrofula.....	Brun...	145	0
13 79		do.....	15	29	51	28	28	0	Good.....	Blonde	110	0
14 35		Mechanic.	12	..	12	29	23	0	Unknown.....	Blonde	95	2
15 23		Farmer...	12	..	12	23	11	0	Good.....	Brun...	150	0
16 34		do.....	15	..	15	25	19	0	do.....	Blonde	104	0
17 29		Mechanic.	13	..	13	29	16	0	Consumptive..	do.	97	0
18 36		Farmer...	14	..	14	25	22	1	4	..	Good.....	do.	108	0
19 21		Clergym'n	12	..	19	2	2	0	do.....	do.	99	0
20 29		Mechanic.	13	..	13	29	16	1	Consumptive..	Blonde	88	0
21 56		Farmer...	15	21	..	26	30	4	33	18	Good.....	Brun...	120	0
22 55		do.....	16	24	30	25	25	0	do.....	Blonde	146	0
23 71		do.....	12	27	2	40	28	Consumptive..	do.	120	0
24 72		do.....	12	26	45	27	27	4	44	36	Good.....	do.	160	0
25 19		Miller...	13	..	13	19	6	0	Consumptive..	Brun...	126	0
26 15		Clergym'n	13	..	13	12	2	0	Good.....	Blonde	110	0
27 39		Mechanic.	14	..	16	23	23	0	do.....	Brun...	94	0
28 21		14	..	14	21	7	0	do.....	Blonde	108	0
29 24		14	..	14	..	10	0	0
30 41		Farmer...	13	..	15	26	26	1	18	Brun...	135	0
31 20		..	13	..	13	20	7	0	Good.....	Blonde	115	0
32 18		12	..	12	18	6	0	do.....	Blonde	118	0
33 46		15	28	31	15	15	2	20	16	do.....	Blonde	130	0
34 35		Farmer...	13	..	15	20	20	1	6	..	do.....	Brun...	115	1
35 33		13	..	13	33	20	2	11	5	do.....	Blonde	120	0
36 32		12	..	12	32	21	2	8	5	do.....	Blonde	120	0
37 43		13	20	28	15	15	3	26	20	do.....	Blonde	130	0
38 20		12	..	12	20	8	1	3	..	do.....	Blonde	110	0
39 17		..	13	..	13	17	4	0	do.....	Blonde	100	0
40 26		14	..	14	23	15	1	6	..	do.	Blonde	115	0
41 21		12	..	12	21	9	0	do.....	Blonde	120	0
42 25		15	..	15	20	10	1	4	..	do.	Brun...	125	0

sexual intercourse, as the term is usually understood, but of a group having promiscuous sexual relations of the most artificial and extraordinary form known to us at the present time, we have

in Table II., upon general physical conditions, a tabulation of great interest. In it I have gathered together all the facts that seemed to bear upon the subject. One item which may have some interest was overlooked. Vegetarianism at one time prevailed as a rule in the community, but in 1877, the date of the investigation, some had abandoned it in its rigid form and used a diet of partly animal food. How many of those whom I examined were pure vegetarians, or what effect it may have had upon nutrition, I do not know.

In the first series of columns relating to the nervous system, the number who complain of pain in very many forms is the first point of interest. Of the total, eighteen make complaints of symptoms that point directly to the nervous system. Sacralgia and ovarialgia are the prevailing symptoms which indicate disturbance of the reproductive nerve centres. They were not at all instances of habitual symptoms; but pain, if any, was referred to the parts named. Mentally, low spirits were complained of in a few instances, and if we examine their ages, we perceive that, with the exception of one party, they were at the time of life to suffer from disappointed affections. Hysteria was remarkably absent, in view of the other nervous symptoms. So far as I am able to judge, they appear to conform, in degree of nervous health and vigor, to the condition of average wives whose physical powers are severely taxed by the duties of life; for it must not be overlooked that each of these women, unless actually excused from duty by disability of disease or the infirmities of age, had assigned her regular duties, either supervisory, clerical, or manual. One part of the investigation was upon this topic—the working capacity of each woman, but we were obliged to abandon it, owing to the opposition of the hostile faction.

The nutritive system, with only a few exceptions, was kept up to a normal degree, even in those cases that developed nervous symptoms, showing that they were liberal and judicious feeders. One fact that may have had a bearing is the justly-famed cooking of the community.

The uniform high pulse rate is explained by the excitement attending the examination, as the pulse was taken at that time; but this did not give so much trouble as pulse-taking is often the occasion of in life-insurance examinations. The great

variety in the range of the thermometer is a matter I cannot explain to my own satisfaction. I had a reasonable confidence in my instrument; all the exposures were of five minutes' duration, and in nearly all cases by the vagina, which ought to record a temperature of one or one and one half degrees higher than the axilla. It occurs to me, since, that the vagina, after a specular examination, is not a point to expect a uniform temperature observation within a normal range of variation, with the subject upon her back, since in the married the passage may be patulous, and air enter and escape in respiratory movements. Such as they are, however, they are before the reader, and he is to draw his own conclusions. Whether the frequent sexual acts—probably prolonged beyond what is usual in physiological coitus—may have incited a higher range of temperature in the genital parts than is generally observed there is a question that would require a long series of observations to determine.

A letter from Dr. Noyes concerning the chest measurements is annexed.

O. C., Nov. 6th, 1877.

DEAR DOCTOR:—The following are the measurements of chest expansion. With regard to some of them, I feel a little doubtful. I find that it is an art to take the measure equally well in all cases. Some are ambitious and really work harder to expand the chest than others. Some of those who only show one-half inch expansion seemed to do most of their breathing with the diaphragm, of which the measurements take no account. However, here they are. I leave in the morning for the East and shall be gone two or three weeks. I will give the estimate of working capacity when I return.

Yours truly,

THEO. R. NOYES.

Many facts of interest are grouped in Table III. concerning the menstrual function. The menstrual interval, taking into account those in the dodging time, exhibits very nearly normal periods. Numbers 5, 9, 11, 32, and 38 are the only cases that show an error indicating pathological frequency. If we trace those numbers that are subject of errors of frequency through the remaining columns, we shall see that number 11 corrects this error by a short period and a scanty flow; but in the last column it is to be seen that she belongs to the congestive type,

Table II.—General Physical Condition at Time of Examination.

No.	NERVOUS SYSTEM.		NUTRITIVE SYSTEM.				Respiratory system.	Muscular system.	Pulse.	Temperature.	Measure of chest, expiration.	Measure of chest, inspiration.	REMARKS.
	Sleep.	Pain and other Symptoms.	Mental State.	Appetite.	Digestion.	Bowels.	Nutrition.						
1	Good.	sacralgia, good.	good.	fair.	const'p'd.	catarrh.	good....	84 98½	35½	36½	Rheumatic.
2	"	" h'dache	hysterical...	good..	indigest'n	regular...	good..	"	medium	80 99	33½	34½	Considerable pelvic pain.
3	"	" none.....	good.....	"	" none..	"	"	"	good....	80 100	31	33	Examine Tables V. & VI.
4	"	"	"	"	"	"	"	"	80 100	37	38	
5	"	headache, rare hysteria.	"	"	"	"	good..	"	"	84 99½	37½	39	
6	Bad..	in r. side, none.....	"	"	indigest'n	diarrhoea.	fair..	habit. c'gh	impair'd	76 99½	32½	32½	
7	Good.	none.....	good.....	"	normal	regular...	good..	normal.	good..	76 99½	36½	38	
8	"	sacralgia.	"	"	"	"	"	"	"	78 99½	33½	34½	
9	"	" none.....	"	"	"	"	"	"	"	80 99½	36½	37½	
10	"	"	"	"	"	"	"	"	"	78 100	34½	35½	Heart palpitat'n until year ago
11	"	sacralgia.	"	"	"	"	"	"	80 100	35½	36½	Hysteria ceased aft'r join'g O.C
12	"	" none.....	normal.....	"	"	"	good..	"	"	70 100	39	39½	
13	Poor.	hypogast'algia.	"	"	"	"	fair...	"	"	70 99½	31½	32½	Rheumatic at times.
14	Good.	in side, overw'k	"	"	"	"	"	"	78 99½	30½	31½	
15	"	none.....	"	"	"	"	good..	"	"	78 99½	34	35½	
16	"	habit. h'ache	"	"	"	"	"	"	72 100	32½	34	
17	"	" none.....	"	"	"	"	good..	"	"	80 99½	30½	32½	
18	"	"	"	"	"	"	"	"	"	84 99½	32½	34	
19	Poor.	"	"	"	good.....	"	n'm'l	nas't cat'h	slight...	85 99	32	32½	
20	Good	"	"	"	dyspeptic.	"	impr'd	normal.	"	96 101	30½	31½	
21	Poor.	"	despondent..	"	normal....	"	n'm'l	"	good....	79 101	35½	37½	

No.	NERVOUS SYSTEM.			NUTRITIVE SYSTEM.				Respiratory system.	Muscular system.	Pulse.	Temperature.	Measure of chest, expiration.	Measure of chest, inspiration.	REMARKS.
	Sleep.	Pain and other Symptoms.	Mental State.	Appetite.	Digestion.	Bowels.	Nutrition.							
22	Good.	none.	nervous.	good.	normal.	regular.	n'm'l	normal.	good	84	98½	32½	34	Had chronic diarrh. for a year. Rheumatic.
23	"	"	normal	"	indigest'n	"	"	"	"	78	99½	34½	35½	
24	"	"	"	"	"	"	"	catarrh.*	"	74	100	38	38½	
25	"	"	"	"	normal	"	"	catarrh.	"	80	100½	34½	36	
26	"	"	"	"	"	"	"	normal.	"	84	100	31½	33	Does not appear bright [mentally.
27	"	"	"	"	"	"	"	"	"	80	100½	30½	32½	
28	"	headache.	"	"	"	"	"	"	"	78	99½	34½	36½	
29	"	"	despondent.	v'r'ble	indigest'n	const'pt'd.	imp'r'd	"	"	96	100	30½	32½	
30	"	none.	normal	good.	normal	regular.	n'm'l	"	good	87	99½	34	34½	
31	"	sacralgia.	"	fair.	"	const'pt'd	an'm'e	"	"	78	
32	"	none.	"	good.	"	regular.	n'm'l	"	"	80	
33	"	"	"	poor.	indigest'n	const'pt'd.	imp'r'd	"	slight.	80	99½	
34	Poor.	sacralgia. r. ov'rgia	despondent.	fair.	"	"	"	"	good	78	98½	
35	Good.	sacralgia. ovar'gia	"	"	"	"	"	"	"	75	99	
36	"	sacralgia.	normal	"	normal	"	fair.	"	"	74	98½	
37	"	none.	"	"	"	"	good.	"	"	
38	"	"	"	good.	"	"	"	"	"	
39	"	"	"	fair.	"	"	"	"	slight.	
40	"	sacralgia.	"	good.	"	"	"	"	good	80	98½	
41	"	Ovarialgia.	despondent.	fair.	"	"	"	"	"	
42	"	sacralgia. ovar'gia	normal	"	"	"	"	"	"	78	99	

Note.—Temperatures, except those indicated by the letter *a*, were taken in the vagina. Concerning the chest measurements I append Dr. Theo. R. Noyes' note.

* Head and throat.

while 38 may also be assigned to the same class. The remaining cases we have selected may be fairly classed as hemorrhagic menstruation. We must bear in mind that great latitude is allowed within the normal limits in this relation. The duration averages a normal period, about five days; but, pathologically speaking, there is no meaning attached to an average of this character applied to a group. The marked errors apply to those numbers already selected out of the total; but glancing down the column, we must confess that, as we have had occasion to question women touching this function in other modes of life, no special difference is to be detected. In the absence of any well-defined standard of what a normal woman really is, I claim that in duration they comply with all we know of this measure. Of the total, twelve show the amount "large" or "profuse;" the former term is intended to indicate a normal limit; four are scanty in quantity, three of whom suffer dysmenorrhea; the remaining numbers show a healthy amount. Including ten of those described as "large," we have only six of whom we can say that they show serious error. In character, we find eight that approach the abnormal. These are described under the terms "watery," "small clots," "pale and watery," "dark and watery." The last column gives the worst showing of all. But here we are confronted with one source of error that would make the gynecologist, who sees in every woman a possible patient, condemn the whole group unless this was clearly stated. The symptoms are those which the women themselves used to describe their sensations, and, with the exceptions noted, are those which three-fourths of the women use to explain their subjective conditions during menstruation. Five of the total number were occasionally obliged to stop work by reason of dysmenorrhea, and in these cases rest was not habitual. Those who said they suffered "slight pain" were classed by Dr. Noyes and myself with those who said they were free from pain, as were also slight sacral pain. Numbers 37, 36, 31, 17, and 11 are the only cases that I should select as subject to noticeable error in this relation. Taking the table as a whole, I cannot see wherein it would differ from a similar tabulation of the menstrual function of a like number of women in the married relation in life as we are familiar with it, and when we take into consideration the

Table III.—Menstrual Function.

No.	Frequency; days.	Duration; days.	Amount.	Character.	Symptoms and other attending conditions.
2	28	3-4	Before child's b'th, scanty; now, moder- ate.	Occasional clots; bright.	Free from pain.
3	25	7-8	Normal... ..	Normal.....	Sacral & hypogastric pain.
4	28	7	do.	do.	Dysmenorrhea until after labor.
5	21-25	5-7	Large	do.	Free from pain.
7	last 70	5	Irregular; last very large.	Bright; clots.	Slight pain.
9	21	4	7 to 8 napkins.	Dark and watery.	No pain.
11	14-21	3	Scanty.....	Pale and watery.	Sacral and hypogas. pain; bearing down.
14	28	5	Moderate. ...	Normal.....	Slight pain.
15	31	5	do.	do.	Free from pain.
16	28-31	5-6	Normal.....	do.	do.
17	21-28	7	Large	do.	Sacral and hypogas. pain more severe a week after.
18	35-56, 70	8	Moderate... ..	do.	Free from pain, probably in dodging time.
19	31-32	4-5	Scanty	do.	Slight sacral pain.
20	28	5	Moderate.....	Occasional small clots or shreds.	Free from pain.
25	28	5-6	do.	Normal.....	Usually free from pain.
26	90	5	Large	do.	No pain; this was the last interval.
27	28	5	Normal.....	Bright color..	Sacral pain; bearing down.
28	28	7	Large	Normal.....	Sacral and hypogas. pain.
29	28	5	Decreasing in amount.	do.	Sacral pain.
30	28	6-7	Normal.....	Rarely small clots.	Pain 1st day; hypogastric.
31	28	7-10	Large	Varying, dark or bright.	Sacral and hypogas. pain occasionally down limbs.
32	14	10-12	do.	Bright.....	No pain.
33	30-120	2-6	Very variable.	Dark offensive	Sacral and right and left ovarian pain.
34	28	5	Normal.....	Normal.....	Free from pain.
35	28	6-7	Profuse	Bright.....	Uterine pain.
36	28	5	Large	Small clots...	Uterine expulsive pain.
37	28	4	do.	do.	Aggravation of general discomfort.
38	21	5	do.	Normal... ..	Sacral pain.
39	24	5-7	Scanty	Watery	do.
40	28	7	do.	Normal.....
41	25	5-6	Large	do.	No pain.
42	28	4	Moderate.....	do.	do.

severe duty exacted of all these women in the various business departments of the Community, I believe that I am safe

in saying that they are subject to no derangements of menstruation that we may assign to their peculiar sexual habits as a cause.

If it be true, as I have somewhere read, that a woman's good or bad condition at her climacteric depends upon the hygiene of her former sexual life, then the story told in Table IV. is a disappointing one. There does not appear but a single error of which it is possible to suspect their peculiar sexual life to be the factor of. Tilt gives the average of the cessation of menstruation in 1,082 women as 45.7 years.¹ Table IV. shows an average of 49 years. Numbers 1, 13, and 23, two having completed menstrual life at 50 and one at 40 years, joined the Community one year after, which fact renders it necessary to exclude these cases, giving us for the remaining numbers an average of 48.7 years, exceeding Tilt's average by three years. This is the error alluded to above. The reader may differ from me, but I submit that number 10 ought to be excluded for the same reason that the former numbers were, as a residence of only two years previous to cessation is not of sufficient duration to affect the result. Making this correction, the average is lowered to 48 years. The reader may offer in objection that, as in number 10 the menstrual function persisted until the age of 54 years, the fact that she joined the Community, and conformed to their peculiar sexual life for two years at such a critical period, is marked evidence of the morbid influence of their sexual habits. As I am giving in this rather unusual contribution facts and not arguments, the matter of nearly three years in excess of the normal limit of menstruation in the average woman has no other apparent explanation than as a result of conformity to their sexual practices. The conditions noted in columns three and four regarding the state of health during and after the completion of the change is remarkable. It shows that, if the tenacity of menstrual life was due to the morbid character of their sexual stimulus, the effect upon the general health was negative. The sexual organs, on careful inspection, seemed in all cases to be undergoing the senile changes in a normal manner. Slight catarrh of the cavity of the uterine neck is not an unusual condition in women well advanced in life, for which reason I

¹ The Change of Life. Am. Ed., p. 48.

have simply given the fact mention. Taken together, if the evidence given by this table is not, with one possible exception, in the main negative, it at least furnishes nothing positive against their sexual theories.

Women who had passed the period of menstrual life conformed to communism in their sexual relation, but not as a rule under the restriction of male continence. Boys who had not yet acquired the art of male continence, and men who found it impossible to keep the act of seminal emission under the control of the will, were obliged to consort with women who had passed the change of life. Younger women were not criticised for declining the company of men who labored under this disability. Both by the law of stirpiculture and of male continence, one who did not possess this necessary credential was

Table IV.—Change of life.

No.	Age at change.	Health during.	Health after.	Length of time resident before change, years.	Condition of uterus.
1	50	good.	good.	21	Depth of cavity $2\frac{3}{8}$ in., healthy on inspection.
6	48	do.	do.	..	Had passed 1 year before joining. Uterus far advanced in senile involution.
7	44	do.	do.	29	Change impending. Uterus $2\frac{1}{2}$ in., free from catarrh. See Table III.
8	50	do.	do.	7	Cavity 2 in. Uterine catarrh slight, acid.
10	54	do.	do.	2	Cavity 1 in., free from all morbid conditions.
12	52	do.	do.	20	Cavity $1\frac{3}{8}$ in., normal.
13	50	do.	do.	..	Joined O.C. 1 year after change. Cavity 1 in. Cervix very small and soft and crowded to left by a firm, non-fluctuating tumor and felt above pelvis, extending $\frac{1}{2}$ of its diameter to left of umbilicus, immobile. Noticed for 3 months.
21	53	do.	do.	27	Cavity $2\frac{1}{2}$ in., erosion of os. ex., slight acid catarrh.
22	48	do.	do.	18	Cavity 2 in., normal.
23	40	do.	do.	..	Did not join O. C. until after change. Cavity 1 in., normal.
24	50	do.	do.	5	Cavity 1 in., normal.

excluded from the peculiar sexual system of the Community. I may say here that I have been told by lady members that the practice of male continence was popular among the females, and was easily followed except by a few men. These exceptional men were relegated to the society of women with whom there was no danger of procreation. While continence

was easy to conform to in the miscellaneous relations of the Community at large, it was difficult to practise between lovers, with whom the psychic influence of mutual passion rendered emission almost a necessity. This was one of the reasons why such constant vigilance was maintained over affairs of the heart.

The presence of catarrhal discharges from the vagina in women who are indulging in sexual connection is so common that, when slight, it may be regarded as physiological rather than morbid. This is quite generally said of those periodical catarrhs that mark the conclusion of menstruation, and I think may be said with equal truth of the slight and constant albuminous discharge observed in a woman at the prime of her functional life. Table V. shows a large proportion of the leucorrhœas to be of this character, and therefore to be regarded as in no way peculiar to the group of women we are studying. It must be admitted that uterine catarrhs were as a rule present. Leaving out those of the character mentioned, we have the condition prevailing to an extent that requires explanation. I have classed 15 of the numbers as having the discharge in its physiological form or as being free from it. The remaining 27 show errors which are too marked to be regarded as due to the usual condition of the parts. While it would not be safe to predicate upon this number of cases the statement that male continence is the cause of the catarrhal condition of the women examined; yet, theoretically, one is justified in the supposition that habitually withholding from the woman in frequent sexual intercourse the sedative and relaxing seminal fluid would favor the passive hyperemia that in a great measure is the cause of uterine catarrh. In one sense of the word, the sexual status of these women must be regarded as polyandrous, and whether this relation would favor a similar result without the practice of male continence I leave the reader to judge for himself. The demi monde is subject to the same catarrhal state of the uterus, but as she is exposed to causes from which the women of the Community were exempt, no conclusions can be drawn from this class.

Taking a first glance at Table VI., 17 per cent only, from a gynecological standpoint, can be regarded as normal. How this may compare with the average of normal wives I am unable to say. We have all manner of figures concerning the

Table V.—Leucorrhœa.

No.	Amt.	Frequency.	Character.	When present.	Reaction	Remarks.
2	Large	Constant...	Albuminous.	Not obs.	Present 8 years ; profuse 5 yrs.
3	Small	Albuminous, after menst. clear.	Alkaline
4	None.
5	Small	Rarely pres- ent.	White and thick.	Not obs.
7	None.
8	Small	Short inter- vals.	Albuminous.	Acid....	See Table IV.
9	Large	Constant...	White and thick.	do.	...
11	do.	do.	Albuminous.	do.	Causes irrita- tion.
14	Small	Rarely pres- ent.	do.	after menst.	Neutral.
15	do.	do.	do.	Acid faintly.
16	None.
17	Small	For a week after menst.	Acid....
18	do.	Rarely pres- ent.	From over- exertion.	Neutral.
19	do.	Constant...	Thick and yellow.	Alkaline	Causes some ir- ritation.
20	do.	do.	do.	Neutral.
21	do.	Occasional.	Thick and white.	Acid....	See Table IV.
25	Large	Constant...	Albuminous.	Not obs.
26	Small	do.	do.	Slightly acid.	Causes some ir- ritation.
27	do.	do.	Just before menst.	Acid.
28	do.	Constant...	Not observed	Not obs.
29	Large	do.	Dark watery.	do.
30	None
31	Large	Constant...	Not observed	Not obs.
32	Small	do.	do.	do.
33	do.	do.	Albuminous.	Acid....
34	Large	do.	do.	do.
35	do.	do.	do.	Not obs.
36	do.	do.	Albuminous, thick.	do.
37	Small	do.	Dark watery.	Acid slightly.
38	Large	do.	White and thick.	Not obs.
39	Slight	Albuminous.	after menst.	do.	...
40	do.	do.	do.	do.
41	Large	Constant...	do.	Acid....
42	Slight	Irregular in- tervals.	After over- exertion.	Not obs.

ratio of one form of pelvic lesion or error to any other form, but I am unacquainted with any figures showing the relation of these errors to women in general. As I was informed in 1877, all of these women, with one or two exceptions which were not noted, believed themselves to be healthy and would not, had they been members of society outside of the Community, have applied to a physician for treatment of any of the local errors which are tabulated. In this view they may compare favorably with the ranks of toiling women who fulfil their sexual destiny in the world at large.

More than half show the position of the uterus to be normal; of the remaining numbers, those with ante flexion of the neck or of the vaginal portion were probably developmental, but if for this reason they ought to be excluded here, they ought also to be rejected from the whole series of tables. More than one-third exhibit variations from the normal in the condition of the cervix, and of these more than one-half are associated with a normal position of the uterus, showing that the errors are due to causes that lead to a chronic hyperæmia of the part. These changes in the condition of the cervix are associated in all the numbers with a softened condition of the vaginal portion, which appeared to me to be an exception to what we usually observe in gynecological cases, and that instances of indurated hypertrophy ought theoretically to be observed. The uterine measurements are the best feature of the table, and yet the absence of general uterine hypertrophy is singular. Faults in the condition of the bladder and urethra are present in sufficient numbers to make this a marked trait of the table, and what adds force to this symptom, they exist independently of uterine position. This, in my experience, is contrary to what we find in practice. If I were to point to an explanation, I should say that here, as already noted in the circulatory errors of the cervix, a persistent hyperæmia of the pelvic organs is a probable cause. I must say that I should be exceedingly surprised if thirty-four women taken without selection from among the married in middle life were to show an equal number of local changes from the normal. Notwithstanding the unfavorable conclusions one is inclined to draw from an analysis of this table, to me, who studied it upon the ground, the impression was a favorable one. This resulted from the

Table VI.—Condition of Uterus and Bladder.

No.	Position of uterus.	Condition of cervix.	Shape and density of cervix.	Depth of cavity, inches.	Condition of bladder and urethra.	Micturition.
1	Normal....	Normal....	2 $\frac{3}{4}$	Normal...	Dysuria, rare, slight.
2	Anteverted.	Gran. eros.	Flattened, soft	3	Painful bladder.	Urination frequent.
3	Normal....	Slight erosion.	Soft.....	2 $\frac{1}{2}$	Normal...	Normal.
4	Retroverted	Normal....	Patulous, soft.	3	do.	do.
5	Normal....	do.	Soft.....	2 $\frac{1}{4}$	do.	do.
6	do.	do.	Slight, conical	..	do.	Rarely irritable.
7	do.	do.	Small, firm...	2 $\frac{1}{2}$	do.	Normal.
8	do.	do.	Atrophied....	2	do.	do.
9	do.	Erosion	Large, conical	2 $\frac{1}{4}$	do.	do.
10	do.	do.	Atrophied....	1	do.	do.
11	Anteflexed.	Normal....	Small, conical	2 $\frac{1}{4}$	Urethra tender.	Occasional dysuria.
12	Atrophied	1 $\frac{3}{4}$	At times frequent.
13	See Table IV	do.	1	Normal...	Normal.
14	Slight retroversion.	Normal....	Soft.....	2 $\frac{1}{2}$	Occasional irritable urethra.	At those times dysuria.
15	Normal....	do.	do.	2 $\frac{1}{4}$	Normal...	Normal.
16	do.	do.	Small, short...	2 $\frac{1}{4}$	do.	do.
17	Anteflexion of neck.	do.	Small, soft...	2 $\frac{1}{2}$	do.	do.
18	Retroverted 2°.	do.	Normal.....	2 $\frac{5}{8}$	Urethra irritable.	Dysuria since child-birth.
19	Normal....	do.	Small, conical	2 $\frac{1}{2}$	Normal...	Normal.
20	Retroverted 2°.	Eros. bleeds easily.	Soft.....	2 $\frac{1}{2}$	do.	do.
21	Normal. ...	Atrophied..	do.	2 $\frac{1}{4}$	do.	do.
22	do.	do.	do.	2	do.	do.
23	do.	Reduced to a trace.	do.	do.
24	Atrophied..	Soft.....	1	do.	do.
25	Very low in pelvis.	Eros. bleeds easily.	Hypertroph'd, soft.	2 $\frac{2}{3}$	do.	do.
26	Normal....	Slight redness of os.ex	Normal... ..	2 $\frac{5}{8}$	do.	do.
27	Anteversion 2°.	Normal....	Soft.....	2 $\frac{1}{2}$	At times irritable.	Dysuria at times.
28	Normal....	Follicular hypertrophy	Soft, patulous.	2 $\frac{5}{8}$	Irritable..
29	do.	Eros. bleeds easily.	Small.....	2 $\frac{1}{2}$	Normal...	Normal.
30	do.	Eros. on anterior lip.	Normal.....	2 $\frac{1}{2}$	do.	do.
31	Anteflex.vag portion.	Eros. of ex.	Small, conical	2 $\frac{1}{2}$	Irritable..	Frequent.
32	Normal. ...	Normal....	do.	2 $\frac{3}{4}$	Normal...	Normal.
33	Retrovert'd, sensitive.	Patulous...	Large, soft...	Occasionally frequent.
34	Retroflex...	Patulous erosion.	Flattened, l'ge	3	Normal...	Normal.

minor character of all the lesions observed, and as the reader will note, if he takes the very nearly general normal measurement of the uterus as an index of the extent to which the morbidly acting causes were at work.

One table which it was designed to construct regarding the industrial efficiency of the women examined, but which, as hinted in Dr. Noyes' letter, was never undertaken, would have told the whole story so far as it concerned the effect of their mode of life upon the moral stamina, or the capacity for steady work. If this quality exists in any one to the extent of keeping him abreast of the general line of competitors, he must possess what is called average health. That is to say, the individual must be able to eat and sleep, be free from persistent pain, exhausting discharges, and the serious invasion by organic disease of any organ capable of becoming a centre of frequent reflex nervous disturbance. I may explain in a comprehensive way that, with two or three exceptions, these women belonged to this class. To one unacquainted with the discipline of the Community, it would be difficult to give an idea of the amount of work and steady application required of each inmate. There were no drones in the hive. To each was assigned the work best adapted to her mental or physical powers, and a debit and credit account kept, so that at the end of a week or a month it was known with rigid exactness how many hours of daily work, and with what result, each one had contributed to the industries of the Community. Such a tread-mill life as this must have required average health to its full extent. Further than this, the communistic theory of property under which they lived, deprived them of the stimulus of exertion for their private benefit, a motive that keeps many a weakly man or woman braced up throughout a lifetime. One other incentive to labor was wanting in the lives of these women, that of love and the instinctive longings of maternity. To the latter no woman's breast is a stranger that has once known man. These feelings were crushed out. The Community was a machine that kept its levers in operation to wring out of the heart of

Note to Table VI.—Through a blunder at the bindery, the MSS. containing the above facts relating to the remaining numbers were lost. As far as No. 33 we have all examined in the official inquiry. No. 30 showed a tumor on right side, reaching half-way to umbilicus, regular ovoid, fluctuating non-adherent to uterus.

woman the emotions that make her all she is to man, love and its tender counterpart, the gentle instinct of maternity.

Now I ask any gynecologist if in the conditions I have tabulated he can trace one or more of them back to the peculiar sexual habits of these people as a primary cause? It is true that in the various tables I have not described well women; but are the habits of the Community, in matters aside from their sexualism, such as contribute to the physical well-being of women? If this is answered in the affirmative, then it is proper to assert that other factors than those which belong to male continence have helped to develop the local errors. I do not know as I am called upon to draw any conclusions, nor would I, if it were not for some tendencies which have entered into gynecology of late years to explain many of the uterine disabilities of women by evasions of the natural consequences of the sexual relation, and by irregularities in the act itself. It is within a recent period that this matter has been dignified with a place in literature, and from several authors it has received the importance due to a new discovery. But such irregularities are nearly as old as the human race, and whatever power they may possess now to produce disease has existed with equal force from the days of Onan down. It will be difficult to convince one who critically compares the physical and mental energies of the present generations of civilized men with those of the past that the race has deteriorated, as it most certainly would have done if such a prevalent and long-existing vice existed with the force lately assigned to it as a morbid factor.

The reader must clearly understand that I am not defending any possible evasions of the legitimate, physiological sexual relation. I am ready to grant that such evasions are physically and morally wrong; but what I am contending for is to give them no more than their just value, as disease-producing causes.

I have described in this paper one of the most artificial sexual mal-relations known to history, and in its most aggravated form, namely, a group of men and women under the laws of communism mingling promiscuously together. In the facts I have presented, without conscious bias, I can discover nothing but negative evidence relating to the effect of male continence upon the health of the Community. My conclusions are

mainly based upon the summary already given, the minor character of the local lesions observed, and the unimpaired working capacity of the women.

A NEW THEORY OF FETAL NUTRITION.

BY

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I DESIRE, in this brief monograph, to call attention to a new theory of fetal nutrition, to which I have been led, after four years' investigation of the physiology of intrauterine life.

Briefly stated, this theory is this :

That, once conception is fairly accomplished, the fetus is nourished by endosmosis and absorption from external sources ; and not through the placental circulation, as heretofore taught. This endosmosis takes place primarily from the engorged tubal and uterine walls ; later the amniotic fluid furnishes the necessary nutriment, and is absorbed at all times not only by the external surface of the embryo, but after the first month it enters the intestinal tract, and is assimilated and distributed to the fetal tissues in a manner identical with the same processes in post-natal life. As a necessary corollary, it follows that the office of the placenta, or at least of the placental circulation, is entirely respiratory, or that of an oxygen carrier and carbonic acid remover.

There are many facts which support this theory, both directly and indirectly, the principal of which are these :

1st. The constant presence of nutritive substances in the amniotic fluid during the whole period of gestation.

2d. The certainty of the absorption by a growing, almost skinless, fetus of any nutritive material in which it is constantly bathed.

3d. The permeability of the digestive tract at an early period, and the necessary entrance therein, according to the laws of hydrostatics, of the albuminous amniotic fluid.

4th. The presence of, as it seems to me, *bona fide* débris of digestion, or meconium, in the lower intestine.

5th. The presence of urine in the bladder, and bile in the upper intestine ; their normal locations.

6th. The mechanical difficulties opposing direct nutrition through the placenta, and the impossibility of nourishment by this method during the early stages of embryonic life previous to the formation of the placenta or umbilical vesicle.

I have indicated these facts in what I consider their respective ratios of importance. The most prominent, therefore, is the constant presence of albumin in the amniotic fluid. Since beginning this investigation I have examined every specimen of amniotic fluid it has been possible for me to secure ; and, owing to the prevalence of criminal abortion in this city, the clinical material has really been ample. In the earlier periods of gestation I have taken only the fluid secured from unruptured sacs. Later, when this was no longer practicable, I used a very clean sponge at the moment the membranes ruptured, taking care to avoid admixture with blood or mucus to any appreciable extent. I have thus been enabled to secure samples at nearly every stage of fetal development, from thirty days to term. Of these I have made quantitative analyses for albumin and albuminoids with the result of finding them invariably present, but in variable quantities. I may here mention that Mr. Adolph Sorrer, of the University of California, very kindly made several analyses for me, which entirely corroborated my somewhat cruder methods. These examinations gave the following results as the amounts of albumin for each period of gestation mentioned. From two to ten samples are represented at each division of the table ; the specimens bring most easily secured before the fourth, and after the seventh month of pregnancy.

At 4 weeks from $\frac{1}{2}$ to 1 per cent of albumin.

" 6 "	"	1	"	$1\frac{1}{2}$	"	"
" 8 "	"	1	"	2	"	"
" 3 months	"	2	"	3	"	"
" 4 "	"	$2\frac{1}{2}$	"	4	"	"
" 5. "	"	3	"	5	"	"
" 6 "	"	3	"	6	"	"

At 7 months from 2 to 4 per cent of albumin.

" 8	"	" 2	" 3	"	"
" 9	"	" $\frac{1}{2}$	" 1	"	"

Now this constant presence of albumin means something. If we accept the water-cushion theory of some physiologists in explanation of the presence and use of the amniotic fluid, we find nature furnishing—for the mother—a very costly, exhausting, and useless ingredient; an anomaly in physiology. If we concede the urinary origin of the fluid, claimed by other observers, we are driven to the necessity of presupposing an universal benignant Bright's disease, which shall drain away as high as six per cent of albumin without injury to the fetus. And, besides, this theory does not account for the fluid found previous to the development of the urinary organs. Its advocates also claim that most of the amniotic fluid is secreted towards the end of pregnancy. This is at direct variance with the facts. I have found that the weight of the amniotic fluid bears a pretty constant ratio to the weight of the embryo. At the end of the first month it is greatly in excess; but, though increasing in positive amount during gestation, relatively it decreases, until at birth the total amount bears no proportion, taking the weight of the child as a standard, to that of the earlier months. As a matter of belief I fancy that but little amniotic fluid is secreted towards the close of gestation, and that it is not so rich in albumin. Either this, or the albumin is more rapidly and completely removed by the relatively great increase in the weight of the child at this time.

In short, the farther we search for the source and uses of this fluid, the wilder our hypotheses become, unless we accept the simplest explanation—almost invariably the best—to the effect that it is secreted by the fetal surface of the placenta and exudes into the amniotic cavity for nutritive purposes. That the fetus is nourished by means of the placenta is self-evident, but I deny that albuminous and other formative material passes directly from the blood of the mother to that of the fetus, and is thence distributed to its tissues. Oxygen is thus carried and carbonic acid removed, but nutritive material takes another channel. There is a fine network of capillaries on the placental surface of the chorion which appear to be the direct agents in the secretion of amniotic fluid.

After about the sixth week of embryonic life, the digestive tract is pervious and invariably full of amniotic fluid. This becomes more and more consistent as it descends the intestine, and as fetal life advances, until it assumes first a gelatinous appearance, and later merges into true meconium. Now, although I have already stated my conviction that meconium is a residue of absorption and digestion, the early secretion of gastric and intestinal juices does not necessarily follow. The amniotic fluid furnishes food in an easily assimilable form, and places it in the stomach and intestines where there is no reason why the fetal lymphatics should not select and absorb it as actively as at any time during the post-natal life of the individual. Undoubtedly the primary action of *primæ viæ* is absorption of a merely mechanical nature, like that of a sponge, for instance. I do not believe gastric juice proper is secreted before the sixth or seventh month, and then in very limited quantities. But of this I have no proof at present, either for or against. I only claim that it is against all analogy to suppose the major part of the vital processes of a fetus are held in abeyance and begin suddenly with the first breath, much as a newly constructed engine starts under its first pressure of steam. Or in other words, that a fetus is a kind of admirably timed infernal machine whose "going off" occurs exactly at birth. This is too loosely reasoned. There is no vital process entirely inactive until birth. Only that placental breathing is substituted for lung breathing, because of physical obstacles of a purely mechanical nature, which are not encountered in such functions as digestion, assimilation, secretion, and circulation of the blood.

Physiologists admit that primarily the impregnated ovum is nourished from the tubal and uterine walls, but as soon as a respiratory apparatus is provided, they lose sight of the fact that absorption may, and does still go on, and claim that this apparatus assumes all its functions. If nutrient baths will assist in nourishing an adult through his horny cuticle, surely the same process can go on with vastly superior results when both skin and intestinal tract are bathed continuously in a highly nutritious fluid.

The presence of meconium is also strong proof that stomachic and intestinal digestion has taken place. Or if not

digestion proper, at least elective absorption, which leaves a residue. This residue contains a quantity of epidermis cells and lanugo, substances which could only enter by the mouth after becoming assimilated through the fluid surrounding the fetus. The bile and intestinal mucus present also point to actual digestion.

Meconium appears in small quantities about the fifth month, and increases slowly in amount to term. To hold that it is anything else than a residue of amniotic absorption, is to suppose the fetal intestine to be an excretory organ until birth and then to suddenly reverse its functions and become absorptive in its action. There is too much meconium in the fetal colon and rectum to account for its presence in any accidental manner. We may suppose it "exudes" from the intestinal walls, just as a late writer on the subject supposes that the earlier amniotic fluid exudes from the fetal tissues, but neither supposition is very creditable to our powers of close observation or of scientific reasoning. We may also assume that nutritive material passes directly from the maternal to the fetal blood, but here, also, all analogy discountenances such assumption. In adult life the blood is a common carrier from whose stores each tissue seizes and transforms material suitable for its use. To directly nourish tissue is an office it never performs, and, indeed, nothing is more disastrous to nutrition than for blood as such to break out of its proper channels. Absorption is the great agent of nutrition from the moment of conception until death. I believe the importance of the circulation from a nutritive, not a mechanical, standpoint has been greatly overestimated. The power which enables bone to select bone-material, and brain, brain-matter from a common stock, is one infinitely higher in the human economy than the merely mechanical work performed by the circulatory apparatus. It is as though we were to prostrate ourselves in admiration of the vast and varied capacity of a freight train and lose sight entirely of the superior intelligence which directs it.

There are some tissues, like the cornea, which depend entirely upon absorption for nutrition through life, being without direct blood-supply. This is admittedly the case with the fetus previous to the formation of the umbilical vesicle and placenta. That this condition obtains throughout fetal existence

I respectfully submit has been proven by the facts I have adduced, and with the above brief statement of them I beg leave to "rest" my case with the profession.

TWO CASES OF INVERSION OF THE UTERUS TREATED
AFTER WING'S METHOD.

BY

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DURING my term of service, this winter, as assistant visiting surgeon to the Free Hospital for women, two cases of inversion of the uterus were admitted. One of the cases was of twenty-three months', the other of two and one-half months' duration. The treatment was that advocated by Dr. Clifton E. Wing, of Boston—a method of treatment with which he has been very successful. In the first case, his method was not carried out to the end, as the fundus uteri got black and sloughy-looking. In the second case, nothing of the kind occurred, and I do not think it would happen again, as experience has taught me to use the instrument to better advantage. I feel sure that in a given time I could now accomplish more than I could at first. In both cases the perineum was ruptured, thus giving me more room in which to work.

The method of treatment was as follows: A soft rubber doughnut pessary large enough to closely fit but not distend the vagina was tied on the end of a broom-stick which had been made smooth by sandpaper. A piece of common soft rubber tubing was tacked on the other end of the broom-stick which was allowed to project two and one-half inches outside the vulva, and used as a perineal strap. This strap was fastened to a band around the waist by safety pins. Direct and accurate pressure could easily be brought to bear on the inverted fundus, by regulating by means of the safety pins the length of the anterior or posterior arm of the rubber tubing. The patient was placed in bed on her side, and the instrument was

inserted and brought to bear on the inverted portion. The perineal strap was tightened until the pain in the back became quite severe. She remained in this position for twenty-four hours, when the instrument was removed, cleansed, and after a vaginal douche of six quarts of hot water, she was placed on the side opposite to the one on which she had been during the previous twenty-four hours, and the instrument again introduced. Every second day the bowels were moved by enema, and the urine was passed as often as necessary into old cloths placed between the thighs. As the inverted portion receded within the cervix, a smaller sized doughnut was used, and the size was diminished until the fundus was well within the cervix. Then, as in the second case, a round stick, such as dry-goods men use to wind braid on, was substituted for the broom-stick, and for a pad, a rubber cap such as is used on chair legs to prevent their scratching floors was slipped over its end, and over this was tied two or three layers of a Martin's rubber bandage. This pad was then placed against the fundus and made to follow it up to its proper place. Below is a report of the cases.

CASE I.—Mrs. R., aged thirty-three. Born in Nova Scotia. Has had but one child. No abortions or miscarriages. Menstruation began at thirteen. Was always regular every four weeks up to becoming pregnant. Flowed three to four days, and soaked four to five napkins. Never any dysmenorrhea. Enjoyed good health during her pregnancy. Since her confinement has flowed at menstrual periods from eight to ten days, and has been regular every four weeks. Menstruation returned soon after her labor. Flowed ten days during her last menstrual period. Her child was born twenty-three months before she entered the hospital. The labor was protracted, and was terminated by forceps. The placenta was adherent and was removed piece-meal. Severe post-partum hemorrhage followed, which was checked by packing the vagina with ice and snow. From the loss of blood, she was very much exhausted and had a very slow getting up. Since then has flowed a great deal at menstrual periods. Her condition was one of profound anemia. Evening elevation of temperature. Hectic in the latter part of the afternoon. Night sweats. Loss of flesh and strength. Abundant thick yellow discharge from the vagina, obliging her to protect her person. There was a stellate laceration of the cervix. Three attempts had been made to reduce the inversion. The procedure adopted in the first two attempts at reduction I know nothing about, except that ether was given both times, but the third attempt was made under ether, the inverted fundus being pushed inside the cervix and

there held by silver wire sutures passed through the cervix; but menstruation soon came on and through the enlargement of the parts due to engorgement with blood, the wires were torn out, and the fundus again came down into the vagina. Dec. 9th treatment began. Each twenty-four hours the instrument was removed, but on the afternoon of the 12th inst. a sharp hemorrhage followed its removal. Considering her weak condition, I decided to check the hemorrhage before replacing the instrument. This I did by packing the vagina with dressings containing alum. Another time I would arrest the hemorrhage at once by replacing the instrument, and would not lose twenty-four hours' time as I did in this case. After twenty-four hours, treatment was again begun. Dec. 17th, fundus almost within the cervix. Dec. 18th, fundus was dark-colored and sloughy-looking, and it was thought best not to continue longer with the pressure; accordingly without ether, the inverted portion was pushed well through the cervix by a cotton stick, and held there by three silver wire sutures which passed through the cervix. Three hot vaginal douches were ordered for each day. Dec. 19th, evening temperature 102°. No chill. No more unfavorable symptoms appeared and on the 22d inst. the patient was examined with the intention of removing the wires and continuing the former treatment, but it was found that the fundus had returned to its proper place. For two hours previous to the examination, she had pains similar to after-pains.

CASE II.—Mrs. J., aged twenty-four. Born in Sweden. Married two years. Has had one child. Two abortions—one at eight weeks and one at three months. Last abortion occurred one and one-half years ago. Menstruation began at fifteen. Was always regular every four weeks except during her nineteenth year, when she menstruated only a few times, flowed but little, and the flow was very light-colored. Usual duration of menstruation five days; but little blood lost. Has suffered from dysmenorrhea since her marriage; the pain coming on a few hours before the flow, lasting the first two days, but most severe during the first day. Menstruated every two weeks from the time of the first abortion until she again became pregnant. This comprised an interval of about two months. After the second abortion, menstruated regularly every four weeks until she again became pregnant. Enjoyed good health during her pregnancy. Had no vomiting or nausea during the first months, but hands and arms swelled during the last month, and legs and feet swelled during the last week of pregnancy. Her child was born two and one-half months previous to her entering the hospital. She was in labor nine hours. The attending physician, a man of large obstetrical experience, after consultation, delivered her with forceps. He tells me the placenta followed the child, and the uterus contracted well. There was no inversion immediately after delivery. Post-partum hemorrhage began soon after delivery, and was very severe, resisting hot water, ice, ergotine,

and ether, given subcutaneously. Finally syncope occurred, and the hemorrhage ceased. Puerperal septicemia followed, lasting seven weeks. Flowed none during these seven weeks, but at about the end of this time, during defecation, while sitting on a chamber, had an alarming hemorrhage. The physician found the inverted fundus protruding through the vulva. When admitted to the hospital, she was blanched and very weak. The posterior lip of the cervix was gone, and the uterus was completely inverted. For the first twenty-four hours she was kept very quiet, given plenty of nourishment, and the vagina was douched three times with a saturated solution of alum. During this time she soaked six napkins.

March 2d, treatment was begun. On seeing the patient twenty-four hours afterwards (she would have been seen before, but the house surgeon was not allowed in the ward, as he was attending a case where suppuration was going on), it was found that the doughnut pessary used was too small, and had slipped past the inverted fundus. Thus twenty-four hours were lost. In twenty-four hours from this time the fundus was within the cervix. In twenty-four hours more the fundus was in its normal position, and the uterine cavity, from the internal os to the fundus, measured three and one-quarter inches. The temperature never rose above 99.5°, and she nursed her baby throughout. Each day she took two quarts of milk, three good meals, and forty-five grains ferri et potas. tart. For about six hours before the fundus reached its proper place, she had pains similar to after-pains. On the day following the reduction, the cavity from internal os to fundus measured two and three-quarter inches. On the third day, the same cavity measured two and one-half inches. On the fourth day, the same cavity measured two inches. She began to sit up seven days after the reduction.

QUESTIONS RELATIVE TO PUERPERAL FEVER.

BY

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(Concluded from p. 718.)

INSEPARABLY bound up with the doctrine of specific diseases, holding true, is the originating *de novo* of a specific disease. It is generally held that specific disease is only propagated by contagion. Thus, writing of measles, Niemeyer remarks:

"Measles is a purely contagious disease. There is no doubt that a person is never affected with measles without having been infected by a person with measles," and he dismisses the objection that the first case must have originated independently of contagion, and that, therefore, there is no improbability in supposing that what once happened may occur again, with the remark that "Such reasoning is idle." Yet in his chapter on Relapsing Fever, he says :

"For my part, I have no doubt that in the course of centuries new infectious diseases have developed and taken the place of others that formerly prevailed. In the writings of the ancients, we find wonderfully accurate descriptions even of forms of disease whose recognition and distinction offer the greatest difficulties. There can be no doubt that the regular and easily recognized combination of symptoms characterizing measles, scarlatina, typhoid fever, etc., would not have escaped the sharp observation of Hippocrates if it had existed then as it does now about the native place of the great Asklepiad."

The following case seems to me to be an example of a zymotic disease originating *de novo*, that is, without exposure to contagion :

G. P. was committed to Her Majesty's Prison, Galway, charged with murder. I examined him carefully on his admission, and found him apparently in perfect health, only it struck me that he did not appear at all conscious of the nature of his offence. Seven days afterwards, he was brought up at Petty Sessions under police escort, and committed for trial at the next Assizes. I again examined him on his recommittal, although he was only absent for a day, and he appeared to have a greater appreciation of his condition, and to be depressed and anxious. He remained well and in perfect health to all appearance for five weeks after his committal, or six weeks from his first being sent in.

At the end of that time, I was sent for, and found him very ill: body and extremities perfectly cold; face bathed with perspiration; pulse almost imperceptible at wrist; at the same time heart sounds were fairly strong, the face was pale, pupils dilated; he trembled violently, and appeared unconscious of everything going on about him, and kept rapidly and wildly calling out: "God don't damn me, Devil don't damn me." He was somewhat better in the evening, and reaction had set in next morning; in the course of the day he got bad again, and that night he became violent, attacking with fury the warder in charge of him, and tore his uniform to atoms. After that he became quiet, and passed through an illness identical in its symptoms with typhus fever, ending after twenty-one days in a crisis.

It seems to me difficult to understand how this could possibly be a case of infection.

He was in good health up to his admission; he was then isolated for seven days; he went out with an escort, not one of whom was taken ill; after his commitment, he was bathed, and put into fresh clothes; he remained well for five weeks; then without any prior period of malaise or sickness is suddenly struck down; he has maculæ, sub sultis, black and dry tongue, sordes on his teeth, high temperature, delirium, unconsciousness to passing events, incontinence of urine and feces, extreme prostration, and finally, a well-marked crisis. Up to the last couple of days of his illness, the only other case of sickness in the prison was in the same ward—a man suffering from an injury to his eye. Yet neither this man nor any of the persons coming in contact with him were affected either with typhus or any febrile complaint.

I have seen typhoid spring up in an isolated country house where there was no possibility of the dejecta of an infected person being deposited in the cesspool which originated the disease, and in the same house and at the same time another member of the family passed into a condition of general asthenia, with headache, languor, and loss of appetite, which disappeared on removal to a healthy residence. There are few men in practice who have not seen cases of specific disease of which they could not trace the contagion, while hybrid conditions in which more than one disease was mixed up are not uncommon.

It may be heresy to propound the opinion that specific diseases not unfrequently spring up *de novo*, but also that they do not invariably breed true. If we admit the theory of a *contagium vivum* of disease being caused by micro-organism, then there is nothing improbable in the belief that the same class of micro-organisms at different stages of development, and falling in different soils, produce different diseases—and in this point of view, that is, that zymotic diseases do not invariably reproduce themselves; their relationship to the puerperal state affords strong presumptive evidence, which is further strengthened by the effects not unfrequently produced in cases of recent operations.

Great weight was attached in the debate on Puerperal Fever in the Obstetric Society of London in 1875 to a paper of Dr. Braxton Hicks, and which is quoted in Playfair's "Midwifery,"

as showing that one form of puerperal fever is merely zymotic disease altered by the puerperal state.

A careful consideration of the cases, however, does not seem to me to sustain the interpretation placed on them ; and it may not be out of place to remark that, while Dr. Hicks considers the scarlatiniform rash and congested throat in his son as septicemia, the same symptoms in a puerperal woman he sets down as scarlatina.

Following his division as to cause of disease, we find two groups : I. Having an ascertained or probable cause. II. Cause uncertain. These again are divided into classes. Of these, the most numerous, if not the most important, are : Class 1, A and B of Group I, being scarlatina—A with rash, B without rash. Now what do we call scarlatina ?

An erythematous rash, sore throat, kidney affection, coupled with pyrexia and digestive disturbance, usually having a period of incubation. I say usually, because I have seen scarlatina develop itself without any appreciable incubative period. Now, Dr. Hicks remarks that the rash cannot be considered essential to the disease ; nor yet sore throat ; that both may be absent. Is then kidney mischief an essential element ? We have the kidneys engaged in diphtheria and typhoid, and taking his line of argument, we may say, neither again can we consider the absence of kidney affection as a proof that the fever is not scarlatina, because doubtless cases do occur where kidney mischief is absent. I am aware that a variety of scarlatina is described as latent.¹ “There may be no symptom whatever, and the fact of a patient having suffered from scarlatina may be known only by desquamation of the cuticle taking place or albuminuria and dropsy setting in.” But desquamation indicates a skin affection and albuminuria predicates kidney disease. To say scarlatina exists where there is no symptom of it but elevation of temperature is carrying diagnosis too far, and is the result of the dogma that zymotics invariably breed true ; yet his practical observation had led him to admit : “There is no doubt but that scarlet fever and erysipelas have much affinity ; indeed, some have suspected them to be but a slight modification² of the same poison apparently interchangeable. How far this may be, these cases bring no evidence. That they possess symp-

¹ Robert's Practice of Medicine.

² Hicks, *op. cit.*

toms in common in the puerperal woman forms no further evidence of their unity than can be said of any other of the diseases which are in this report shown as the cause of the post-partum disturbance."

In group 1, class I., A, there are twenty cases. Of these twenty, fourteen had no symptoms whatever of puerperal fever, but were simply bad cases of scarlatina, pure and simple. With regard to one of them, however, there is this significant remark which bears on the view of interchangeability of disease. "There was no evidence of her having been exposed to scarlet fever before delivery. *Her medical man was attending two cases of diphtheria at the time of his attendance on her.*"

In case 4, there were uterine symptoms, pain, tenderness, and enlargement of uterus; in case 5, after ten days of fever, being the thirteenth from delivery, tenderness in abdomen and distinct thickening in left groin appeared. No. 9 aborted during convalescence from scarlatina, complicated with arthritic swelling, joints swelled again, symptoms of metritis and toxemia, and death.

Case 10 is a very doubtful one; there is no history of contagion, and the rash is described as measles-like, no symptoms of scarlatina; similarly case 11, the diagnosis is obscure. The rash "intermediate in appearance between measles and scarlatina, she had none of the peculiar symptoms of either," but observe, two children had had measles in the next house; there had been no illness in the house of the patient before her confinement, but on the next day her children were taken with scarlatina. Case 19 is also more than doubtful. A measles-like rash accompanied this condition without any other symptom either of measles or scarlatina. No account of measles or scarlatina could be found in the neighborhood.

Thus out of twenty cases in only four was there any evidence of pelvic or abdominal mischief. In three it is more than doubtful if the disease was scarlatina; while one was suggestive of the infection of measles producing a hybrid in the mother, which in its turn gave rise to scarlatina in the children.

The evidence thence derivable from the group is simply to the effect that scarlatina or measles attacks puerperal women, that it is recognizable by its ordinary symptoms, and that it is very fatal. In group 1, Class B, are seventeen cases attributed to

scarlatina without rash. They are all headed as puerperal fever. Fifteen had been exposed to the infection of scarlet fever either through its having been in the house, or the patients prior to labor having nursed relatives in scarlet fever, or the medical attendant having cases of scarlatina on hand, but in only one case is there a single symptom of what we usually conceive to be essential to scarlatina recorded.

No rash, no sore throat, no kidney affection. In six, death supervened so soon after the inception of the disease that it may have been caused by scarlatina, time for the production of symptoms not having elapsed; but in the others, although living long enough and three recovering, no symptom of scarlatina is recorded, not even desquamation of the cuticle or dropsy. In case 11, no evidence of infection is produced, and the sole grounds on which her case is set down as scarlatinal appears to be the absence of any exiting cause and that her nurse and husband had rigors, sore throats, and fever; both of them had had scarlatina before. But as I have experienced personally, the same symptoms may occur where all question of scarlatina or other specific affection is excluded. Case 17 is manifestly one of septicemia from a putrid fetus, and no evidence is given as to why it should be considered as possibly due to scarlatinal infection.

Valuable as these records of cases are, they have decided defects. We are not told over how many years they extend, nor whether the patients had already suffered from scarlet fever. We know that an attack of scarlatina generally produces immunity against infection; that second attacks are unusual and third exceedingly rare. I do not know that there is any proof that during the post-partum period this immunity is removed. It would be interesting to know whether in a protected person the poison of scarlatina can produce a disease, without the symptoms of scarlatina, and which has been called puerperal fever.

If it can, then it appears to me evidence strongly presumptive of zymotics not invariably reproducing themselves.

From considering all these cases as scarlatina, Dr. Hicks has been forced into a dilemma: to assume that either the pregnant woman possesses a wonderful power of resistance, or that this disease is communicated to her during labor through the medium of the vagina. I don't think either is correct. No doubt

scarlatina has been inoculated, but there have been many failures; it does not seem easy to propagate it by inoculation. Moreover, the cases in which it could be conveyed through the vagina are very limited, only possible where the doctor or nurse are suffering from scarlatina themselves, or the parturient woman is delivered in the bed in which a scarlatina patient is or has been, or where infected sponges or linen are used about her vulva, all exceedingly rare, so that the question of inoculation through the vagina may be dismissed as a very exceptional mode of conveyance. Nor is it proved that pregnancy gives exemption. On the contrary, even in the cases recorded we find some of scarlatina during pregnancy. The dilemma has evidently arisen from few cases being given. The synthesis is erroneous because the analysis is imperfect. To form an opinion as to the influence of scarlatinal poison on the puerperal condition we should know its effects on puerpera generally, but that is exactly what we don't know. In the debate of 1875 in the obstetrical society it was stated by practitioners that they had attended all sorts of infectious diseases and midwifery cases, and had never seen a case of puerperal fever. A general practitioner attends every kind of fever and does a large midwifery practice. Now I do not mean to say that he may not or does not convey infection to lying-in women, but I want to point out that special cases may be and often are fallacious, inducing us to believe in a *propter hoc* because of a *post hoc*. It does not follow that in Dr. Hicks' fifteen cases, because these women were exposed to the contagion of scarlatina, that therefore puerperal fever followed. At the same time there may have been hundreds of other puerperæ exposed to infection and without any puerperal disturbance occurring. Again, there are cases recorded, and many must be known to men in large general practice, when pregnant women nurse their children and relatives through scarlatina, measles, and other zymotics, yet who pass through their lying-in without any abnormal condition. Dr. Cordes' case is not an isolated example. While, therefore, we must admit that a puerperal woman can contract scarlatina, that the post-partum condition is a bad one for any zymotic disease, and one in which it is exceedingly likely to run a rapid and fatal course, and admitting that it is probable that scarlatinal infection develops a fever indistinguishable from what is called puerperal fever, we require further evidence before admitting the constancy of this

result. We require in such cases not only the fact recorded of the exposure to contagion and illness following, but a record of the previous history: whether there had been a prior attack of scarlatina at any period of life, and how long before; whether it had left traces of kidney disease, and what had been the woman's state of health, not only during pregnancy, but before marriage.

Diphtheria has been credited with being the sole cause of puerperal fever; that certainly is not the experience in this country, nor is its relationship so closely marked as that of scarlatina and erysipelas. No doubt, when it attacks the genitals, the symptoms of typhoid, metritis, and peritonitis may be produced with disastrous results, but when it occurs in its usual site, the fauces, its course does not appear to be much influenced by the puerperal condition; thus out of seven cases recorded by Dr. Hicks, four recovered.

The close connection between erysipelas and puerperal fever has been frequently noticed, so much so that puerperal fever has been stated to be erysipelas of the peritoneum.

Further that puerperal fever is only erysipelas modified by the puerperal state.

Two arguments are advanced in support of this theory. 1, That erysipelas is a zymotic disease and therefore reproduces itself. 2, That parturients exposed to the contagion of erysipelas, or when erysipelas is epidemic, contract puerperal fever and not unfrequently their infants or attendants are infected by them with erysipelas. But it is not at all admitted that erysipelas is a zymotic disease. We find it generally divided into traumatic and idiopathic, and as producing the latter an almost infinity of causes are given; *e. g.*, "whatever has a tendency to disorder the digestive, hepatic, or other important functions," certain articles of diet, mental emotion, exposure to cold and heat, wet atmospheric conditions, etc., etc. Sometimes it appears without any apparent cause; but granting it to be a zymotic disease, it would not follow that it would invariably reproduce itself, and it differs in this from other zymotics, that one attack, so far from producing immunity, rather tends to predispose to its recurrence. In its relationship to parturient women, it also acts differently from other zymotics; they frequently reproduce themselves, erysipelas seldom. There are, indeed, some few cases recorded of erysipelas running its course unaffected by and unaffecting the puerperal state. Such cases are

rare ; the cutaneous affection, the edema and sloughing which characterize its varieties in idiopathic or traumatic cases are the exception, not the rule, and in puerperal women erysipelatous infection produces usually puerperal fever of a highly contagious character.

That erysipelas should be conveyed, by a woman suffering from puerperal fever, to her infant or to her attendants is not astonishing. The child, even in natural labor, undergoes a good deal of squeezing and bruising. It is born with the skin highly congested ; frequently it is scrubbed rather too vigorously to get rid of the vernix caseosa. What wonder if the epidermis was removed in places from its delicate skin, and if it were attacked with erysipelas, more especially if that disease happened to be epidemic ? It goes without saying that, if the mother is ill during labor, if the secretions are acrid or unhealthy, that erysipelas would be likely to result in the child ; but before this can be accepted as a proof that the maternal disease is erysipelas, it must be shown that it does not occur in the children of women who pass through a natural period.

That nurses or attendants on a case of puerperal fever should sometimes contract erysipelas is not to be wondered at, when the physical labor, the mental anxiety, the watching night and day, irregularity of meals, and constant exposure to unhealthy emanation is considered, and I venture to say that, whatever be the presumed exciting cause in the woman, erysipelas may attack the nurse or child. It would be well if we had reports as to erysipelas communicated in cases which are supposed to have originated from scarlatina or other zymotics.

One of the most instructive reports of the highly infectious character of the puerperal fever generated by erysipelatous infection is that published by Dr. Atthill.

On the 15th February, 1877, a woman suffering from erysipelas of the head and face was brought into the Rotunda Hospital, Dublin, late in the evening. Her condition was not ascertained until she was undressed, and as the child's head was on the perineum, she could not be sent out. She was placed in a small ward, by herself, adjoining large ward No. 2, which was also empty, having that morning undergone the usual process of cleaning. Shortly after admission, she was delivered, and next morning, before nine o'clock, although, to all appearance, doing well, she was transferred to another hospital. All the

bedding was then removed to be washed and stoved; the ward itself was fumigated and left unoccupied for some weeks.

On the 16th and 17th after her removal, six patients were admitted to No. 2 ward, which was separated from that in which the patient had been by a small ward occupied by one of the patients. Five out of the six suffered from severe symptoms of peritonitis, and two were alarmingly ill, but all eventually recovered; the only one escaping being a case of abortion at the third month.

On the afternoon of the day on which the erysipelas patient was delivered, four women were admitted to No. 1 ward, which was on the opposite side of the corridor to the ward occupied by her, and further separated by the width of the staircase; of these, two complained a little, a third had symptoms similar, though in a less marked degree, to those in No. 2 ward, and the fourth died. Thus, out of ten admitted after the case of erysipelas, nine were attacked, and one died.

Dr. Atthill asks: "Now, what was the disease thus originating? Are we to believe that erysipelas can induce any disease other than itself?"

I believe the facts answer for themselves. There does not appear to have been a symptom of erysipelas amongst the nine; on what ground, then, but the purely theoretical one of disease invariably breeding true, are we to suppose that a fever so totally dissimilar to erysipelas that it could not be diagnosed as such, or supposed to be it, unless the fact of an erysipelatous case having been in the house was known, was anything else but puerperal fever?

I have already given instances of interchangeability of disease, and would only now remark that the change in type, which is admitted, strongly bears out the view. That fevers are not now what they were every text-book bears witness to. That treatment has varied, not only from advance in the knowledge of the laws of life and pathological changes, from more accurate knowledge of the action of drugs, and improved therapeutics, but from observation that disease acts differently, and, as is admitted, that in different epidemics the type even of the same disease alters, and its symptoms vary.

"Thus, then, we find that the same seasons give rise to erysipelas, typhus fever, scarlatina, and puerperal fever; that they prevail epidemically at the same time, and as an epidemic take on

the same type and appear capable the one of giving rise to the other, or of co-existing in the same patient.”¹

In every epidemic, too, numerous cases will be met with varying so much from the zymotic disease “going” that, were it not for the epidemic, they would never be classified as belonging to the prevalent zymotic, so much do they differ from it in type and symptoms. An individual exposed to contagion will not contract disease unless there be something, call it a leaven or ferment or pabulum, with which the poison of that disease, be it miasm or micro-organism, can combine and multiply; that *something* may be absent, may be present in small or in large quantity, and accordingly the disease will vary in type and severity. Or is it not possible that that entity may be not altogether of the nature to combine in the ordinary fashion so as to reproduce the disease, yet capable of a combination which will disturb the health and produce anomalous symptoms, or those of some other affection, and so explain the variations of disease we daily meet with?

It seems to me that there is clear proof that all the zymotics, typhus, typhoid, scarlatina, erysipelas, diphtheria, measles, etc., act in these ways towards puerperal women:

1. Originating the zymotic disease to which they are exposed, often, but not invariably, of a severe type.
2. Producing a distinct disease of which, no matter what the originating zymotic was, no symptom resembles it, but of which the symptoms are almost identical in all.
3. They produce no deviation at all from the normal standard, no matter how prolonged or intense the exposure may have been.

The distinct disease so produced is capable of communicating to another puerperal woman, not the original zymotic, but itself. It is asserted that this peculiarity arises from the infection being conveyed to the genital canal, and entering the system through some wound, in fact being inoculated. Of this there is absolutely no proof, while there are many against it. A disease, when inoculated, generally reproduces itself. In some cases the infection may act with greater certainty, but the effects on the system are slighter. Thus inoculated small-pox is most usually discrete and often aborts without secondary fever, a result due to the smallness of the dose, as shown by the more fatal effects of the Chinese mode in which the operation is effected by

¹ Churchill : Diseases of Women.

stuffing the nares with dried scab and so allowing a large dose to be inhaled.

But we know that cases have over and over again occurred where there was no possibility of inoculation, and where no vaginal examination was made by any infected person.

The conclusion appears obvious that a fever, truly puerperal, is thus originated, and that it is an illustration of zymotic disease, although they generally breed true, yet not invariably doing so.

I think it would be of extreme service to medical science if a collective investigation, both in hospital and general practice, were instituted into the communication of disease by zymotic influence, and also as to the effects produced by them on wounds.

Quite independent, however, of all the foregoing causes of septic or sapric absorption or contagion, or of infection from zymotic disease, or produced by cold or mental distress, we do find cases of puerperal fever occurring. Often the symptoms commenced during or immediately after labor; of these Dr. Barker has recorded some, and a most virulent, though localized epidemic is described by Dr. Atthill, in a paper read at the Irish Academy of Medicine in July, 1883, and in a paper in the obstetrical section of the meeting of the British Medical Association in the same year.

The discoveries connected with septic poisoning in surgical cases, the incalculable benefits which have resulted from antiseptic treatment, whether we include under that term Listerism or all modes of keeping wounds scrupulously clean and sweet, have so fascinated the profession that the tendency is strong to attribute, as far as possible, all diseases to septic poison, and consequently the term puerperal fever has been eliminated and puerperal septicemia substituted; but admitting that puerperal women may and often do suffer from septicemia, I deny that all febrile disturbances arise from that cause. Some surgical operations performed without antiseptic precautions may get well without febrile disturbance, the wound may heal by the first intention, without the formation of pus or a trace of inflammation. Such cases are the exception, not the rule, and the graver the operation the less likelihood of this result.

But, in labor, it is exactly the reverse. We are told the uterus is left like a stump after amputation. We are assured on high authority that, in every case, there is more or less laceration and wounding of the cervix uteri, of the vaginal orifice,

or of the vulva, and yet septic poisoning after labor is rare, and the strangest thing of all is that it is rarest after severe operations.

"Serious symptoms after severe operations (excepting placenta previa) are really uncommon," and this, too, with the lochia passing over the wounded surface. There is, however, a source of irritation, if not of septic poisoning, which has not, as far as I am aware, been pointed out. That is, except in the lower classes, patients use a bed-pan, and the urine dribbles down over the vulva and back into the vagina through the relaxed and open ostium vaginae.

It is easy to see how this has escaped observation. It is not felt by the patient, and coming away with and colored by the lochia, it cannot be observed on the napkins. My attention was first called to it in a patient suffering from hematuria, who feared that premature labor was coming on, as she said she had frequent discharges of blood from the vagina. Careful examinations showed that labor had not set in, that there was no bleeding whatever from the uterus, and that the fluid stained with blood only came, and invariably so, on the patient turning in bed or sitting up, after using the bed-pan.

This fact may help to explain the occurrence of septic poisoning in some cases; in primiparae albuminuria is most common, and the passing of an albuminous urine into the vagina may give rise to septic absorption, or provide a suitable medium for the growth of septic germs. But even with this predisposing cause puerperal septicemia or puerperal fever is comparatively rare, and after many years' practice in the west of Ireland, I can say that it is most uncommon even amongst the peasantry in the remote districts, whose fecundity is considerable and whose surroundings are the reverse of sanitary.

To discuss the question of treatment fully would occupy far more than the space at my disposal. When there is any suspicion of retained clots or portions of placenta, the uterus ought to be syringed out, and, in the latter case, examined with the finger or hand. To avoid the retention of clots and to prevent absorption of decomposing or septic matter, a thorough contraction after labor ought to be secured. Antiseptic injections of the uterus are doubtless good, but the prevention of their necessity is far better. If a permanent contraction is

¹ Braxton Hicks, *op. cit.*, p. 45.

secured, there will be little need for injection, for clots to decompose cannot then form, and sinuses to absorb will be closed. I doubt much that the placental site has the absorbent power attributed to it, and I agree with Dr. Barker that coagula in the sinuses are rare. It is too often forgotten that the sinuses are veins, whose province it is to carry the blood back from the placenta, that they are collapsible and easily emptied of blood. Post-partum hemorrhage comes from the arteries, and in them clots form. No doubt, in an imperfectly contracted uterus the sinuses may be open, and then, indeed, absorption of septic or sapric matter is likely to take place. There will be few cases indeed in which permanent contraction, closing the sinuses, and expelling all coagula from the uterus, cannot be obtained by proper management of the third stage of labor, by giving ergot and strychnia, and in aguish climates, ergot and quinine immediately after delivery.

This and the application of a firm binder and pad I look on as a most valuable prophylactic quâ the uterus. Then we have to meet the danger of septic absorption from wounds of the vagina or vulva. If there was much danger in the fresh lochia, then septicemia would be far more common. The risk lies in decomposing discharge in a wound. We know that antiseptic spray, antiseptic dressings will be of little value unless the secretions are rapidly removed, that is, unless there is free drainage. Yet free drainage is just what ordinarily is not obtained in midwifery practice. The patient is kept lying on her back, the vagina, a curved canal, is consequently partly filled with discharge, all that comes away is the overflow, and a pool of lochia is left lying in the vagina to decompose and spread its contamination. Syringing only partly remedies this, and probably one reason of the great exemption of our country poor is that they do not lie still after labor. A few hours after delivery a country woman will sit up in bed. On the second or third day she will be up and about the house. Now, I think, we keep our better class of patients lying down too much. If there has been secured a firm uterine contraction, if a firm bandage has been well applied, then after a few hours' rest, I am satisfied, no injury will happen from allowing for a short time the shoulders to be raised by pillows, the patient to move about in the bed, and the vagina to be freely drained. I do not think we know how frequently, even within an hour or

two of labor, the nurse contravenes our orders, and puts the patient on the chamber, and, if found out, will excuse herself by saying she could not pass water on the bed-pan, and yet no harm results. Next as a prophylactic, I think, proper feeding deserves a high rank. My experience entirely agrees with Dr. Playfair on that point. I have read a great deal about milk-fever. I have seen some cases, but never one in my own practice, where I could induce the patient to discard the traditional regimen of starvation and gruel. I have carefully, in many cases, both in primiparæ and others, taken the temperature twice a day, and have found the flow of milk established without a rise beyond the normal. On the other hand, I have seen more or less fever follow when, owing to respect to tradition on the part of the patient, or obstinacy on the part of the nurse, the gruel system has been carried out. Dr. Hicks has observed that, when either undoubted zymotic disease or puerperal fever attributed to its infection occurs, it commenced generally about the third day, that is, at the time that milk-fever sets in. If a patient, after the expenditure of force, and in the condition of nervous exhaustion and loss of blood consequent on labor, be properly nourished, she will be far better able to resist the effects of contagion. It is also worthy of notice that the lochia are far less profuse, and the patient gets up much stronger, and without what many complain of when first moving about, "a falling-down feel." With these precautions there should be scrupulous cleanliness and the use of antiseptics for the hands and instruments and for injections. If fever develops itself in spite of all precautions, our trust must be chiefly in quinine, nutriment, and judiciously administered stimulants.

Salicylate of soda I have found in some instances to reduce the temperature where quinine failed. That may have been because quinine was not used in sufficiently large doses. In both puerperal fever with abdominal complications and in typhoid, I have experienced the best results from the use of cold wet compresses over the abdomen. If the temperature is going up, I have them changed every five or ten minutes. If not high or falling, they are left on, according to circumstances, for half an hour to two hours. The patients like them, and they undoubtedly tend to keep down the temperature.

Some authors still advise the administration of calomel, with or without opium. Where there is pain, sleeplessness, nervous

irritation, the latter is invaluable; but in some cases, chiefly, I think, where the kidneys are engaged, it cannot be borne, and I have seen the most intense distress produced by the itching following a hypodermic of morphia.

As for calomel, except as a purge, my experience of it has been most unfavorable. At the very outset of the disease, a dose of from five to ten grains is often very beneficial, but to attempt to salivate a puerperal patient I look upon as little less dangerous than shooting her. The following case illustrates the fatal result of mercury in puerperal fever, and is interesting as a case in which none of the ordinary causes were present. I could discover none. The house was healthy, isolated in the country; no zymotic infection could be traced; no source of either septic or sapric poison could be found. I understand the child was blind; but as, because my patient died, I was held responsible for her death (through the kindness of neighbors) I had no means of ascertaining the fact. It, however, died in less than a year.

X., a primipara, æt. 28, was delivered on 12th April, 1880. About a month before that date, I was engaged to attend her, and as she was very nervous and expressed great apprehension that she was not all right, I made a careful examination. She was well-made, and appeared a healthy young woman. Pelvis ample and roomy. She had last menstruated in July, 1879. Shortly afterwards suffered from irritability of bladder. Had flow of blood for two days in first week in August, and for one day on the 15th. Complained of constipation; bowels never moved without medicine. I did not learn until some time after that about the third month of pregnancy she had suffered from pain in the left groin, apparently due to fecal accumulation, and had been warned by her doctor in England to be careful about her bowels. She mentioned that she had heard of a friend dying, some time previously, after her confinement, and she looked forward to her lying-in with apprehension.

About first week in April, she took a long drive, and suffered from pains off and on. On the 11th April, she took a dose of oil, and woke at 4.30 A.M. on the 12th, with slight pain and show of blood. A messenger was at once sent off for me, and reached my house at 6.30. I arrived at 8.30, having fourteen Irish miles to drive, and found that the child—a male—had been born at 7.45. Labor progressing rapidly, the local doctor had been sent for. The placenta came away naturally about 8 A.M. As labor had set in a few days before the time fixed, the nurse had not arrived. The uterus was firmly contracted, but large. I injected ergotin subcutaneously, and applied the binder. She was excessively nervous; pulse 110; discharge moderate. At 5 P.M.,

she appeared to be doing nicely; pulse still fast; slight headache; discharge normal.

13th April.—Headache gone; did not sleep much, being troubled with after-pains during the night; some coagula had been expelled; pulse 100; to be syringed with Condyl's fluid and water; to get a rhubarb pill at bed-time and dose of castor oil in the morning.

14th.—Slept well; discharge normal and free from odor; bowels had not acted; to get enema; pulse 90.

15th.—Bowels well moved after enema last night; did not sleep well; slight smell from discharge; no pain or tenderness over abdomen; uterus well contracted and normal in size; complains of indigestion and slight heart-burn; to be syringed every three hours; temperature normal; exceedingly nervous; to get a rhubarb and soda pill at bed-time, and if bowels don't act, an enema in the morning. On 16th received a telegram that she was going on nicely, and feeling so well that I need not visit her.

17th.—Going on well; still complains of heart-burn and indigestion; no smell from discharge, which is normal in quantity; no tenderness or pain in abdomen; tongue clean; temperature 98.4°; slept well; not to see her until 19th.

18th.—Sent for in afternoon; nurse reported that she slept well until 6 A.M., then complained of slight pain in the stomach, bowels not having been moved for twenty-four hours. She got two enemata; the second operated, and brought away some large, hard lumps. Pain increasing and vomiting of bilious matter setting in, I was sent for, and saw her at 4 P.M. At that time, she had not vomited for some hours. Her face was flushed; pulse 120; temperature 102.8°; marked tenderness over abdomen; complains of nausea; gave her liq. opii (Battey) ℥x., tr. aconite ℥ij., acid hydrocyan. ℥ij. in a small quantity of iced champagne, and ordered three doses of liq. ammon. acetat. with two ℥ tr. aconite every two hours, turpentine stupes to abdomen, and ten ℥ liq. opii sedative every two hours at 10 P.M. The temperature being 102°, pulse 120, and the pain and tenderness still present, she got pulv. opii gr. i., ext. belladonna grain one-half in pil. and ten grains quiniæ sulph. At 2 A.M. on the

19th.—The nurse gave ten grains more of quinine; at 8.30, pulse 100; temperature 101°; had slept well during the night; feels better; slightly deaf; no pain; tenderness almost gone; lochia free from smell, and fair quantity. No milk; slight pain in left side between ribs and hip; ordered five grains quinine every four hours.

At 5 P.M. temperature 100.6°; pulse 90; bowels moved four times; up to 8.30 P.M., two more stools; temperature 100°; pulse 80; inclined to be hysterical; linseed poultices to abdomen.

20th.—Had a bad night; did not go to sleep until 12, and awoke at 2 A.M. in pain, and suffering from heart-burn and sensation of lump in chest; at 6 A.M. vomiting set in, of green bilious matter, and continued incessantly till 9.30; at 10.30,

pulse 130; temperature 102.6°; tongue coated; pain considerable; slight tenderness; no enlargement of uterus; no smell from lochia; vomiting ceased during the day, and at 7.30 P.M. the pulse had fallen to 100 and temperature to 100.8°. Dr. Kidd, of Dublin, saw her with me in consultation at 10 P.M. Although she did not appear alarmed or nervous at seeing him, she flushed up, and at 10.30 her pulse was 130 and temperature 102°, and vomiting set in again, complained much of indigestion and heart-burn; passed a tolerably quiet night; pain slight; some perspiration; lochia present, quite free from smell; got four grains calomel.

21st, 9 A.M.—Temperature 102°, pulse 110. Vomits occasionally. At 11.30 bowels moved; discharge of the consistency of thick gruel, slate color, with small hard lumps through it, most horribly offensive; moved again at 5 P.M., quite as offensive. At 6 P.M. the temperature was 101.2°, pulse 108. Suffered from indigestion and vomited occasionally during the day.

22d, 9 A.M.—Had a fairly good night; tongue clean; temperature 100.6°, pulse 100; indigestion and heart-burn gone; no pain in abdomen; has had two stools in rapid succession, the last one large and chiefly composed of solid scybala; complains of pain under right scapula, which catches the breath; no crepitus, râles, or friction sound.

5 P.M.—Great pain under scapula; face flushed; breathing accelerated; no cough; temperature 100.6°, pulse 120. Has had no vomiting or indigestion; abdomen tympanitic; no pain or tenderness; uterus well contracted and not enlarged; a castor oil and turpentine enema brought away a large quantity of hard lead-colored sybala, moulded to shape of bowel.

10 P.M.—Has slept off and on up to this hour; breathing shallow; pain easier; pulse 110, temperature 102°.

23d, 3 A.M.—Trembling in limbs; much pain; breathlessness complained of; friction sound under right breast and under scapula.

8 A.M.—Bowels moved; fair quantity, consisting of broken-down scybala, foul smell and of gray color; pulse 120, temperature 100.6°; breathing and pain easier.

1 P.M.—Much worse; pulse 140, temperature 102.4°; very breathless, feels weak; no effusion into pleura, but friction sound distinct.

6.30 P.M.—Has slept since three o'clock; breathing easier; temperature 103.4°; has taken nourishment well.

10 P.M.—Slept till 9.30; breathing easier; pulse 138, temperature 101.7°; bowels moved; full of hard masses about size of a pea; vomited once.

24th, 4.15 A.M.—Pulse 124, temperature 100.6°; has slept well; perspired in early part of night.

9 A.M.—Pulse 120, temperature 100.4°; had two motions; fair quantity, consistency of gruel, gray color; pain much better; breathing easier; has some pain across loins; slight dulness on percus-

sion over posterior portion of right lung; clear in front; friction sound diminished; breathing audible in front and back; pain gone from thorax, but rather severe in lumbar region and round by margin of ribs on both sides; heart sounds clear and distinct; urine fair in quality, slightly albuminous, but as it was not drawn with catheter, not more so than would be caused by admixture with discharge.

6 P.M.—Passed a fairly good day; took nourishment well; pain in loins still troublesome; temperature 101.8° , pulse 120.

At 7 and 9 P.M., vomited dark-green stuff, after which pain passed away, breathing became easier, fell asleep; temperature 99.8° .

25th, 5 A.M.—Has slept on and off; rather restless; pulse 120, temperature 100.8° ; tongue clean; slept fairly well till 10.15 A.M.; pulse 120, temperature 101.2° ; breathing easy, pain almost gone; bowels not moved since 2 P.M. yesterday.

3.30 P.M.—She progressed favorably until this hour, when a change for the worse set in which lasted two or three hours; at 5.30 the pulse was 140.

At 6.30 P.M. she felt and looked better; pulse 120, temperature 99.8° ; vomited green stuff.

9.30 P.M.—Bowels moved; consistency of discharge semi-solid, but the smell was the most appallingly bad that I ever experienced; pulse 120, temperature 100.8° .

26th, 2.30 A.M.—Pulse 130, temperature 100.6° ; at 4 A.M. the pulse was 140; vomited frequently up to 7 A.M.; greatly exhausted.

Rallied considerably, but vomiting continued to 10.30 A.M., after which hour a decided improvement took place, the vomiting ceasing, the pulse getting fuller, stronger, and slower, while the temperature fell.

A consultation was held at one o'clock with two other doctors' when it was decided to endeavor to salivate the patient; to this decision I was opposed and declared my positive conviction that it would take away any prospect of recovery, pointing out that since morning the patient had been steadily improving, vomiting having ceased, all pain and tenderness having passed away, while the pulse was steadily falling in rapidity and getting fuller in volume, and that the temperature was keeping down, and that she could not bear to have her vital power lowered and depressed with mercury. I left at 3.30 P.M., up to which hour the improvement continued, the case being taken charge of by one of the other doctors. At about five or six o'clock calomel was administered every two hours and unguent. hydrarg. rubbed into the axilla; early in the morning diarrhea set in. I saw her at 12.40 next,

27th day.—On my arrival I was triumphantly told she was much better, that the mercury had taken effect, and that every bad symptom had passed away; the relatives were jubilant; on going into the room, one glance at the patient was sufficient—she evidently was in articulo mortis, unconscious and pulseless. I

have had few more trying tasks than the undeceiving of her husband and relatives, plunging them into despair from an assurance of her safety. She quietly died at 1.30 P.M.

The treatment may briefly be summarized. Efforts to keep down the temperature by quinine and salicylate of soda, turpentine stupes and poultices to the abdomen; opium by mouth and hypodermically to relieve pain, in the light of the pleuritic affection. Minute doses of mercury stopped at once on signs of reduction of the inflammation, and not carried far enough to depress the vital powers; nutrient enemata, poultices, and afterwards blisters to chest. The bowels kept regular, and nourishment given.

It appears to me, therefore, that we have evidence of septicemia, autogenetic and heterogenetic, in puerperal woman. That in addition, various causes can and do produce one and the same disease which is, I believe, a true puerperal fever. That these causes are also auto- and hetero-genetic, such as mental emotion and distress, and the infection of acute specific fevers. That independently of such causes there is evidence that puerperal fever of a highly contagious character occurs. That such fever, from whatever sources arising except septicemia, is a specific infectious disease, finding its suitable soil in the condition peculiar to the puerperal state, and that it occurs epidemically and sporadically like any other acute infectious disease, and that it is highly probable that it may convey a different disease to others, such as erysipelas, diphtheria, scarlet fever, septicemia. Not necessarily because it originated itself from any one of these, but depending either on some development of the poison, or some peculiar receptivity in the recipient with which we are not yet acquainted.

While this paper was passing through the press, I saw in consultation a marked case of puerperal fever produced by nervous excitement.

The patient, a young and healthy woman, was delivered of her second child after an easy labor. The child was a very small one, and, as I verified for myself by a careful examination, there was no injury to the ostium vaginae. She went without a bad symptom until the seventh or eighth day after her confinement, when she heard that her eldest child had met with an accident or was very ill. This proved to be a false alarm, but the report threw the mother into a state of extreme excitement; a few hours after she had a rigor, and all the symptoms of bad puerperal fever set in, from which she barely escaped with her life. I made a searching examination to find out, if possible, any other cause. No infection from zymotic disease, there was not a case

of any kind in the neighborhood; no possibility of sewage infection. The discharges were perfectly sweet. I satisfied myself there were no clots in the uterus; no wounds on the genitals; and although intrauterine antiseptic injections were employed, they did not reduce the temperature, and, seeming more injurious than useful, were discontinued. In fact after most careful inquiry and examination of patient, nurse, husband and doctor, I could ascertain no cause of her illness, which had all the symptoms of puerperal fever, save and except the fright about her child. When I saw her, her pulse was between 130 and 140, and her temperature 105.4. Quinine and intrauterine injections failed to lessen the fever. Twenty-grain doses of salicylate of soda with cold compresses to the abdomen produced an immediate effect.

REMARKS ON THE SIGNIFICANCE OF ANTEFLEXIONS IN PREGNANCY.

BY

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AFTER narrating at some length a case of anteversion of the uterus which produced hysterо-epilepsy (*vide N. E. Medical Gazette*, August, 1883), in conversation with a medical friend, he remarked that such a case was very satisfactory to have done with. It left a sense of having done a real service to the patient.

There appears to be a dubious state of professional opinion in regard to the influence of ante-displacements of the gravid uterus in the early months upon the progress of pregnancy and on the general health. Having found these displacements accompanied by the gravest symptoms, it becomes a matter of interest to ascertain if displacement and symptoms stand related as cause and effect. Authors believe that "these forms of displacements are very rare in the early months of pregnancy." (*Leishman, Cazeaux.*) The following case is offered as being in point:

CASE I.—Mrs. F. S., aged twenty-four, Ipara, slender, small figure, consumptive antecedents, toward the latter part of May, 1883, began to suffer from nausea, hemoptysis, and excessive leucorrhœa. Symptoms increased through June. Early in July she

became greatly weakened by repeated vomitings, hematemesis, hemoptysis, and inanition. The sympathetic nervous system was profoundly and dangerously disordered. Her pulse sank to 44 beats in the minute. I first saw her July 19th, and made a digital examination per vaginam. Found the fundus low down against the pubes, the *os* high and well back against the rectum, soft, patulous, and eroded; much leucorrhœa. At next visit, applied a strong solution of arg. nit. to the *os*. July 27th, symptoms slightly ameliorated; began faradization of the stomach twice a day. August 9th, considerable improvement, but still cannot sit up or stand. As a preliminary to the application of a Thomas' anteversion pessary, the uterus was turned well back, and held in retro-position for several minutes. August 10th, the above-named pessary was applied, and she was ordered to get up a little next day and every day, and gradually accustom herself to voluntary exercise. Massage had been in use in conjunction with faradism for previous two weeks. From this time I did not see her until October 8th, when she was brought in a buggy to my office, a distance of twelve miles. I did not remove the pessary, but ordered it to be taken away as soon as she could have opportunity for quiet and rest. She removed it herself October 10th. She continued to go about after its removal, and had fairly comfortable health till February 15th, 1884, when she was delivered of a girl baby after a hard but natural five hours' labor, vertex presentation. Leucorrhœa continued all through pregnancy. She is in better health now than previous to pregnancy, and writes, April 13th, 1884: "Last fall when I began, being able to walk about a little by the use of a cane, I weighed eighty-five pounds, and to-day I tipped the scales at one hundred and twenty-three pounds."

Remarks.

Theoretically, I could wish that the pessary had been used at least a month earlier in this case; but when I first saw her, her strength had been so far reduced by vomiting, hemorrhage, and inability to eat that it seemed wise to proceed with much caution until something of a rally should be made. Before I saw her, this patient had gravitated into a strict dorsal decubitus as a self-recognized necessity. Other than for a few tentative efforts, this position was steadily enjoined by me until after the pessary was applied.

Scientific diagnosis and treatment are not and cannot be fully satisfied by clinical observations that vomiting in pregnancy is relieved by diligent faradization of the stomach. A knowledge of the ultimate pathological error causing the sickness may render it evident that electricity, though it be able to qualify or even suppress the symptom of reflex nervous

disorder, is yet incapable of removing the organic cause. Therefore, let us seek to know the morbid organic element, not solely with a view to allaying present suffering in the case in hand, but to forestall a morbid evolution or development. The application of nitrate of silver to the ulcerated *os* in cases of severe vomiting in pregnancy has long been accounted a judicious and successful treatment. But the first of exciting causes of ulcerated *os* in general, according to Thomas, are displacements, flexions of the organ itself. Evidently, in a case like the above, efficient treatment for the displacement approaches, as nearly as our knowledge can yet lead us, to the removal of the first cause of all the offending symptoms.

I have reason to believe that these displacements are not so rare as has been taught. I have observed two cases of abortion in the second month with forward displacement, one of which I treated unsuccessfully for several weeks before the completion of the accident. This latter case, however, would not keep quiet, and no artificial support was used.

CASE II.—Mrs. E. C. C., aged thirty-six, IVpara, in March, 1879, when five months gone in pregnancy hurt herself by leaning over the rail of a calf pen. Immediately after she felt violent pains darting through the uterus, and followed by a flow of blood. Fetal movements ceased, she began to diminish in bulk of abdomen, and there was a continual fetid discharge through the vagina. Her general health suffered considerably, but there was no local pain or evidence of labor until after she was brought to my office on June 27th, more than three months after her accident. I found the *os* closed and located rather high up and backward toward the rectum, with a sharp flexion at the upper extremity of the cervix, the fundus being depressed low against the pubes. I returned the uterus to its natural position by bimanual manipulation. Ordered a bandage to keep the fundus elevated as soon as she should reach home thirteen miles distant, for she would not consent to remain in town over night. Symptoms of labor came on within half an hour after she left my office, and on reaching home, as she reports, she applied bandage as ordered, which had the evident effect of increasing her pains, followed in a few hours by the birth of the fetus and membranes, the latter being badly decayed.

In the absence of knowledge in this case, it seems fair to presume that anteflexion had existed during the whole of the time since the death of the fetus, and that this displacement had not simply retarded labor, but had in fact prevented its beginning.

In conclusion, I would echo the sentiment expressed by Dr. Gehring in his suggestive and helpful article (in this JOURNAL, July, 1882), that "forward displacements should be studied more carefully in their bearing on pregnancy, miscarriage, fetal position and labor, than they have been studied heretofore."

REMARKS ON THE CAUSES OF STERILITY.

BY

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THE scope of the subject of sterility is so large that I propose to take up only a few of the salient points for our present consideration. While the subject has been studied, no doubt, from the time man was created, it is probable that little advancement was made previous to the time that is within the memory of many of our older practitioners. Hippocrates, that original and profound observer, 2,500 years ago treated the sterile woman with a skill that, though it may fall behind in the great advancement of our modern science, was founded on the very principles that are acknowledged at the present day. Hence, in details of causes and methods of treatment, we may lay claim to originality, yet the grand principle of enlarging the cervix and stimulating a neurotically imperfect set of generative organs appears to have been comprehended, though perhaps crudely, by him. Even incision of the os and cervix for their enlargement was practised 200 years ago by that sage, yet reticent practitioner, Mr. John Hall, whose papers, unpublished, were purchased by a Mr. Cook, of Warwick, and by him published after Hall's death.

Other observers have written on the subject, but to Sir James Y. Simpson is due the credit of bringing the operation of incision prominently before the profession in 1843, since which time the profession has been divided for and against this and other methods of treating sterility.

Barrenness is a condition which is found to exist in the male as well as in the female, and perhaps more frequently in the

former. Grünewald estimates that one woman in eighteen is sterile. Matthews Duncan believes a still larger number are barren, namely, one in ten. This same author gives us the startling figure that of males in the human family one in six is sterile. These figures, viz., one-tenth in females and one-sixth in males, would render the number of productive marriages not over one in five or perhaps less. I cannot but believe that this is too large a percentage for any country or race—certainly for this country. However this may be, it is certain that a large percentage of males are sterile, as our lamented Sims has clearly shown, and that to subject a woman to a long course of treatment for sterility without a careful examination of her husband would be exceedingly improper and unscientific. The language of Dr. Sims is especially severe against those who are guilty of such failures, certainly none too much so, as all will admit.

It is my purpose, however, to omit the consideration of sterility in the male, and confine my remarks only to the subject as it exists in females, all the while admitting, of course, that these two subjects of study are of equal importance, and that any marriage which is unproductive requires our careful scrutiny as to the condition of both parties.

While it is easy to examine and study the physiological action of the male generative organs, it is not so with the female. Indeed, we may safely say it is impossible. Of all that has been written on the normal action of the female organs during and subsequent to copulation, very little is positively known. It is true that we have plenty of theories, almost as many as we have writers. The action of the uterus in receiving and transmitting the spermatozoa is the least understood of all, owing to the fact that the nature of the subject is such that its study is almost or quite impossible. Difficult as the subject is, however, there are a few points which have been pretty well determined, and which we will proceed to mention.

Before doing this, it may be well to note that the old doctrine so long in vogue, that the spermatozoa were endowed with some sort of intuition or instinct—that they would turn in the direction of the os uteri, wading through the acid mucus of the vagina, travel patiently upward and around the

vaginal portion of the uterus, enter the uterus, and proceed onward in search of the waiting ovum, is still indorsed by a few who certainly deserve the reputation they have received of being careful observers. Particularly among these may be named Dr. W. C. Roberts, of New York (see *Medical Record*, Vol. IV., p. 318), whose language is so strong that I cannot refrain from quoting a few lines.

He says: "A deposit of semen upon the vulva is sufficient. . . . Their volition (the zoosperms) is seemingly directed by instinct towards, and in spite of all obstacles, the ovum which they are to impregnate." This was written as late as 1869. Whether he has since changed his views, I have no means at present of knowing. Since that time, more particularly, numerous contributions on the subject have been given of physiological doctrines at variance with such views which it will be well to briefly examine, for it is readily conceded that a correct physiology often and generally leads to corrections in practice. For our present purpose, we shall examine only two or three points, because they are important and comparatively recent, though, as I think, fairly established, because they are indorsed by some of our best gynecologists.

About the year 1859, a German by the name of Eichstedt announced his belief that the uterus during copulation was not the passive organ it had almost universally been taken to be; that it had an action of such a nature that it was capable of sucking in or aspirating the semen in the very brief period occupied in the act of coition. The skill with which he argued his points naturally excited the attention of others, prominent among whom was Wernich, also a German, who published a paper in 1872 entitled, "The Erectile Properties of the Lower Segment of the Uterus and Its Significance;" that the body of this organ was not composed of true erectile tissue as had been previously advocated by Rouget; that this erectile power resided only in the vaginal portion or neck; and that this erection was the cause of the aspiratory movement. He also followed Hohl, that during congress the cervical portion elongates materially, and that the whole organ sinks far down in the pelvis as if to meet the glans of the male. In the same year, only several months later than the one of Wernich just alluded to, a rather remarkable case was reported in

the *St. Louis Medical and Surgical Journal*, and later elaborated in the *AM. JOUR. OF OBSTETRICS* for Nov., 1874, by Dr. Joseph R. Beck entitled, "How do the Spermatozoa Enter the Uterus?" Dr. Beck had no knowledge of the existence of Dr. Wernich's paper, and was so far original. His case clearly demonstrated the aspiratory action of the the uterus. The case was an excitable woman whose orgasm was very easily excited by an ordinary vaginal examination. After learning this, he wisely concluded to take advantage of it to note whatever changes might take place during the height of the venereal excitement. What followed in his examination, I beg to give in his own words: "The os and cervix uteri had been about as firm as usual, moderately hard, and, generally speaking, in a natural and normal condition, with the external os closed to such an extent as to admit the uterine probe with some difficulty; but instantly that the height of the excitement was at hand, the os opened itself to the extent of fully an inch, as nearly as my eye could judge, made five or six successive gasps, as it were, drawing the external os into the cervix each time powerfully, and, it seemed to me, with a regular rhythmical action, at the same time losing its former density and hardness, and becoming quite soft to the touch. Upon the cessation of the action as related, the os suddenly closed, the cervix again hardened itself, and the intense congestion was dissipated."

This case was not remarkable in one sense: that of the voluptuous excitability of the patient, for every one who practises gynecology meets with them, though the intense application of the physician in the study of his case prevents his notice of it. Again, Dr. Paul F. Mundé, in 1883, distinctly enunciated in the *AM. JOURNAL OF OBSTETRICS*, Vol. xvi., p. 846, his own careful observations on this point. His own language is as follows: "We ourselves have seen the gushing almost in jets of clear viscid mucus from the external os during evident sexual excitement, produced by a rather prolonged digital and specular examination in an erotic woman (a 'femme entretenue,' a blonde Swede). The lips of the external os alternately opened and closed, with each gaping emitting clear mucus, until the excitement (which we confess to having intentionally prolonged by gently titillating the cervix with a

sound through the Sims speculum) reached such a height as to cause the woman to sit up on the table, and thus end the experiment."

For some time I have taken no little pains to observe very carefully the action of the female generative organs, whenever a favorable opportunity offered, and while I have never noticed the violent spasmodic action as described by Dr. Beck (and no others have that I know of), I *have* observed the hardening and elongation of the cervix: the sinking of the uterus downward toward the vaginal orifice, the moderate yet distinct enlargement of the os uteri, and the *gradual* return to the normal condition—all this not in a single but many instances.

From my own observations I wish to offer, summarily, a theory of the manner in which this aspiratory action is accomplished. The influx of blood in the process of erection (whether of the cervix alone or of it and the body together, probably of the neck alone, as the anatomy as given by Henle and others would indicate) not only elongates and enlarges the neck and at the same time slightly everting, or in other words enlarging the os; but it also, as is readily conceived, greatly narrows the cervix and for the time being almost obliterates it, forcing out, by an ejaculating process, any mucus it may contain: then, as it returns to its normal condition, it draws in whatever is within its reach, mucus and semen alike. Therefore, if the semen be discharged where it cannot be reached in this uterine reaction, it is lost. This aspiration, I am confident, continues for a minute or so after the orgasm had passed, and after fully passed, no inherent action of the spermatozoa can carry them to the body of the uterus.

Rusconi, only a few months ago, expressed decidedly the belief that semen abandoned in the vagina can never make its way into the uterus (*Medical and Surgical Reporter*, March 1st, 1884). He deems the fact demonstrated by injections of the uterus after death. Sims, Kolbert, Rouget, Pallen, and others bear the same testimony.

One point I wish here to iterate, founded on my repeated observations, viz., that the aspiratory movements of the uterus are not spasmodic, but comparatively *slow and gradual*, and I doubt not very powerful. I am aware that Dr. Beck's case stands opposed in a certain sense to this theory, and yet per-

haps not really, inasmuch as the gradual aspiratory movement may have continued after the spasmodic action had ceased in his patient.

Interesting as this part of our subject is, we must abandon it for want of time, and proceed as rapidly as possible to the consideration of a few of the causes which a somewhat lengthened experience has found to exist.

I shall divide these into mechanical, pathological, and neurotic, to which must be added faults in development. The last I do not propose to consider, as they are self-evident, and only need to be kept in mind in our examinations, in order that they may not be overlooked.

In regard to the question of *stenosis* of the os and cervix uteri as a cause of sterility, the profession is not agreed. Many illustrious names are to be found on the list of those opposed to the theory, some of them using very strong language in their opposing arguments. We may on this side mention such names as Henry Bennett and Dr. Head. As late as 1883, Grünewaldt, Matthews Duncan, and Baker Brown took decided opposing ground.

Nevertheless I am quite convinced that the larger number of gynecologists indorse fully the doctrine, as their practice of incision and dilatation clearly proves. My own belief is very clear that such cases do exist, and are indicated not only by the existence of sterility, but by the impossibility of introducing the smallest sound, as well as by the intense pain at every menstrual period. It is proper to remark just here that dysmenorrhea is not positive evidence of such a degree of stenosis that the woman cannot conceive, for it is well known that many of them do, and further, we have the testimony of Sir James Y. Simpson as early as 1859, and others, that the menstrual flow may be so rapid that an ordinary-sized canal cannot carry it. It is scarcely necessary to mention further the fact that pure neuralgia may cause the pain. Hence sterility caused by a closed uterine canal *must* be accompanied by dysmenorrhea, but the latter need not be accompanied by sterility.

Flexions and versions of the uterus may be causes of sterility, by obstructing the entrance of the fecundating fluid. I think, however, these causes are very rarely sufficient. In order to be so, they must be quite extreme in degree. The testimony

of Scanzoni, Grünewaldt, Roberts, Joulin, and others decidedly supports this view. I take it that Sims also entertained about the same view. In support of this belief, it would seem clear that, while the cervix is in a state of erection, the point of flexure would be in all probability almost completely removed, and the entrance of the spermatozoa permitted. Reasoning also, in the case of versions, if the sperm be deposited in the immediate vicinity of the os, no matter what its location, its aspiration would be successfully accomplished; if not, it would be lost.

A conical cervix I need not allude to, as it is rarely met with. Scanzoni thinks it is no cause for sterility. His pupil, Mundé, thinks it is, and his opinion is coincided with by most practitioners, as I believe. These are the more common of the mechanical causes, and time will not permit me to even allude to others.

Endocervicitis and endometritis, like the other causes spoken of, have been argued for and against by different members of the profession. I cannot but believe them to be decided and very common causes, notwithstanding the contrary opinion entertained by Duncan (see *JOURN. OBSTET.*, Sept., 1883, page 985). It must be admitted that the lining membrane of the uterus must be in a perfectly healthy condition in order to retain the impregnated ovum; that in the catarrhs and inflammations of this organ, particularly of its lining membrane, it is not in a healthy state, and hence it is unprepared to retain and nourish the fertilized germ. Further, the secretions from such a diseased condition may be and probably are of such a character that, as Sims clearly showed, the spermatozoa are quickly destroyed. Various other causes there are which will be passed over.

We come now to speak of the neurotic causes, those which have solely to do with the nervous supply of the female generative organs—a class of cases which have never to my knowledge been described as such, but which from their frequency and importance I deem worthy of notice. In my experience they occur about as frequently as any from any other cause, and I doubt not are often met and treated successfully without the knowledge of the gynecologist as to the particular nature of the case he is dealing with. It is not, therefore, on account of any peculiar difficulty in the management of such cases or

of the great importance of the diagnosis, but simply to call professional attention to the fact, as I believe it to be, that a more conservative treatment may reasonably be expected to be as successful as the, at present, more popular heroic management. Conservatism, we are all happy to know, is finding its place in all branches of our great science, fortunately, as I think, in gynecology. Let us take a typical case: A woman presents herself on account of her being sterile. She is of proper age, and has the appearance of health. Inquiry is carefully made as to her previous history; nothing of importance connected with the object sought is elicited. A digital examination of all the generative organs reveals nothing that may be deemed abnormal; examination with the speculum yields the same negative results. The sound is introduced and is arrested at the normal distance. The uterine secretions are carefully tested and found perfect. With still greater precaution she is asked to come very soon after a congress; the spermatozoa are abundant. Now what is the cause of her sterility? It seems to me that but one answer can be given; namely, that it is a peculiar condition of the nerves supplying the uterus, an abnormal condition. We look for similar conditions of nerves supplying other organs of the animal body, and we find them in the olfactory, the auditory, and the gastric branches of the pneumogastric, also in other organs; either an exalted or depressed sensibility or an indescribable condition called *altered*. Many others undoubtedly have found such cases; I certainly have, and I at once resort to such treatment:—any treatment that will produce a shock locally and thus perhaps restore the nerves to a normal state. It may have been, after all, an undiscoverable disease, it is true, and if so, why does the simple introduction of the uterine sound, say twice a week during the second and third weeks after menstruation for a month or two, result in conception? A cutting instrument or a piece of solid caustic, or a few sponge tents might have done the same thing, but undoubtedly in the same way, namely, by the shock produced, and the resulting change in the nervous sensibility.

I omitted to mention, in its proper place, another cause of sterility found only in women who have already borne children. I refer to lacerations of the cervix. A laceration favorably situated will not always prevent conception. I think Dr. Mur-

phy, of the Columbia Hospital for Women, is of the same opinion. See his Report for 1873. It seems evident that if the rent is not too deep, and if the sperm is deposited in or immediately adjacent to it, the aspiratory movement might appropriate it; whereas, if the rent was situated, for instance, posteriorly and the uterus anteverted, the fecundating fluid would be lost. The same would happen if the laceration and version were reversed.

I shall omit any notice of other causes and proceed as briefly as possible to the consideration of a few points in the treatment, and in doing this I shall only take up those cases which result from causes that have been noticed in this paper.

In cases where actual stenosis of the uterine canal exists, the appropriate treatment at once suggests itself, namely, the enlargement of the canal. The method of doing this admits of great difference of opinion. Since the time that Sir J. Y. Simpson called attention to the propriety of incision, I think this has been the prevailing practice, especially since the powerful indorsement of Sims. To these are to be added such names of Hewitt, Heywood Smith, Barnes, Thomas, Schröder, Emmet, Mundé, and many others. On the other hand Dr. Goodell strongly urges against cutting. Dr. Reamy, of Cincinnati, gives the results in 15 cases in which he operated by incision, 3 became pregnant, one of whom aborted, making of successful cases cases only about 13 per cent, and concludes the operation unwarranted, often inducing conditions more to be deplored than the original trouble, as eversion, etc., and is attended with danger. It is well known that Dr. Tilt opposes it except in very rare and exceptional cases. Dr. Peaslee says, "the operation is hazardous in consequence of the change in the shape, size, and relations of the whole uterine cavity by the deep incisions, profuse and sometimes fatal hemorrhage, pelvic cellulitis and septic peritonitis, and a decided tendency to abortion." Chrobak fully indorses such belief. Dr. Gream says, the operation is a failure except to produce cellulitis, hemorrhage, etc. We might quote from Henry Bennett, Matthews Duncan, Dr. Head, Grünewaldt, and Baker Brown, and give nearly the same language. Hence, as the matter stands, the operation is decidedly unsettled, with, as I conclude, the tide against it.

Dilatation with sponge tents, bougies, etc., is another remedy that is argued for and against with about the same vehemence, so that practitioners are left without any positive sure guide. My own experience (and I have often performed both) is decidedly in favor of carefully and slowly dilating with metallic bougies, Peaslee's for instance, doing this always and only during the second and third weeks after menstruation. Such a procedure, in my experience, is surely harmless, and as successful as any other. Mann and Chamberlain use preferably the uterine sound, and I have several times succeeded with it, leaving it or the bougies in place a few minutes at each operation. Mundé is also authority on this point. Joulin strongly favors dilatation. Goodell has had no accident in 130 cases. Dilatation may be done, of course, with laminaria, gentian, or sponge tents. These last, however, I believe to be decidedly objectionable on account of their tendency to favor sepsis to a greater or less degree. It is true they were used by Henry von Roonhuysen more than two hundred years ago, yet their age carries no merit, as we have now better material. They are opposed by Gream and others, and recommended by Joulin, Meisteler, Becquerel, McIntosh, and I am sure many others. Courty had several successes with them in sterile cases; Meisteler, seven out of nine, and McIntosh, twenty-four out of twenty-seven. Certainly good results.

Nothing need be said on the treatment of catarrhs, as all are familiar with it, and each has his preferences.

For the management of neurotic cases, very little is required to be said, as almost any operative procedure or application will often slowly change or alter the nervous state. Mundé virtually acknowledges this class of cases and the same line of treatment, and so does Duncan. One word in regard to lacerations of the cervix. I have never performed the operation of repair, because I never met a case where I deemed it necessary, the laceration not being sufficiently extensive. Looking at the physiological action of this important factor of the generative apparatus, namely, the uterine cervix, it would certainly seem reasonable that an extensive rent would be better if healed, and no doubt it would if the case was favorable and the operation itself well performed. That is, we must be sure that the parts are not so drawn together that a stenosis remains.

Even in such a case, an operation for enlarging the canal could be performed. I almost wonder that the language of Dr. P. J. Murphy, of Washington, should be so strong in opposition. However, many are opposed to his views decidedly, and are strongly in its favor.

A CASE OF INVERSION OF THE UTERUS WITH ADHERENT PLACENTA.

BY

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EARLY on March 19th, 1884, I was called in great haste to attend Mrs. S., who had just been confined by a midwife. Upon my arrival I was told that the child had been born half an hour before, after a short and easy labor, but that the after-birth could not be removed, for a tumor was in the way. I hastened to the bedside of the patient, and found a large pendulous tumor protruding from the vulva. Considerable hemorrhage was present. I immediately recognized the tumor to be the uterus inverted, with the placenta attached to the fundus and a large portion of the body. Further examination revealed that there was a good deal of strain upon the left ovary, which could very plainly be felt. I peeled off the placenta from the walls of the uterus, and bringing my fingers together in the form of a cone, applied steady but firm pressure upon the fundus, which could easily be distinguished by its dimple-like depression, and thus succeeded in replacing the uterus to its normal position. The patient was very much exhausted, lying in a semi-comatose state for about four hours, with pulse almost imperceptible. Stimulants could not be given, for vomiting was so violent that nothing whatever could be retained by the stomach for a considerable time. Finally the patient rallied, and the stomach became quiet. On the following morning the patient was quite cheerful, having passed a quiet night, with a pulse of 98 and a normal temperature. From this time the patient gradually improved, the pulse never exceeding 98, nor did the temperature rise above $99\frac{1}{2}^{\circ}$. The lochial discharge was of a very fetid character for some time, and warm vaginal douches, containing a weak solution of carbolic acid, were used twice a day.

The above case presents several noticeable features. The first is, that the uterus was replaced with comparative little dif-

ficulty. The second, that the patient, a Ipara, with a previous weak and anemic constitution, after losing considerable blood and receiving such a shock to her system, should make such rapid recovery without any untoward symptoms. The third is the immunity of septic infection, no intrauterine injections having been used, nor was the uterus washed with any kind of disinfecting fluid before its reposition.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF NEW YORK.

Meeting, January 15th, 1884.

DERMOID CYST IN THE PELVIC CELLULAR TISSUE.

DR. T. A. EMMET presented a specimen of dermoid cyst which had been removed ten days before from a woman about twenty-two years old, who had been in good health until puberty, but since that time had suffered from very severe dysmenorrhea, which had gradually reduced the general health, until she had become a confirmed invalid. There had been repeated attacks of pelvic inflammation. She entered the hospital in the autumn of the past year, and every means were resorted to to relieve her sufferings and restore health, but without success. All of his colleagues at the Woman's Hospital saw the patient, but were unable to make a diagnosis beyond that of pelvic inflammation. On passing the finger into the posterior vaginal *cul-de-sac*, the impression was received as of a tumor growing from nearly the entire length of the posterior wall of the uterus—possibly a fibro-cyst or a fibroid tumor. The patient had suffered so much and her mental powers were becoming so greatly affected that an exploratory abdominal incision was decided upon, with the hope of finding and removing the cause. At the operation, Dr. Emmet was surprised, on cutting through the peritoneum, to come upon a dermoid cyst in Douglas' *cul-de-sac*, below and entirely outside of the peritoneum, and without any connection with the ovary or any other organ. He then read the pathologist's report.

Dr. Emmet further remarked that it was impossible to find either ovary, so extensive had been the adhesions excited by repeated attacks of cellulitis. The left Fallopian tube, which seemed to be somewhat enlarged, was removed. He inquired whether the occurrence of a dermoid cyst in the pelvic cellular tissue, outside of the peritoneum, and having no connection whatever with the ovaries was not unusual. This was the only case of the kind which

he was able to recall. The case also illustrated the advantage often of opening the abdomen for diagnostic purposes.

DR. H. J. GARRIGUES said that without doubt the case was a rare one, but he would call attention to the fact that dermoid cysts were not always developed in connection with the ovaries, but often under the skin, and sometimes even in the internal organs. He was therefore not surprised that one should have been developed in the locality referred to. For the explanation we should probably have to go back to the beginning of fetal life, at which time the foundation of the condition was probably laid. We could easily imagine that at this time some small portion of the epiblast became included in the mesoblast, and, in the course of time, becoming separated from the epiblast, might develop into the form of a dermoid cyst in any portion of the interior of the body.

DR. W. M. CHAMBERLAIN thought that the late Dr. Peaslee, some seven or eight years ago, presented a specimen of dermoid cyst of about the size of that presented by Dr. Emmet which had developed anteriorly and beneath the peritoneum in the iliac fossa.

UTERINE CASTS.

DR. GARRIGUES presented a specimen with the following remarks: When fresh, it was 6 cm. long, 2.5 cm. wide, 1.3 cm. thick, and of a pyriform shape. The outer surface was grayish; the cut surface was of a dark-red color; it was soft and stringy, and appeared very much like a fibrinous clot. The odor was offensive. The physician who brought the specimen to him said it was one out of a hundred, meaning by that that many similar ones had been passed. The patient said there came one after each menstruation, but added that it came only in case there had been coition. If, during the whole period between two menstruations, no connection took place, none would come. Menstruation was always somewhat protracted. The present body had come seventeen days after the beginning of a menstrual flow. Dr. Garrigues had examined part of the specimen, and had found that the sections, by removal from one saucer to another, would constantly break. The microscopical examination showed it to be rather peculiar. It consisted of a net of fibrin filled with small round cells. Consequently it was not a blood-clot. The blood was gone, so to speak: the fibrin had been left and was full of cells. The question arose: where did the small cells come from? So far as he knew, red blood-corpuscles were never changed into such cells. Might they not have been due to wandering blood-corpuscles from the walls of the uterus, or to proliferation of the colorless blood-corpuscles originally contained in the clot? It resembled connective-tissue stroma, but the network was composed of fibrin and not of connective tissue. Compared with other specimens which had been presented to the Society, it showed the importance of a microscopical examination of bodies passed from the uterus. About three years ago, Dr. Skene presented a so-called clot which, upon microscopical examination, Dr. Garrigues found to be composed of placental tissue. Subsequently he had reported on an-

other body passed from the womb, which proved to be uterine tissue, illustrating dissecting metritis, eight examples of which he had now seen. The specimen now presented might easily be mistaken for the product of conception, on account of the patient's assertion that such bodies were expelled only after coition.

DR. CLEMENT CLEVELAND had to-day received a specimen the gross appearance of which resembled very much the one presented by Dr. Garrigues; it looked like uterine tissue. The patient was twenty-seven years of age, and had suffered regularly from intense dysmenorrhea for years, the pain coming on before the onset of the flow. The clot was passed every month. There was anteversion.

DR. GARRIGUES thought it probable that the specimen consisted of the membrane thrown off in membranous dysmenorrhea. There was another form of body cast off from the uterus, known to be due to exfoliative endometritis, three cases of which had been described by Kubasson (*Zeitschr. f. Geburtsh. u. Gynäk.*, ix., 1883, p. 310). The whole mucous membrane lining the uterus was expelled in the form of a sac. The cast was so complete as even to have taken with it a thin layer of muscular tissue beneath the mucous membrane. It had nothing to do with menstruation.

DR. CLEVELAND remarked that the specimen to which he referred bore no resemblance to the membrane sometimes discharged by those suffering from dysmenorrhea.

DR. A. S. CLARKE was reminded of a case recently observed of which he had not seen an example before. The patient, a young woman, married, had had one miscarriage, and complained of very great pelvic and uterine uneasiness. Sharp antelexion was found, similar to congenital antelexion. He attempted to effect dilatation by means of a laminaria tent, preparatory to the use of a stem pessary, but the canal was so tightly closed that he was unable to introduce the tent up to the fundus. During the night it was pressed out, and was not replaced. The evening of the next day, he was sent for, and, on his arrival, was shown a mass, similar in size and appearance to that presented by Dr. Garrigues, having a very offensive odor. It had been passed from the womb. The central portion was perforated by a canal corresponding to the uterine canal. Dr. Clarke was of the impression at the time that it was composed of inspissated mucus which the uterus had been excited to discharge by the presence of the tent. The patient was afterward much relieved.

RUPTURE OF AN OVARIAN CYST, THE CONTENTS BECOMING CLOSED IN BY PERITONITIS.

DR. T. A. EMMET related the following case: In July last, a woman with a supposed ovarian tumor was brought to his office from Memphis, having been taken from her bed during an attack of peritonitis, and arriving more dead than alive. She was placed under Dr. Harrison's care, and it was determined, if she recovered sufficiently from the peritonitis, to perform ovariectomy, which was accordingly done in the middle of July. Dr. Emmet found, to his surprise, on cutting through the abdominal wall, that he had opened directly into a large space. The fluid, which was dark, not unlike ink in appearance, was emptied out apparently from the

abdominal cavity and not from an ovarian sac. As soon as the cavity had been washed out, numerous long and large strips of membrane, apparently composed of the walls of the sac, were readily peeled off. On reaching the portion covering the stomach, there appeared to be another cyst, but it proved to be the distended stomach. The case evidently was one of ovarian tumor which had ruptured, and the contents become encysted by the products of inflammation; the walls then rotted, or softened, and could be peeled off in the manner mentioned. He had never seen a similar case. Dr. Harrison would give the result and the history of the after-treatment.

DR. G. T. HARRISON said that after the operation he continued to irrigate the cavity very freely with an antiseptic fluid, and it sometimes required one hour and a half before the liquid came away clear. This was continued for some weeks. Finally the patient was able to sit up, had a good appetite, and was doing well, when symptoms of intestinal obstruction developed. Dr. Lee, and afterward Dr. Weir, were called in consultation. Dr. Weir opened the abdomen and, as was believed, relieved the constriction. But it proved not to be so. The intestine was then opened and stitched to the abdominal wound. A further exploration was contemplated, if necessary, the next day, but the patient died in the mean time. At the post-mortem, an adhesive band, stretching from one coil of intestine to another, was found to have been the cause of the constriction. Had it been discovered during life, the patient might easily have been relieved.

DR. POLK said a very important question was raised by this report, namely, as to just how far we should interfere in searching for an intestinal obstruction. There could be no doubt that, when the surgeon searched for any great length of time, feeling around in the abdominal cavity, he jeopardized the patient's life by that procedure alone, and yet, on the other hand, if he did not do so, he was liable to have the mishap which occurred in this case. The question had been raised before, but its importance would justify further discussion, if any of the members had any suggestions to make.

PAPILLOMATOUS OVARIAN TUMOR INVOLVING THE PERITONEUM.

DR. DAWSON reported a case as follows: About a year ago Dr. Griswold sent a patient to his office with a view of ascertaining the character of a mass which he had detected in the pelvis. Dr. Dawson made an examination, and found a slightly fluctuating mass occupying the whole left pelvic region, somewhat movable, but not sufficiently so to point distinctly to a free ovarian cyst. The patient complained only of occasional pains and some dysmenorrhea. That the mass contained fluid seemed very evident to Dr. Dawson, although Dr. Griswold believed differently. He made arrangements to see the patient again within a few weeks, with a view to proving the fluid nature of the tumor. He was then disposed to think it an ovarian cyst, but aspiration was de-

cided upon to make the diagnosis positive. About a pint of cloudy fluid was withdrawn, which, under examination, pointed to an ovarian cyst. The patient was a strong woman, in good health, and was advised to let the tumor alone for the time being, and report further. She returned with Dr. Griswold last autumn, saying that she suffered a great deal of pain. The mass was found to have become considerably larger. It was still rather immovable, and not connected with the uterus, and, from the history of the case and the examination of the fluid, he was still of the opinion that it was an ovarian cyst. Dr. Lee saw the patient, and concurred in the diagnosis. The patient was told that, if her condition demanded relief, she might return, and an exploratory incision would be made. She returned on the 19th of last month complaining of marked distress. A purely exploratory opening was determined upon. An incision two inches and one-half long was made, and it was noticed that at the lower angle of the incision there was a decidedly papillomatous appearance. All the gentlemen present were immediately struck by the papillomatous development of the peritoneum which had been incised. As was expected, an ovarian tumor was found firmly adherent to the intestines and to all surrounding parts, and Dr. Dawson decided not to remove it, but to sew up the abdominal wound and, if necessary, establish drainage by the vagina subsequently. The temperature did not rise above 100° F. at any time; in eight days the patient was sitting up, and on the twelfth was walking about, no unfavorable symptom having developed. As the general health was becoming affected, and the patient showed the peculiar *facies*, a rapid destruction of life was looked for. Very strict cleanliness was observed, the towels and the napkins were carefully washed, the patient was bathed, the operating-room was scrubbed, all under Dr. Dawson's immediate supervision. But, further than strict cleanliness, antiseptics were not employed.

DR. EMMET remarked that there was generally but little vitality left in these patients, and they often died on the operation-table.

DR. DAWSON thought it would have been better for his patient had his expectations been fulfilled, and she had died from the operation. But, as it was, she made a remarkably good recovery, and that notwithstanding he had been unexpectedly compelled to employ improper sutures in closing the wound.

SUPPOSED ACUTE YELLOW ATROPHY OF THE LIVER WITH PREGNANCY.

DR. CHAMBERLAIN gave the history of the following case, for the purpose of asking a question regarding diagnosis. He had been called in consultation to see a primipara in the middle of the seventh month of pregnancy, who had been attacked with intense pain across the epigastrium. No cause for indigestion, nor for the agonizing pain located at the epigastrium had been recognized. It was shortly followed by colliquative diarrhea to the extent of

seven or eight passages. Then there came dark vomiting. These phenomena perhaps occupied the first twelve to fourteen hours. When he first saw her, she was decidedly pallid, and was very tender across the epigastrium, but not lower down on the abdomen. She was of small frame, and the tension of the abdomen was such that little could be determined by palpation and percussion. He believed the case to be one of icterus malignus, or acute yellow atrophy of the liver—a very rare disease, but which in at least half the recorded cases had occurred in the young parturient woman. The icterus increased, the pain in the epigastrium did not diminish, and could only be controlled by morphine. The diarrhea ceased, and the patient passed into a typhoidal condition. He had expressed the belief that premature labor would occur. Labor began on the third day, and was completed within about sixteen hours. Having seen but one case of acute yellow atrophy, in which the patient died the second day after delivery, he had but little doubt that such would be the issue in the present case. Although the pains were weak, yet the pelvis was roomy, pregnancy had advanced to only seven months and one-half, and delivery was effected by the unaided efforts of nature. As it had been noted that hemorrhage usually attended such cases, the patient was given a full dose of ergot just before delivery. From the day of delivery the woman improved. At the time of delivery the temperature was 103° F., the tongue was brown, the intellect wandering. She gradually came out of this condition, and was now believed to be convalescent. So far as reliance could be placed upon a physical examination, under the circumstances, the left lobe of the liver was retracted. The question arose whether this was really a case of icterus gravis, or acute yellow atrophy, in which recovery took place. An interesting feature in the case was, that, although delivery had been completed naturally, he was much surprised to hear the next day that there was a certain black swelling upon the vulva. Her family physician, upon examination, found the surface of one labium gangrenous, and this afterward sloughed. From the low typhoidal condition of the patient, and from the presence of a constantly breaking-down mass of gangrenous tissue in the vulva, he thought they were in a very fair way for all the sequelæ of sepsis, but there were none. The tissues, as fast as they broke down, were detached, and antiseptic irrigation was employed.

DR. A. JACOBI asked what the condition of the urine was.

DR. CHAMBERLAIN said there was no tryrosin, leucin, nor albumin. There were a few blood discs. In the case of acute yellow atrophy of the liver already referred to, the vomiting and the diarrhea did not cease until death.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF PHILADELPHIA.

Special Meeting, June 6th, 1884.

Vice-President, DR. B. F. BAER, in the Chair.

DR. WM. GOODELL exhibited specimens of

PYO-SALPINX AND HYDRO-SALPINX.

In the former case the lady was unmarried and had suffered from pelvic pains and menorrhagia for several years. Last autumn a tumor was discovered by her physician, who deemed it a fibroid of the womb. Early this year her sufferings became so great that she took to her bed. Very large doses of morphia were needed, and septic symptoms now set in. After she had been in bed for several weeks, Dr. Goodell was called in to see her. The tenderness of the abdomen was now so great that the examination was made under ether. Even then the diagnosis was obscure because she flinched, and her recti muscles became tense whenever the abdominal wall was pressed upon. A cyst was discovered, but of what nature it was impossible to determine. Dr. Goodell operated on her at his private hospital. The womb was studded with small fibroid nodules, posteriorly it had an outgrowth as large as a small egg. Closely adherent to the womb, to the pelvic fascia, and to the intestines, was a thick-walled cyst of the left ovary as large as the largest orange. The corresponding oviduct was very thick and enlarged to the size of a small sausage. It and the cyst were filled with a very dark purulent fluid, although there was no communication between them. The lower end of the cyst had become necrosed and was so thinned out that it would very soon have given way at that point. On account of the presence of fibroids in the womb, the right ovary was also removed. Attached to the fimbriae of the oviduct were three very beautiful pedunculated vesicles, while two others not yet pedunculated lay in the stroma of the broad ligament. The recovery of the lady was uninterrupted.

In the case of hydro-salpinx, the patient was a widow, aged thirty-seven who had been sent to him in order to have her ovaries removed. Severe pains began a week before the menstrual flux, culminating during the flow and continuing one week longer, then fading gradually away. For three weeks out of every month she was confined more or less to the recumbent posture, and wholly so during the menstrual week. A tear of the cervix and one of the perineum had been well repaired by two surgeons, but with no improvement. Dr. Goodell wished her at first to try the rest

treatment with massage, electricity, and graded muscular movements, for he had repeatedly cured cases of this kind through such a mode of treatment. She was, however, too poor to take this treatment privately, and therefore was urgent to have her ovaries removed. The operation was performed fifteen days ago, and she is now doing very well indeed. The ovaries as exhibited were much enlarged and showed marked follicular degeneration. From this condition Dr. Goodell thought that nothing short of the operation would have cured her. Attached to one oviduct was a delicate vesicle with a thread-like stem of over an inch in length. In view of the frequency with which they are found he could not but think that these vesicles played some rôle in the economy, and that they had sometimes a pathological bearing. He had on several occasions met with small post-uterine cysts which burst either spontaneously or under the pressure of an ordinary vaginal examination. Taking advantage of this fact he had quite recently burst one designedly by bimanual pressure. Such delicate cysts, and also those very movable ones, which remained small without increase in bulk, he was disposed to attribute to these vesicles. After bursting, these cysts sometimes refill. One he had known to burst and refill at least six times before it disappeared. Now small ovarian cysts had, in his experience, thick walls, and, further, they rarely remain small any length of time. Dermoid cysts, on the other hand, often remain stationary for years, but they were generally not very movable, and they also had thick walls.

DR. ALBERT H. SMITH had found these cases of pyo-salpinx very difficult of diagnosis. He had been present at an operation by Knowsley Thornton upon a case in which the lesion was double, and both tubes and ovaries were removed. Rupture had occurred previously, and had been followed by peritonitis. The patient recovered.

DR. B. F. BAER inquired if Dr. Goodell would recommend rupture of cysts arising from the hydatids of Morgagni?

DR. GOODELL would consider it good surgery for the purpose of preventing the further growth of the cyst. He had always found the fluid in small cysts to be unirritating.

DR. ALBERT H. SMITH remarked that Schroeder holds that the fluid of an ovarian cyst is not noxious to the peritoneum. He makes no effort to protect the peritoneal cavity from its ingress during an operation, and yet his statistics show a remarkable success.

In response to a question by Dr. C. Meigs Wilson, DR. GOODELL stated that the dressing of the wound after the operation was glycerole of carbolic acid with the Lister gauze.

DR. GOODELL also gave the following history of a

CASE OF HYSTERECTOMY.

The woman was unmarried, aged forty-seven. Her monthly fluxes began to be free in 1867. A year ago they became so exhausting that she could not pursue her trade as a seamstress. On April 30th she consulted Dr. Goodell, who found the whole abdo-

men filled with multiple fibroids of the womb. The cervix had disappeared and the os uteri lay so high up that it was not possible to introduce the sound. The operation was performed at the Hospital of the University of Pennsylvania on May 22d, on the same day with the preceding case. One outgrowth as large as two fists contained a cavity filled with cheesy matter, and was so adherent to the abdominal wall and intestines as to need the knife for its release. It was possibly the right ovary, but he was by no means certain. Kœberlé's wire clamp was passed around what corresponded to the neck of the womb, but it was as large as his arm above the elbow. The woman's recovery thus far has been uninterrupted. The temperature reached 100° but once. The clamp fell off on the sixteenth day, leaving a very deep, funnel-shaped pit. He had intended to exhibit the specimen, but it was too bulky to carry, and also had become quite offensive. In this case had he been able to reach the ovaries or to have discovered them, he would have removed them in preference to performing hysterectomy; but the firm adhesions prevented the rotation or the lifting up of the tumor, hence the ovaries were inaccessible. Sometimes even when the uterine fibroid can be lifted out of the wound and the ovaries reached, these organs are so embedded in the fibroid, or so drawn out in ribbon-form on the surface of the tumor as to make their complete removal impossible. When, however, the ovaries can be removed with safety, the operation is a most promising one, as he could attest from several most successful cases.

DR. W. T. TAYLOR reported the following case of

PARTIAL PLACENTA PREVIA.

Mrs. S., an English woman, aged forty-six years, the mother of ten children, came to see me in December, 1883, in consequence of abdominal pains, headache, and vertigo, with a suppression of her menses, which she attributed to a "change of life," as she had been irregular for a year past. She also had numbness, with tingling in the hands and feet, and had not been so affected in any former pregnancy. Consequently she would not believe in her condition until some weeks later, when she quickened. In the month of February last she had enlargement of the veins of the legs, with edema of the feet and ankles, for which she took occasional doses of potassium bromide with a solution of cream of tartar (ʒi. to water one pint) to be taken freely. By this treatment she was temporarily relieved.

On March 27th, she had abdominal pains and a profuse hemorrhage, which saturated her clothing and greatly alarmed her. On examination, I found the os uteri high up and slightly open. Although the hemorrhage diminished, yet the pains would recur at intervals, and I fully expected labor would soon begin. Under the use of equal parts of wine of ergot and solution of sulphate of mor-

phia, she began to get easier, and in a few days was out of bed and able to resume her household duties, feeling more comfortable, as the enlarged veins were smaller, her feet and ankles had diminished in size, and the headache gone entirely. I told her that Nature had come to her relief and bled her without my ordering it.

Feeling satisfied that this was a case of placenta previa, and that there was no immediate danger, I concluded the most prudent course was to let Nature alone, and wait until labor began. She had no more trouble until May 10th, when she passed a large clot of blood and complained of slight pains in the abdomen. These occurred occasionally for two days, when the membranes ruptured, and a sudden gush of water, followed by a flow of blood, indicated that labor had begun. On examination, I discovered within the os a spongy, ragged, bleeding mass of tissue, which was recognized as the placenta; with each pain the flow of blood increased as the cervix dilated. Sweeping my index finger around within the mouth of the womb as far as I could reach, to detach the placenta from the uterine walls and assist the first stage of labor, I felt the fetal head beyond.

To arrest the bleeding which, if it continued, would exhaust the mother and destroy the child, I plugged the vagina completely with strips of old muslin well saturated with lard, and waited patiently for the os to dilate and the head to advance, giving at the same time occasional doses of quinine and wine of ergot as a tonic and stimulant. In about an hour the advancing head had expelled a part of my tampon, and, on removing the remainder, I found that the bleeding had ceased, and the vertex was presenting in the left occipito-posterior position. Auscultation revealed a feeble fetal circulation, but as the pelvis was roomy and my patient somewhat exhausted, I gave her freely of milk-punch until her pulse became stronger, which it did in half an hour, when, as the head had ceased to advance, I applied the forceps and delivered her of a medium-sized girl, which, in a few minutes, began to cry with some vigor, contrary to my expectations, for I had told them it would probably be dead. Its vitality had probably been preserved by the adhering part of the placenta, which then came away quite easily. A teaspoonful of fluid extract of ergot contracted the womb firmly. The patient was weak for several days, but under the use of tonics with nourishing food she soon recovered her usual strength.

DR. A. H. SMITH remarked that this case being partial and without profuse hemorrhage, could have best been carried through by rupturing the membranes and bringing down the head, which would have stopped the hemorrhage as soon as it engaged in the superior strait. Dr. Smith asked the question, Under what circumstances are we warranted in interfering? If the hemorrhage is alarming and the patient exhausted, she is in a poor condition to bear interference, and, on the other hand, we have no right to interfere if there is no pain or hemorrhage. To interfere by

manipulation is very dangerous, unless the uterine contractions are rapid and effective after labor once begins. The position of the child should be carefully and accurately determined by external manipulation before interference becomes necessary, so as to know where to seek the feet if turning becomes imperative. When the placenta previa is complete, dilatation of the os causes a terrific hemorrhage, the blood streams from the patient like water from a hydrant or a small fire-plug, and death comes very quickly. Only perfect knowledge of the condition of things and the position of the child will enable the physician to avert the doom. Now as to the tampon. I would not use it. It hides the hemorrhage which may be going on profusely behind it, as was so vividly described by Dr. Goodell in his paper on *Concealed Accidental Hemorrhage of the Gravid Uterus* in vol. 2. AMERICAN JOURNAL OF OBSTETRICS, in which he showed that the woman might bleed to death without one drop of blood escaping externally. The tampon conceals the hemorrhage without necessarily preventing it, and while it remains in place, one hand of the physician should be constantly on the patient's pulse to note instantly any failure of the heart, while the other should be on her abdomen, to note any changes in size of the uterus or position of the fetus. In Dr. Taylor's case the treatment was beyond criticism, because the result has been happy.

DR. GOODELL agrees with Dr. Smith that placenta previa is the most formidable complication in obstetrics. No general rule can be made applicable to the treatment of all cases. In partial ones the membranes should be ruptured and the head brought down. It must always be borne in mind that, in these cases, the implantation of the placenta has caused increased vascularity and thickness of the cervical walls; they are easily ruptured, and if torn bleed profusely. There is greater danger of septicemia from absorption of decomposing lochial discharges when passing over this surface if it is torn. He well remembered one case which he attended years ago in consultation with Dr. Augustin Fish, since deceased, in which, in consequence of what he now considers undue haste, lacerations of the cervix occurred, and although the labor terminated happily, septicemia set in a few days later and resulted fatally. It would have better in that case to tampon. When the placenta is not central there is very little danger; there is some. The tampon may be used, but the pulse must be constantly watched, and frequently abdominal palpation should be made. He had been struck with the method practised by Dr. Ellwood Wilson more than twenty years ago. It consisted in gentle digital dilatation of the os; introducing first one finger, then two, and so on: as soon as sufficient space was obtained, he gave ergot, made podalic version, and delivered. Very few practitioners advise that method. Dr. Goodell had not met with many cases, and the one spoken of above was the only fatal one. He has used Barnes' dilators, taxis, strength, and courage, and has tried to adapt his treatment to the indications of each particular case.

DR. W. H. PARISH thought the tampon was not used as frequently now as it was a few years ago, either in the form of Barnes' dilators or the vaginal plugs. Under similar circumstances he would probably do as Dr. Taylor did. Partial placenta previa is not very dangerous, but in complete the hemorrhage is excessive. In one case he had tamponed for several hours, when the os being dilated, he etherized and rapidly delivered a living child.

The mother was in imminent danger of death from hemorrhage. The abdominal aorta was compressed; ice was used to the cervix, but without success. The hemorrhage was controlled by the application of a cloth wet with Monsel's solution to the denuded cervical and uterine surface.

DR. TAYLOR has used the tampon in several cases of partial placenta previa and in numerous cases of abortion, and has never yet had bleeding to go on behind it. The line of treatment practised in this case has always proved satisfactory as regards results.

DR. GOODELL remarked that the womb at term was large, and concealed hemorrhage might be free enough to cause death, but there was no such danger in an abortion at two or three months.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF LONDON.

Meeting, Wednesday, May 7th, 1884.

H. GERVIS, M.D., F.R.C.P., *President, in the Chair.*

DR. ARTHUR MITCHELL read a paper on

STRONG MENTAL EMOTION AFFECTING WOMEN AS A CAUSE OF IDIOCY IN THE OFFSPRING.

The author remarks that this cause is often assigned by the mother, but in most cases without sufficient reason, especially as more probable causes can usually be discovered occurring after birth. In some cases, however, he has convinced himself that this is the true cause, especially if the mental state has been protracted, and if the rapidity of developmental changes in the fetus be considered. Such disturbance may act either as arrest or as misdirection, and probably through the bodily health of the mother. Sudden emotions may in some cases have a similar effect; under such conditions patients have described violent fetal movements following the shock, and afterwards a cessation of movements for some days. Such movements are probably convulsive, and if so are quite capable of accounting for permanent mental damage. Mental emotion is known to lead to abortion, sometimes apparently preceded by the death of the fetus. The paper concludes with an account of 6 cases, drawn from 443 cases of idiocy or imbecility consecutively examined, with allusion to 22 other cases, making 28 cases observed in a total of 1,500. In the 22 cases the evidence was less satisfactory.

THE PRESIDENT gathered that the effect was presumably produced through the general health of the mother, rather than directly on the fetus.

DR. WEST remarked that such statistics could not be trustworthy until compared with a similar series in which no such results had

followed from the same causes. The latter class would in his opinion prove a large majority. His present opinion coincided with the verdict "*not proven*."

DR. FLETCHER BEACH said that, according to statistics collected by him in the Dareuth Asylum, fright and other violent mental emotions accounted for about one-tenth of the cases in which histories could be obtained. But in many of these cases there was also intemperance involving worry and anxiety to the mother, ill usage from the husband, pawning of furniture, and want of food. In a large number of cases there was a history of insanity, imbecility, epilepsy, or paralysis in the parents.

DR. CLEVELAND agreed with Dr. West; he related the history of a family of six, three of which (each alternate child) were idiots; both parents were sane, there was a history of insanity on the mother's side, but she had never herself been insane.

DR. AVELING regretted the absence of histories of insanity and consanguinity from the cases related.

DR. MATTHEWS DUNCAN observed that Dr. Mitchell did not claim to have proved his thesis, but only said he had "convinced himself" of the truth of it; he (Dr. Duncan) thought he had made it highly probable. Dr. Mitchell had already shown that the age of the mother, the number of the pregnancy, and of twinning, influenced the offspring. Ancell had shown that the injurious or favorable influence of the number of the pregnancy extended over the whole life of the children to adult age, influencing the rate of their mortality. Such facts proved the grave effect of slight causes, and favored the author's view.

DR. HEYWOOD SMITH pointed out the omission from the paper of the date in utero-gestation at which the perturbing influence occurred. In physical deformity of the offspring, the mental shock to which the mischief was ascribed by the mother happened, in his experience, generally between the third and fourth month.

J. H. MATHIESON, M.D., St. Mary's, Ontario:

ON A CASE OF EXTRAUTERINE GESTATION—DELIVERY OF A LIVING CHILD PER VAGINAM—REMOVAL OF PLACENTA, RECOVERY.

The patient was aged 30, the mother of five children. In November, 1880, menstruation (which had been regular, though scanty for two months) ceased and abdominal pain began. In December, 1880, a lump was noticed low down in the left inguinal region. She was first seen by the author in March, 1881, when she had a tumour rising to the level of the navel, filling the left inguinal region and extending two inches to the right of the middle line. The uterus could not be mapped out. The sound passed three and a half inches slightly to the right side and in front of the tumor. There was occasional pain in the tumor, tenderness and a feeling "like jelly moving" which made her feel sick, painful micturition, irregularity of bowels. The tumor continued to enlarge, fetal movements to grow stronger and the pain more severe. On April 12th, she had an attack of pain and nausea with pulse 146, temperature 103.6, cold extremities and cold sweat, relieved by morphia. A similar attack occurred on April 23d, another on May 2d, and another about May 18th. On June 2d, a somewhat similar attack was accompanied by pretty regular labor-like pains inde-

pendently of fetal movements, and accompanied by a red discharge. The tumor continued to grow and its most rapidly growing part was always the most painful. On June 13th occurred another attack like the last, but the pains were bearing down, though no bulging during them could be felt per vaginam. On June 28th, she had a very severe attack during micturition, followed by rapidly following attacks of syncope. The roof of the vagina was filled with a doughy mass solid on deeper pressure. Operation per vaginam was at once decided on and performed without chloroform at the patient's request, by cutting with a knife and tearing with the finger-nail. The face of the child was found presenting, and was delivered by forceps easily. It was still-born, but soon revived. The placenta was at the posterior and left side of the cavity, its lower border some two inches from the vaginal roof. It was easily peeled off, a sponge soaked in perchloride of iron following it up. There was very little bleeding. The clots were removed from the cavity, its walls were touched with perchloride, and the operation ended. Much exhaustion followed the operation. The child weighed eight pounds seven and a half ounces and appeared mature. The placenta was three-lobed, but was destroyed before a careful examination could be made. The after-treatment consisted in washing out the cavity antiseptically. The opening healed in three months. Severe pain in the side lasted for nearly two years after the operation. She was seen in August, 1883, in excellent health, had no pain except after great fatigue, had menstruated normally for six months. The child was healthy, the largest, "and a great deal the heartiest of the family." The author debates the treatment pursued (1st) as to the operation per vaginam, (2d) the removal of the placenta.

THE PRESIDENT doubted the propriety of the vaginal operation and of the removal of the placenta. Each case had to be judged on its own peculiarities, but generally he was in favor of primary operation, abdominal section, and leaving the placenta.

DR. AVELING suggested the possibility of the sac being a secondary uterus.

DR. PLAYFAIR disapproved of vaginal operation and of removal of the placenta: the case as related was most extraordinary, especially in regard to the labor pains described.

DR. CHAMPNEYS remarked that such pains are part of the ordinary phenomena of such cases; the cyst seems to go into a sort of labor on its own accord.

DR. BANTOCK thought the case was one of tubal pregnancy, the tube giving way on its under surface, and that the fetal sac had grown between the layers of the broad ligament.

DR. CARTER related a case of extrauterine pregnancy; the fetus died about term and was removed by abdominal section, though it could easily have been removed per vaginam, abdominal section being indicated by an abscess pointing at the navel. Labor-like pains were frequently observed.

DR. EDES advocated primary operation as an operation of election, especially in view of the present position of abdominal surgery.

MR. GRIFFITH thought that the presence of a contractile cyst-wall necessitated the gestation being either interstitial or in an undeveloped horn of a double uterus.

DR. CHAMPNEYS observed that the uterine platysma, which extended widely over the pelvic organs, was sufficient to account for contractions, and that these occurred in undoubted extrauterine pregnancies.

DR. GODSON alluded to a case under Dr. Duncan, in which unsuccessful attempts had been made to kill the fetus by morphia injections.

DR. H. GERVIS:

CASE OF CYSTIC DEGENERATION OF THE CERVIX UTERI.

The patient, *æt.* 45, had had nine children, the youngest five years old. The menopause had occurred three or four years previously to her admission, which was indicated by abdominal enlargement, which turned out to be due to obesity. The cervix was covered with 'close-set, tense, glistening vesicles, varying in size from a millet to a hemp seed. There was no other abnormal appearance, and there were no uterine symptoms whatever, the vaginal examination having been indicated by the abdominal enlargement. On puncture nothing but air escaped with an audible sound, the vesicle so punctured afterwards being indistinguishable except for slight traces of the marks of the punctures.

DR. HERMAN referred to the disease of the vagina and cervix described by Winckel as "*Colpohyperplasia cystica*," characterized by cysts containing air or gas.

(The following remarks of Dr. Barnes on Dr. Kilner's papers were accidentally omitted from the report of the last meeting.

DR. BARNES had found faradism capable of initiating as it was of intensifying uterine contractions, but he had discontinued its use partly on account of the pain it produced. Its power of reducing arterial tension might relieve the vomiting of pregnancy and other cognate disorders.

REVIEWS.

AUS DER GEBÄRANSTALT DES KAISERLICHEN ERZIEHUNGSHAUSES ZU ST. PETERSBURG. Medicinischer Bericht für die Jahre 1877-1880.—MEDICAL REPORT FOR THE YEARS 1877-1880 OF THE OBSTETRICAL SERVICE OF THE ROYAL MATERNITY AT ST. PETERSBURG.

In this report are collated the statistics of this institution during the four years ending with 1880. The total number of patients admitted was 10,714, of which 60 were discharged before delivery. This leaves a balance of 10,654 cases, made up of 9,441 labors at term, 850 premature labors, and 363 miscarriages. The first part of the report, concerning pregnancy and labor, was compiled by Dr. E. Bidder. The cases are analyzed from this standpoint with regard to presentation, pleural births, miscarriages and premature

labors. Brief histories of interesting and anomalous cases are given; the injuries which the parturient passages suffered during labor are described and in turn analyzed; the anomalies and diseases of these same passages are then noted and this part of the report ends with reference to the cases in which there existed some complication such as pelvic deformity, tumors, placenta previa, etc. The statistics, in short, are exhaustively considered and in an interesting way. Amongst the cases reported in full, to which we would call attention here, are thirteen of uterine rupture, ten of which were complete, all ending fatally; four cases of occlusion (conglutinatió) of the os uteri during labor, five of vaginal septa, and eight in which there existed one or another abnormality of the uterus. The total mortality for the four years is placed at 1.60 per cent, of which .36 per cent was from non-puerperal and 1.24 per cent from puerperal diseases. Four of the deaths were of women pregnant and undelivered. On one of these the Cesarean section was performed six minutes after death, but the fetus was not saved; on the others the section was not attempted, owing to the fetal death before the maternal.

The second part of the report is from the pen of Dr. H. Tarnowsky, and describes the puerperium. Morbidity and mortality tables for each year are first offered, and then the statistics for the past fifteen years are tabulated, showing an almost uniform proportionate decrease for each successive year. For instance, whilst from 1871 to 1876 the morbidity varied from 58.6% to 34.4% and the mortality from 4.50% to 1.46%, from 1877 to 1880 the former varied from 27.1% to 34.4% (1880) and the latter from 1.21% to 1.38%, reaching the low rate of 1.02% in 1878. The cases are then still further analyzed according to that portion of the genital tract whence the disease took its origin, that is to say, two grand divisions are made into the diseases of the external genitals and vagina and into those of the internal surface of the uterus. Of the total number of women admitted into the institution during the four years covered by the report, 3,267 presented one or another form of puerperal affection; 135 or 1.2 per cent died, 18 still remained in the service, and 3,114 were discharged cured.

The concluding pages of this report are devoted to a résumé of the ovariectomies and hysterectomies performed in the gynecological service by Dr. A. Krassowsky from 1877 to 1881. There were twenty-seven operations of the former kind with nine deaths and five of the latter with three deaths.

To such as are interested in statistics, this report commends itself as being carefully compiled and full of valuable information of varied nature.

EGBERT H. GRANDIN.

DE LA PINCE À OS ET DU CRANIOCLASTE, etc. Par le DR. A. AU-
VARD.—AN HISTORICAL AND EXPERIMENTAL STUDY OF THE
CRANIOTOMY-FORCEPS AND THE CRANIOCLAST, preceded by re-
marks on the fetal head and the perforation of the skull. By
DR. A. AU-
VARD, late Interne at the Hospitals and at the Mater-
nité of Paris. With forty-five woodcuts. Paris: Octave Doin,
Editeur, 1884.

From a series of carefully conducted experiments the author of this monograph is enabled to draw well-founded deductions as to the sphere of applicability of the cranioclast under different presentations of the fetal head and trunk. These deductions apply in particular to a modification of the instrument devised by Carl

Braun, a modification which widens its field of usefulness by enabling it to accomplish what neither Braun's nor any other form of cranioclast can to any great degree. To take Braun's instrument as a type, its main function is to serve as a tractor, and its most useful application, in general, is to those cases of contracted pelvis where delivery by means of that larger and bulkier instrument, the cephalotribe, is impossible. Dr. Auvard offers us a modification which, whilst equally efficacious as a tractor, is also, from his showing, an excellent compressor, which Braun's instrument is not, and yet should be under certain limits of pelvic deformity and in certain fetal presentations. The real difficulty in extraction of the diminished head is offered by the base of the skull. The accoucheur's aim should be to break up that portion of the base extending from the occipital foramen to the face, whenever possible. From such considerations, then, and with this end largely in view, Auvard has modified Braun's instrument as follows: He gives the cranioclast such a curve as will allow it to seize this portion of the skull, and he has so constructed it as to enable the blades to lock either with their concavities together or else like the ordinary cranioclast. When locked in the latter sense we can make traction; when locked in the former sense, compression. His instrument also differs from Braun's in that the female or fenestrated blade bears the pivot, and the extremity of the male blade is turned into a cork-screw. Such being the instrument, it is used, to take an example, as follows: Given a vertex presentation, perforate the vault and, having ascertained the location of the occipital foramen, introduce the male blade within the cranium and screw it gently into the foramen. Now apply the female blade over the face and lock so that the concavities of the instrument shall be in apposition. By screwing the blades together the base of the skull and the face are crushed. Now unlock the blades, turn the male blade around, relock and the blades are opposed as in Braun's cranioclast and we have just as efficient a tractor. Such, then, is the method of application of this modified instrument. We have chosen its description because it is the essentially practical novelty of the monograph. By means of this instrument a fetal head at term may be brought through a pelvis contracted to fifty mm. Where the head is yielding and Braun's instrument can obtain a favorable hold, it will do as well. But under different conditions, according to our author's showing, the modified instrument is superior.

We have noted above but a portion of the contents of the monograph. It contains also an extended study of the fetal head, the object being to determine the best point for perforation, according as it is desired to engage one or another diameter; experiments made with different perforators to establish the form of instrument which makes the best perforation (preference is given to Blot's); the action and application of the cranioclast under varying positions and presentations, etc. These different sections are all illustrated by the relation of experiments made on fetuses delivered through Professor Tarnier's bronze pelvis.

The monograph is an interesting one and reflects credit on the ingenuity, zeal, and patience of Dr. Auvard. It will undoubtedly be of service in popularizing an instrument in France which, up to the present, has been much neglected and has been erroneously considered as presenting no advantages over the bulky cephalotribe. If the modified instrument proves as simple in its employ-

ment and as effective in its results in the hands of others as it has in the hands of its originator, Dr. Auvard will have added something of real value to our obstetrical armamentarium.

EGBERT H. GRANDIN.

PRACTICAL MANUAL OF OBSTETRICS. By DR. E. VERRIER, Lecturer on Obstetrics in the Faculty of Medicine of Paris. First American from the Fourth Enlarged and Revised French Edition, with Revision and Annotations by EDWARD L. PARTRIDGE, M.D., Professor of Obstetrics in the New York Post-Graduate Medical School. New York: Wm. Wood & Co., 1884. (April Number Wood's Library.)

The judicious revision of the American editor places this work on a par with the accepted practice of this country, and protects it from much adverse criticism. It is really a useful manual, has proved a favorite in France, and should commend itself in its English dress to the American student. There are only a few points to which we must take exception. The first on page 136, where the customary French practice of making traction on the cord for the purpose of delivering the placenta is indorsed. For many reasons, which it is unnecessary to repeat here, we should prefer Credé's method of placental expression, and claim that it alone should be taught to students, because, however safe traction may prove in the hands of those who possess the *tactus eruditus*, it may be far otherwise when applied by the non-expert. Credé's method, on the other hand, is both simple and safe in its application and ordinarily sure in its results. Another point to which we except is the treatment advocated for reposition of the retroverted gravid uterus on page 221. "Put the woman in the knee-chest position, and, by means of the finger introduced into the cervix, try to push the body of the uterus in a direction *opposite to its deviation*: rarely do we have to use instruments." We question the advisability of introducing the finger into the cervix, apart from the fact that this proceeding is not always possible. A safer method, and, to our mind, more effective, is the introduction of two fingers into the posterior cul-de-sac, thereby lifting the uterus above the excavation, and then completing the reposition by hooking one finger over the cervix and making backward traction. As for instruments, we should never use them, except a tenaculum or pessary, if needed, since, from the very nature of the case, instruments for reposition are contraindicated.

As minor points, we presume that in the following sentence (page 220): "Pregnancy, far from curing retroflexion, aggravates it, or, at the third or fifth month, transforms it into retroversion," the words "retroflexion" and "retroversion" have by mistake been transposed: and we will also attribute to an oversight the inelegant wording of the following (page 256): "(Attention to the prostration of shock and hemorrhage must be bestowed.—Ed.)"

In conclusion, we are glad to see Dr. Verrier, the pupil and ardent admirer of Professor Pajot, at variance with his instructor's far too strong leaning towards embryotomy. We believe with him that the day is not far distant when laparotomy, by one or another method, will be substituted, in every case where the child is alive, for embryotomy; and we believe further that, whilst the child will thus be given a chance, the mother will not, owing to the perfection of the operation, be subjected to any greater risks.

EGBERT H. GRANDIN.

A STUDY OF THE BLADDER DURING PARTURITION. By J. HALLIDAY CROOM, M.D., F.R.C.P.E., Physician to the Royal Maternity Hospital, etc., etc. Edinburgh: David Douglas, 1884.

In this compact little essay, Dr. Croom studies the behavior of the bladder at various periods of pregnancy, during labor, and in the puerperium. The relation of this organ to the uterus in the unimpregnated condition, its shape compared with that of the male, its asymmetrical form, are first succinctly stated, and then are noted the positions it assumes in consequence of the growth of the uterus and the phenomena which accompany labor. We are then introduced to the ultimate aim of the essay—the determination of the amount and distribution of the pressure exerted on the bladder during parturition. This pressure was determined by means of a specially devised manometer, and the experiments are incorporated in charts appended to the book. Dr. Croom's conclusions may be summarized as follows: Pressure is brought to bear on the bladder during labor. The sources of pressure are: The changes in shape of the uterine ovoid, the stretching of the cervix, the action of the abdominal and other respiratory muscles (these last to the greatest degree). During the interval of pain, bladder pressure amounts practically to nothing. The maximum pressure, in ordinary labors, is 3.2 pounds to the square inch, and the minimum pressure .1 pound. These results are the same for primiparæ and multiparæ, and the quantity of urine in the bladder does not influence them, provided the point of over-distention be not reached.

As supplementary to these studies, a few pages are devoted to the bladder during the early puerperium, in which the author shows the influence of the organ in causing or modifying upward, backward, and lateral displacement of the uterus, as well as rotation.

The essay in general is interesting. The experiments on which the contained data depend were evidently carefully made, and the deductions, therefore, may be accepted as sound and accurate.

EGBERT H. GRANDIN.

ABSTRACTS.

1. Kleinwächter: The Influence of Age on Primiparous Labors (*Zeitsch. f. Geb. und Gyn.*, X., 1).—The object of this elaborate paper is to determine what effect the age at which conception first occurs has on pregnancy, labor, the lying-in period, and the child. The material from which the deductions were drawn consisted in 920 primipara varying in age from 16 to 41. K. also, where possible, compares his results with those obtained by other writers. The main deductions are as follows: The older the primipara, the more tardy has been the appearance of her first menstrual period, and the more likelihood she has suffered from irregular menstruation. Those congenital or acquired conditions which render menstruation tardy or irregular, as well as those conditions which render coition difficult for the husband, and therefore conception difficult for the wife, obtain most frequently in the old, seldom in those of

middle age, only occasionally in the young. Accidental morbid complications, apart from the gravid state, affect the young primipara seldom, the old more frequently; whilst those connected with the gravid state affect the old most frequently and then the young. Hemorrhage during pregnancy is more like to occur in the case of the young than the old. The amount of liquor amnii does not apparently vary with the age, but the duration of labor is considerably above the normal the greater the age, and ineffectual pains as a cause of prolongation obtain particularly in the old. Therefore the forceps must be used more frequently with increase of age. Their use is less frequent between 20 and 29. The mortality percentage from their use rises with the age of the patient. The older the primipara the more likely perineal rupture, as also postpartum hemorrhage, though the likelihood of this last is by no means so great as has been thought. With increase of years there is greater tendency to kidney disease, as also towards edema without kidney lesion; there is less tendency, however, towards mastitis as well as lessened ability to nurse. The old sicken and die more frequently of puerperal fever, have a greater tendency towards puerperal mania, and show a higher morbidity and mortality percentage from the diseases of the puerperium. The reverse of all this holds for primigravidae between the ages of 20 and 29. Spontaneous premature labor is very frequent with old primigravidae, the ages from 20 to 29 being specially exempt. With increase of age the tendency towards abnormal presentations rises, particularly breech and those head cases where towards the end of labor there is lack of rotation. Deformity of the pelvis has no causal connection with the age at which the first conception occurs. Male births are more frequent with increase of age, excepting from 20 to 21 when females are more frequent. According to Hecker, Wernich, and others, the first child will be heavier and longer the older the mother, the umbilical cord drops off sooner from the first-born of the old, and the reverse of the young. With increase of age there is increased tendency towards twin births, and less likelihood of misformed children. The infantile mortality at birth rises with the age of the mother, reaching with the oldest a by no means insignificant figure.

E. H. G.

2. Kaltenbach: A Contribution to Laparomyotomy (*Zeitsch. f. Geb. und Gyn.*, X., 1) —K. has now performed this operation ten times. The histories of six cases are given in this paper, four having been elsewhere reported. Of the ten cases, two were simple myotomies, that is to say, the uterus was not implicated in the operation, and the pedicles were dropped, whilst in the remaining cases, the uterus was removed as well, in one case the section being made through the body, in seven at or about the level of the internal os. The pedicle in these cases was treated extra-peritoneally. In all cases of supravaginal amputation the ovaries should be removed, to guard against the occurrence of menstrual hematocele on the one hand, and ectopic gestation on the other. The indications for operation in these cases were, in three, the great size of the tumor (6–10 kilogr.); in four, the rapid growth of the tumor in connection with hemorrhage, recurrent attacks of peritonitis, interference by compression of abdominal organs; in one case, metorrhagia; in one, complete prolapsus uteri; in one, interference, by compression, with pelvic organs, the tumor being in Douglas' pouch. Nine of the cases recovered, and were free from the symptoms for the relief of which the operation had

been performed. Formerly the prognosis of laparomyotomies was much inferior to ovariectomy. But nowadays there is little difference in the results after each. The improved method of treating the pedicle is in above all the cause of this. A few years ago, it was a question as to which method of treating the pedicle—extra- or intraperitoneal—offered the best results, and the answer was in favor of the former. Since then, however, the intraperitoneal method has given results so much better than formerly that the question is again open. Obviously that is the preferable method which gives us the greater number of safeguards against hemorrhage and sepsis. K. has obtained most excellent results from the extraperitoneal handling of the stump. The elastic ligature has in all his cases sufficient to forestall hemorrhage, and the treatment of the stump by the hot iron and chloride-of-zinc solution has kept its aseptic until the parietal serosa has united with the superficies of the stump and until its central part is no longer capable of absorption. Latterly the technique of the intraperitoneal treatment of the pedicle has been much simplified and the danger from hemorrhage and sepsis lessened. The method which Schröder has followed with such good results is to be recommended, whereby the blood-vessels running either side of the tumor together with the broad ligaments are ligatured in one or two portions and the peritoneum sewed around the stump so that both it and the parametrium are completely shut off from the peritoneal cavity. Granted the safety of the intraperitoneal method, it is certainly the preferable, since by it the after-treatment is simplified, and it is peculiarly applicable to those cases where, from shortness of the cervix or from the intraligamentous position of the tumor, the stump either cannot or else can only with difficulty be brought to the surface. The chief statistics from the side of the extraperitoneal method are: Kasprzik reported in 1881 a series of twelve cases after Hegar's method with but one death. In this series are included three successful cases operated on by K., who, as noted above, has had but one death (a Péan operation) out of eight. Bantock reported in 1881 eight cases extraperitoneal, all successful, and four intraperitoneal, all fatal. Later he reported fifteen cases extraperitoneal, one death, and six intraperitoneal, one recovery. Of eight extraperitoneal cases (six supravaginal amputations), Horwitz lost two cases. From the side of the intraperitoneal method the main statistics are: Schröder, fourteen cases, one death from sepsis; Olshausen, nine myotomies (one extraperitoneal, recovery), two deaths; three supravaginal amputations, two deaths; Leopold, six cases (myotomies and supravaginal amputations not distinguished), two deaths; Spencer Wells, four recoveries, by too great hemorrhage; Kuester, thirteen laparomyotomies, nine recoveries, seven deaths.

From these statistics it is impossible to affirm that the intraperitoneal method has marked superiority over the extra. Better results are obtained from it now than formerly and likely enough in a short time it will be in the ascendant. From the standpoint of present research, each method has its advantages.

E. H. G.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY . . . GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

SUDDEN DEATH IN DIPHThERIA.

BY

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THOUGH fully aware that diphtheria has been exhaustively discussed by many of our best authors, still the reintroduction of the subject may be pardonable when we have in view the recent reports of sudden deaths occurring during the decline of the disease and the backwardness of the reporters in venturing an opinion as to the probable cause of this sudden dissolution.

In the case upon which this paper is based there are many interesting points which I will endeavor to explain. By comparing this with other cases I hope to be able to show that sudden death is not invariably due to paralysis of the heart.

On the 23d of December, 1883, my attention was called to Chester A., a bright bouncing boy of sixteen months, well-developed, with good constitution, twelve teeth, and a fontanelle normal at the age. His mother said he occasionally put his hands to his head, and grunted as if in pain. As he was then running merrily about the room and into the kitchen, and as he was accustomed to imitate the actions of the elder members of the family, I told them that apparently he was well, and advised them to let him alone.

At this time the mother, a healthy lady, complained of feeling languid, but went about and ate as usual, and did not ask for advice.

At 4 A.M., December 24th, the father came for me in great haste, saying that his baby was dying of convulsions. I hastened to the house on De Sales street, and learned that a homeopathic physician had been there, had pronounced it a case of difficult dentition, and left some *aconite* in a tumblerful of water.

The child was now sleeping very quietly; skin hot and dry; respiration jerky; fontanelle depressed; pulse rapid, with good tension; tongue fair; no muscular twitchings; gums neither red nor swollen.

I obtained the following history: The child had gone to bed feeling bright, and the members of the family had been called in to see his antics. In the early hours of morning he began to be restless, fretted, and seemed agitated, but his parents thought it mischievousness, and scolded him; he would keep quiet for a while, but very soon would repeat the movements. For a time all slept, when the mother was suddenly awakened by the peculiar noises he made, felt him to be rigid, and sprang from the bed, running into her mother's room to the light, to find that her babe was in a severe convulsion. This attack was described as lasting ten minutes. Upon inquiry I learned that the diet of the previous day had been of the simplest, consisting of bread, potatoes, and milk; he had not had any sweetmeats or fruit. Knowing that the little fellow was accustomed to spend most of his time running about the kitchen, I directed my attention to the probability of his having obtained some indigestible substance, and his nurse, who was greatly alarmed at his condition, admitted that the day before he did get some potato rinds, but that he had only eaten a little when she discovered it.

This gave me a clue, as I supposed, and I ventured the opinion that the convulsion was due either to some indigestible material in the alimentary tract or to the development of some acute disease, and that I favored the former view.

The child had a natural stool the day before, but my first aim was to rid the alimentary canal of any offending material. For this purpose I gave an enema of warm water, which was retained ten or fifteen minutes; ordered calomel gr. i. every hour until two were taken. To quiet the nervous system he was given the potassium bromide gr. iij. every hour.

Anticipating further trouble, I lay on the bed in a distant part of the room. He slept at intervals, and when awake manifested no signs of returning convulsions. At 8 A.M., he was lying on the lounge resting so quietly that his grandmother left the room for a few moments. I was awakened by a peculiar cry that seems to be characteristic, and going to him found that he was then in an eclamptic attack, which could by no means be called severe, and which lasted about two minutes. I now gave another enema which soon came away without any fecal matter. I now directed that if by 9 o'clock he had not a stool, to give two more of the calomel powders as before.

As I was about leaving the house the mother asked me to look at her throat, as it felt sore. Her skin was hot and dry, pulse bounding. Upon the right tonsil was a dirty patch about the size of a pea. I ordered her to bed, and refused to state the nature of the patch, but favored the view that it was aphthous, saying that at my next visit I would tell her what it was.

This unexpected discovery caused me to return to the baby and examine his throat. His tonsils were slightly enlarged and intensely hyperemic, but there was not the slightest trace of a membrane.

I visited the patients at 2 P.M., and learned that about 10 o'clock, after taking the two powders, the baby had a copious stool consisting of fecal matter mixed with potato rinds, cranberry seed, and undigested particles of orange and potato. At 11 A.M. he had a convulsion, which was described as being very slight. He did not seem very much exhausted; skin slightly moist; tongue dry; fontanelle depressed; pulse frequent, but good. Again examined his throat, but saw no evidences of membrane. Continued the potassium bromide; milk diet.

I now directed attention to the mother, and found a quick, bounding pulse, hot, dry skin, tendency to sleep, tongue covered with a dirty brown fur, and two-thirds of the right tonsil covered by a thick, dirty membrane. I could now pronounce the case one of diphtheria. I enjoined absolute rest in bed, liquid diet, and the proper local applications.

The two patients, with the child's father and grandmother, were now isolated, and every precaution taken to prevent the spread of the disease among the family, including three children, the eldest not yet five years of age.

At 7 P. M. I saw the patients. The baby had gotten along very well after my last visit until 5 o'clock, when he had the fourth and last convulsion, which was described as being almost as severe as the first. He had a hot, dry skin, very restless, pulse rapid and full, respiration quick; had not taken any milk, and had slept at intervals. Upon examining his throat I found the right tonsil nearly covered with a membrane; his pulse 150, and a temperature in the axilla of 104.5° . I now pronounced this a case of diphtheria. From the age of the patient, the accompanying convulsions, and the fact that several years ago a brother had died on the second day of diphtheria, I felt justified in pointing out the extreme gravity of the case, and expressed a desire to have Dr. Samuel C. Busey called in consultation, which was allowed.

Dr. Busey soon met me. He thoroughly examined the mother and child, confirmed the diagnosis and agreed with me in making an unfavorable prognosis.

As the further history of the mother's case is not germane to the subject, and as later on I will explain my motive for introducing so much of it, I will drop it to complete the progress of the child's case.

We thought the convulsions were under the control of the bromide, but still deemed it advisable to continue it during the night with instructions to increase the dose should they recur. To reduce the temperature sodium salicylate, gr. iij. every two hours, was ordered, though we were apprehensive of its depressing effect upon the heart. Throat to be sprayed every two hours with the

hand-ball atomizer, containing a mixture of carbolic acid, chlorate of potassium, tincture of the ferric chloride, glycerin, and water. As he had not taken any nourishment during the day we insisted upon the milk and lime-water being pushed. A pan of lime and water, in the room near where the child lay, was kept boiling during the entire illness. I remained with the patient until midnight, and, after instructing those in charge how to use the atomizer, left him resting quietly.

December 25th, 8 A.M. P. 144, R. 28, temp. 99.4°. Has had no convulsions, slept very well; surface cool and moist, one stool; urinated freely three times; bright, and expression better; has taken half a pint of milk.

10 A.M. Dr. Busey in consultation, P. 138, R. 32, temp. 100.2°. As the fever was lower, the heart depressed, and there being no evidences of recurring convulsions, it was determined to stop the sodium and potassium, and give a grain of the hydrochlorate of quinia every four hours. The character of the pulse and the depression of the fontanelle indicated the necessity of stimulants, so fifteen drops of whiskey every two hours was ordered. The membrane was extending over the uvula and left tonsil, but there was no difficulty in breathing. He fought so against the spray, and being afraid the tube would injure him, it was directed to be used every four hours.

2 P.M. P. 146, R. 40, T. 101.4°. No material change.

8 P.M. Dr. Busey in consultation, P. 165, R. 36, T. 103.2°. No stool; but little sleep; uneasy and fretful; has taken a pint of milk; seems to have no pain in swallowing; urinated freely; tonsils and uvula covered with membrane. Increase whiskey to twenty drops. Continue the other treatment.

December 26th, 8 A.M. P. 145, R. 36, T. 100.2°. Slept a good deal, but not quietly; occasionally awoke screaming; fontanelle depressed; pulse tension poor; skin comfortable; one free stool; urinates freely. One pint of milk taken.

10 A.M. Dr. Busey in consultation. Throat looks about the same. Discontinue quinine. Whiskey thirty drops.

2 P.M. P. 140, R. 24, temp. 102°.

8 P.M. Dr. Busey in consultation. P. 140, R. 30, T. 101.6°. Has passed a restless day, no stool; not a pint of milk taken.

December 27th, 8 A.M. P. 152, R. 32, T. 100°. Restless and fretful during the night; no stool; right tonsil clean, small patches on left and uvula. Took a quart of milk.

10 A.M. Dr. Busey in consultation. Slight improvement.

2 P.M. P. 142, tension good, R. 24, temp. 101.4°.

8 P.M. Dr. Busey in consultation. P. 160, tension bad; R. 30, T. 101.8°. Has been bright and cheerful all day; played with his toys for the first time; one large pasty stool; no membrane perceptible; a few large moist râles heard over both lungs. Has taken a pint of milk. Same treatment and one grain of the quinine every four hours.

December 28th, 8 A.M. P. 140, R. 22, temp. 100.6°. Passed

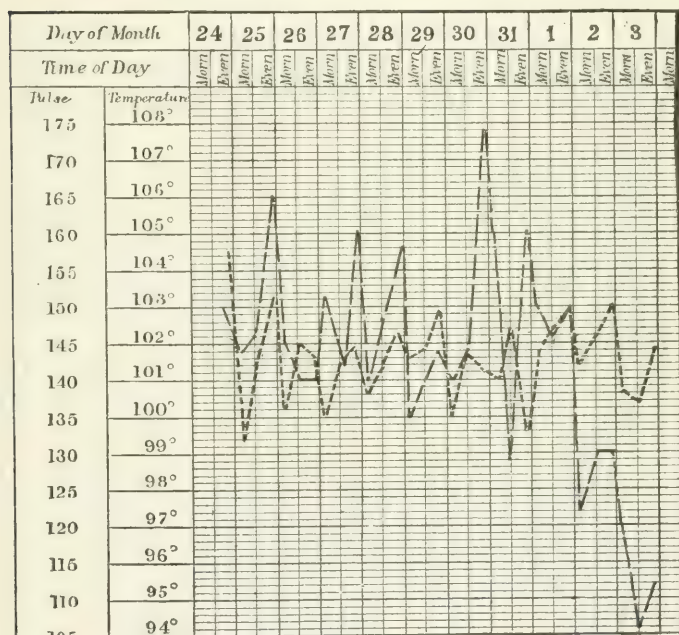
a very restless night; nose seemed to be stopped, which awakened him at short intervals; fretful; one good stool; membrane has reappeared on right tonsil; râles increasing, a few crepitant. Oil silk jacket, and ammoniom carbonate, gr. i. every four hours ordered.

10 A.M. Dr. Busey in consultation. No change.

2 P.M. P. 148, R. 33, temp. 101.4°.

8 P.M. Dr. Busey in consultation. P. 158, R. 35, T. 102.2°. Has passed a very comfortable day, breathing through nose easier; has taken sufficient nourishment. Continue treatment.

December 29th, 8 A.M. P. 135, R. 24, T. 101.6°. Slept very



well during the night; membrane has reappeared on the left tonsil; very little nourishment during the night; a few râles over right lung.

10 A.M. Dr. Busey in consultation. Condition as above described; no change in treatment.

2 P.M. P. 140, R. 28, T. 101.8°. Has been sitting on his mother's bed playing with toys.

(At this visit my attention was called to Mrs. R., grandmother of the child. She had flushed face, coated tongue, hot dry skin, pulse 120, R. 27, T. 101.8°, pain in back of neck; and sore throat; a large patch of membrane on right tonsil. She was put

to bed and a professional nurse obtained. It may be of interest to note that up to this time this lady had almost continuously held the baby in her arms, and in spite of my protestations, persisted in fondling and kissing him, so that there is nothing strange about the poison infecting her. She was put under systematic treatment.)

8 P.M. Dr. Busey in consultation. P. 144, R. 29, T. 102.8°. All in all he has had a very comfortable day; one stool; urinated frequently; a few sonorous râles; has taken large quantities of beef-tea and milk. As the temperature did not yield to the quinine it was stopped. The little fellow fought so hard against having his throat inspected, as each attempt made it bleed, and as he was breathing easily, it was deemed prudent to let him alone.

December 30th, 8 A.M. P. 140, R. 21, T. 100. A comfortable night; breathed easily; one large stool; no râles; skin moist; expression good; one pint of milk. Discontinue ammonium carbonate.

10 A.M. Dr. Busey in consultation. Condition more favorable.

2 P.M. P. 144, R. 28, T. 101.6°.

8 P.M. Dr. Busey in consultation. P. 174, R. 36, T. 101.2°. A comfortable day, seemed bright; takes plenty of nourishment. It will be seen that the pulse was very rapid, but we thought it was principally due to the agitation of the patient at our presence. However, the nurse was instructed to increase the stimulant to thirty-five drops if the pulse continued rapid.

December 31st, 8 A.M. P. 160, R. 28, T. 101°. Slept well, and his breathing is easier than at any time before; two large stools; but little cough; no râles. Asked for nourishment and took it freely.

10 A.M. Dr. Busey in consultation. Continue treatment.

2 P.M. P. 124, R. 32, T. 102.4°. Renew quinine every three hours.

8 P.M. Dr. Busey in consultation. P. 160, R. 29, T. 99.6°. Rested during the day; three stools, no cough; seems to have pain in head; cervical glands of right side hard and tender; takes sufficient food. Increase whiskey to forty drops.

1884, January 1st, 10 A.M. Dr. Busey in consultation. P. 150, R. 40, T. 101.8°. Passed a quiet night; no cough; pain in neck seems to be less, though the swelling has increased; one stool, sufficient nourishment.

2 P.M. P. 130, R. 56, temp. 102.2°.

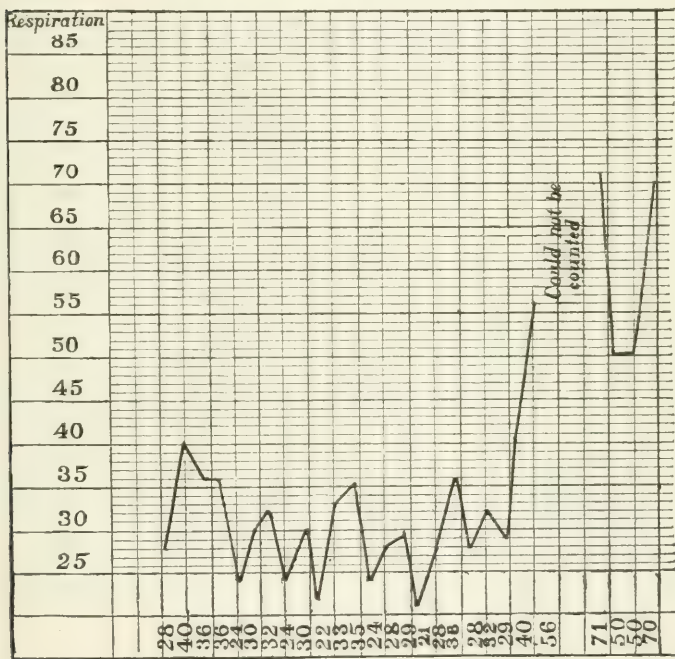
8 P.M. Dr. Busey in consultation. P. 150, R. —, temp. 102.8°. Was restless and fretful all the afternoon, and vomited once, since which his condition has been better than at any time during the day.

January 2d, 10 A.M. P. 122, R. —, 101.4°. Vomited five times during the night; slept but little; pain in head; two good stools; awakes screaming as if frightened by dreams. Owing to the irritability of the stomach the quinine was stopped. Hardly any nourishment during the night.

2 P.M. P. 130, R. —, T. 102.2°.

5 P.M. P. 130, R. 71, T. 103°. Has passed a very uncomfortable day; stomach has rejected nearly everything. Stimulants and food only.

January 3d, 10 A.M. Dr. Busey in consultation. P. 120, R. 50, T. 100.6°. Was sent for to see the patient at 9 o'clock last night. He had vomited three times since my visit at 5. Stimulants and food were stopped to give the stomach a rest. Ordered one-quarter drop of creasote in a drachm of lime-water every hour until easier. Visited him again at midnight. Had taken two doses of the medicine, had not vomited, and was then sleeping



quietly. The stomach to rest until early morning, when he was to have milk and lime-water in teaspoonful doses every ten or fifteen minutes. This morning he seems greatly improved; pulse tension remarkably good; has retained large quantities of milk; cervical glands increasing and very tender; no lung complication to account for the rapid respiration, nor any obstruction in the upper air-passages. His condition is very encouraging, though we will not venture a prognosis.

2 P.M. P. 106, R. 50, T. 100.4°. Screams at times, and mostly when I am in the room, so that it may be attributed to temper.

8 P.M., pulse 112, respiration 70, temperature 101.8°. Has slept a greater part of the day; would wake up and scream, but as he would take food with avidity and then go to sleep, I was still forced to attribute these outbreaks to anger; but those about him insisted that something else was the matter. The pulse was regular (see charts showing pulse, respiration, and temperature for the entire period), full and with very good tension—better than at any time during his illness. Respiration was easy, no evidence of any obstruction in either the upper or lower air passages, but very rapid; nostrils not dilated; no râles of any kind; no cough; skin cool and moist; vomited at 4 P.M., but two doses of the creasote mixture quieted the stomach; four greenish, watery stools. Had I compared the record of to-day with that of yesterday the prognosis would have been favorable; however, the enlarged cervical glands, which now obliterated the depression between the inferior maxilla and clavicle, the rapid breathing, and the drowsiness led me to anticipate trouble. So without alarming the sick, I instructed the nurse to send for me if she noticed any change for the worse. Nor was I mistaken, for at 1.30 A.M. (4th) they sent for me. I hastened to the house and on entering the room perceived a material change for the worse. He had brightened a little about 9 o'clock, had taken milk and beef-tea, and had fallen asleep. Between 10 and 11 he began to toss about; would give the most heartrending screams, point about the room as if he wanted something, but after trying to satisfy him in every way, and failing, they sent for me.

I found him tossing and screaming, pulseless, blue lips, countenance livid, respirations above a hundred and superficial, semi-conscious, face pinched, nostrils dilated, lungs œdematous and rapidly filling, surface bathed in a clammy sweat, and nose, hands and feet cold. Could the little fellow have now described his suffering, it would have vividly re-called the last moments of one of our late beloved members who said that he felt "as if hands of iron were constricting his chest." In less than half an hour, after a hard-fought battle, he died.

As I could not see that anything would be learned by a *post-mortem* examination it was not requested.

At the risk of being tedious I have minutely described the progress of the case, with the treatment. By doing this the therapeutics will be shown.

It is not my purpose to enter upon the subject of diphtheria, but rather to point out what I believe to be the interesting features of this particular case.

1. A point of great interest is the simultaneous development of the disease in both mother and child. They had been sleeping in the same bed for weeks, had enjoyed exceptionally good health, and at the same time, on the morning of Dec.

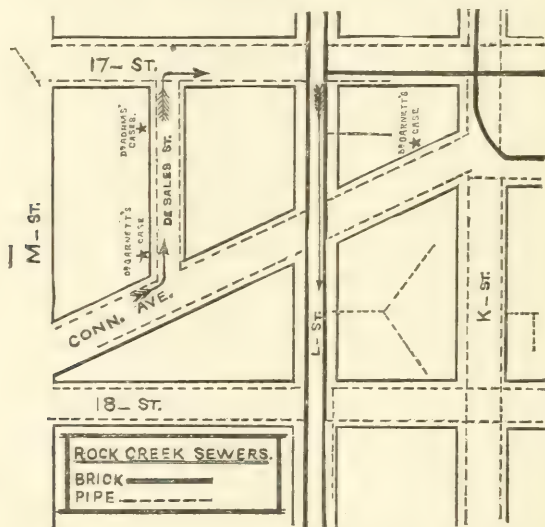
23d, though going about as usual, both manifested what turned out to be the initial symptoms. Because the membrane was recognized in the mother first, we are not justified in concluding that she infected the child, for the premonitory symptoms were exhibited in both about the same time, and, further, the alarming symptoms were first seen in the child. So I am forced to conclude that both were infected by the same contagium at the same time, and ran their courses simultaneously.

2. Whence came the poison? Here the cases are more puzzling than ever. The child had not been near any children outside of the house. The washerwoman had no one sick in her house, and had not washed for any sick person. The mother had been shopping on the 22d, but it is not reasonable to suppose that she contracted the disease in that manner. I was a visitor at the house, but as I had not seen a case of diphtheria for more than a year, the conveyance of the poison could not be traced to me. Is it reasonable to suppose that the father, who had been in all the large cities of the east, and who arrived on the night of the 22d, and slept in the same bed, was the medium of contagium? This can hardly be presumed, for he had neither seen nor heard of a case in his travels and never manifested the slightest symptom of the disease, though almost continuously in the sick-room.

By examining the accompanying plan of the north-west section of this city, it will be seen that the house of my patients is drained by a sewer beginning at 17th Street and Rhode Island Avenue, thence by Connecticut Avenue, to De Sales Street, to 17th Street, to the large brick L Street sewer, which in turn empties into Rock Creek. I learned from Dr. A. Y. P. Garnett that, five days before my patients were taken sick, he was called to attend a case of diphtheria in a house at the Connecticut Avenue end of De Sales St., which was drained by the same sewer as the house of my patients.

May it not be a reasonable presumption that the poison was conveyed along this sewer and backed into the house at the 17th St. end? After the outbreak, a sanitary inspector condemned the plumbing and pronounced the house full of sewer gas. The plumber who put in the new apparatus, under the directions of the health officer, represented that the drip pan of

the closet was caked with filth that was offensive enough to sicken any one. But as I do not favor the spontaneous generation of the diphtheritic poison from filth, but believe it a poison *sui generis* which, with filth as a *nidus*, is a rapidly propagated one; and as the days of Dec. 20th, 21st, 22d, and 23d, 1883, were cold enough to freeze Rock Creek and thus cause an overflow of gas, I strongly favor the transmission of the contagium from Dr. Garnett's case, through the sewer, to my own patients who slept in a room adjoining the bath-room, the communicating door being almost always open, and were consequently breathing air impregnated with sewer gas.'



3. What was the cause of this sudden death? Was it due to loss of heart power? I think not, because the heart had been acting remarkably well for sixteen hours, and when I left him at 8 P.M., the pulse was 120 with excellent tension. Was it because the stimulants had been withheld and the heart wore itself out? If this were so why did not the pulse show the necessity for re-stimulation? From the history I am compelled to exclude paralysis of the heart as the cause.

Was it due to traumatic pneumonia? I hardly think this

'I am at present attending a fourth case in the same house, a child two and a half years old.

tenable, because there was no paralysis of the muscles of deglutition, and no history of a foreign body entering the larynx.

Then was it due to paralysis of the lungs? During the whole course of the disease there was nothing unusual about the pulse-temperature-respiration ratio until the 3d of January, when the pulse fell and the respiration rapidly rose. This break in the ratio made me apprehensive. What caused these rapid, shallow respirations? If the respiratory centre was irritated by the poisoned blood, why did the circulatory centre escape? As the glandular enlargement before mentioned increased, the respirations became quicker. My opinion is that paralysis of the lungs caused my patient's death—or in other words, paralysis of the vagus. Whether the cause lay in the respiratory centre, or along the tract where pressed upon by the infiltrated ceryical glands, I am not prepared to say.

Treatment.

If there ever was a case systematically managed such was this. Those in charge were intelligent people and at once realized the importance of carrying out every detail. Twice a day, the directions were written out, giving the hour each medicine was

CHESTER A—	
January 2d, 1884.	
WHISKEY—40 DROPS.	2 4 6 8 10 12 P.M. 2 4 6 { VOMITING } A.M. 8-10-12 }
SPRAY.	4 8 12 P.M.
SOL. OF QUININE.	2 12 A.M. 2 { SLEEP } 2 P.M. 6 }

to be given; the hour was to be crossed when it was given. By this means we were able to tell at each visit, how much of each drug had been taken (see table). Again, when the drug was not given there was a reason for it, and the nurse recorded it. By reference to the clinical notes it will be seen that on this date at 7 P.M. the child vomited, and had an irritable stomach the rest of the night, so that he missed the

whiskey. Again at the time for his quinine he was asleep, and soon after he awakened nausea supervened.

As an antipyretic, sodium salicylate has in my hands, in treating children, outranked the quinia salts. In this case the fever promptly subsided under its use, but with the subsidence came a depression of the heart which required free stimulation. Later our attempts to lower the temperature with quinine failed. It is remarkable how much stimulants children with diphtheria can bear. This child took large quantities without apparent effect upon pulse or fontanelle. It may be interesting to note that in two weeks' time these three patients drank fourteen quarts of whiskey. The bronchitis responded promptly to the ammonium carbonate. It may be thought that the patient in the earlier stage was not sufficiently nourished; I grant this if we exclude the alcohol, which can hardly be fair, since it is the best of foods. Later on he surely had enough food.

Having learned that Dr. Armistead Peter, of this city, recently lost a case, I wrote to him and was favored with the following:

"About 6 P.M. September 10th, 1883, Frank W., aged four years, passed my office apparently well. Same evening, nine o'clock, I was summoned to see him; he had a slight fever. Visiting him the next morning I found him covered with a decided scarlatina rash, pulse and temperature good, throat presenting the usual redness of mild scarlatina. September 12th, a well-marked diphtheritic exudation covered right tonsil, quickly spreading over uvula. Appetite, pulse, and temperature good. Case progressing favorably, and on the morning of the 19th throat clean; glands on both sides of neck (which were enlarged) resumed their natural condition, and the slight nasal discharge had ceased. I encouraged the parents with every hope of a good recovery, at the same time warned them that the sequelæ of scarlet fever and diphtheria were often more fatal than the primary disease, especially telling them of sudden deaths from heart trouble, having met with two cases during the last few years. The child was nursed with the greatest care, never allowed to make any effort to help himself or leave his bed. September 19th, 4 P.M., six hours after my morning visit, I returned home and found an urgent message to visit my patient. On arriving at the house was told of his sudden death, and Dr. Kleinschmidt, who had been called to see him at 3 P.M., informed me that at that hour he was suffering from paresis of the heart, but recovered (during this recovery his mother told me the child conversed and seemed to be all right); in about thirty minutes another attack

followed, and death was quickly the result. Drs. Grafton, Tyler, and Louis Mackall also saw the child, but had left the house before I arrived." "I have no theory to advance as to the pathological condition that causes these sudden deaths, except that there must be some latent poison in the blood, peculiar to the disease, that acts directly on the nerve centres. I call your attention to the theory of Edward Woakes, of London, who claims that sudden death is caused by paralysis of the vaso-motor nutrient vessels of the vagi."

"During the nine days' sickness the pulse and temperature were good, never exceeding 100°. Dr. Kleinschmidt saw him on the 16th, pronounced his general condition excellent; the edges of the diphtheritic deposit were then becoming ragged, inflammation subsiding, and, as I said before, on the morning of the 19th all traces of this deposit had disappeared, the tonsils and uvula looking clean and healthy."

In Dr. Peter's case the cause of death was undoubtedly paralysis of the heart. Dr. Kleinschmidt, who was called in when the first alarming symptoms occurred, and remained with the patient until his death, informed me that when he arrived at the bedside the heart had stopped, that he stood the patient on his head and compressed the chest, when the heart very quickly resumed its functions, and the patient recovered as described by Dr. Peter. When the second attack of syncope occurred the same means were resorted to, but the heart made no effort to act.

As the report of neither of these gentlemen states whether there was any undue muscular exertion, or whether the patient assumed the erect posture, of course we cannot conjecture the immediate cause of this sudden and repeated heart failure.

Dr. Ferguson,¹ of Canada, reports three cases of sudden death after the diphtheritic symptoms had subsided.

CASE I.—A young man had made a good recovery, with a slight paralysis of the right arm. He was sitting on a log and threw a stone at a small bird, when he fell over and expired in a few moments.

He attributes death to paralysis of the heart, and accounts for the blood found at post-mortem in the right heart as being passive, due to inspiration. The left ventricle was empty, and must have stopped in systole.

CASE II.—Was that of a little boy, aged five. He was apparently doing well, and while sitting on the chamber he complained

¹ Medical News, Dec. 15th, 1883.

of being weak, was put to bed and died in a few moments. Blood was found in both sides of the heart.

CASE III.—A girl, aged thirteen years, had recovered sufficiently to be in the parlor. While there she was affected so much by the death of a sister that she was carried to her room and died shortly after.

In commenting upon the cause of death in these cases, Dr. Ferguson says that it was not due to the poison affecting the nerve centres, for two of his cases had fairly recovered. In the case of the little boy, the heart's failure might have been due to the poison. Nor can he see how Woakes' theory is tenable in his first and third cases. "It claims that sudden death is due to a vaso motor paralysis of the vagi, so that these nerves are gorged with blood, and the fibrillæ suggillated to such an extent that they are unable to conduct inhibitory impulses, and the heart speedily wears itself out, being exhausted by a succession of rapid beats."

We cannot deny that the heart may be affected by strong emotions. The nerves supplying the heart are undoubtedly weakened by the poison in the blood that nourishes them. In such a condition emotions may act so powerfully, through the mind, on the heart as to produce a sufficient derangement of its action to cause death. Dr. Ferguson believes that the lesion is central.

Dr. S. J. Radcliffe,¹ of this city, reports a case of diphtheria following scarlatina. In half an hour after the doctor left him, apparently doing well, he was informed by a messenger that the child "sat up in bed to be fed, and while in the act suddenly fell back and expired." He says: "I considered the remote cause of death in this case to be asthenia, and its immediate cause heart failure—failure to carry on its intrinsic work in the erect position."

Dr. Radcliffe seems to have lost sight of the probable existence of paralysis of the muscles of deglutition, and of those which protect the larynx from the entrance of foreign bodies during deglutition, and thereby the entrance of food causing death by apnea. He gives us no indications of loss of heart power, for in his brief clinical history he refers neither to the arterial tension nor to the pulse rate.

Dr. R. G. Mauss furnished me the following: Tracheotomy had been performed on a white boy, aged six, on the 6th day of

¹ Medical News, Jan. 5th, 1884.

diphtheria ; was doing well when seen by the attending physicians at 8 P.M. At 9.30 the same night he shivered slightly and within five or ten minutes expired. Tracheotomy tubes were carefully examined and no obstruction discovered.

This history is too incomplete to venture upon the probable cause of death.

In the *Medical News* of January 12th, 1884, is an editorial based upon the four cases above referred to, and from which I shall quote at some length. The editor thinks that these cases are more to be dreaded because they occur at the time of supposed convalescence, when the patient's friends are full of hope, and a recovery has been predicted by the physician. "Very often the end comes when the patient, raised to the erect posture, is in the act of taking food." In such cases, death is attributed to failure of the heart, "when apnea has been induced by closure of the glottis by food or drink." Donders showed that paralysis of the muscles of deglutition is not uncommon in simple cases. The food not being properly conveyed along the proper tract, inspiration may take place and the food be drawn into the larynx ; further, incomplete closure of the glottis may also allow particles of food to enter the larynx, and thus set up a fatal pneumonia.

Paralysis of the respiratory muscles may also take place, causing rapid edema of the lungs and sudden death. "The respiratory muscles may suddenly fail and death ensue with apnea."

"It is quite foreign to our purpose to consider the various lesions of the heart which may occur in the course of diphtheria. We are here concerned with those changes which are common causes of sudden death. These are chiefly two : myocarditis or parenchymatous degeneration of the muscular tissue, and paresis of the cardiac motor ganglia. To the changes of the former may be added the effects of migrating micrococci colonies, and the latter is a part of a general change in the ganglia of the sympathetic. Damaged in respect to one or both of these parts, the heart may still be capable of carrying on its work when the body is recumbent. The fatal paralysis occurs when a change to the upright position requires the organ to put forth more vigorous efforts. Either the ganglia cannot produce the needed force, or the muscles prove unequal to the sudden strain, or both fail simultaneously."

"Another mode of sudden death—cerebral—there is reason to believe, does happen more frequently than is now supposed. We refer to cerebral hemorrhage and the so-called serous apoplexy, both accidents which may attend on albuminuria. In making this statement, we are not unmindful of the convulsive phenomena and the direct interference with the respiratory centre, which may be due to the cerebral changes consecutive to albuminuria."

It may be claimed by some that my case is not properly classed and that it only ran the usual course of the disease. All the cases referred to, except one of Ferguson's, died during the convalescence. I do not claim that my patient had convalesced, but that that period was at hand when the acute symptoms had subsided, the membrane had disappeared, the pulse and temperature excellent, and when there was every indication of a speedy recovery. If my patient died as other cases, how can we account for the excellent pulse during the twenty-four hours preceding death?

What is meant by sudden death? Are we to conclude that only such cases as have been reported, where the patients were up and about, and from undue excitement or exertion dropped dead, are the only typical cases of sudden death? Surely, no one would feel justified in maintaining such an opinion. If a case is progressing favorably, and there is every prospect of a speedy recovery, when, from some unexpected change, whether the cause be known or not, the patient in a few hours succumbs, that is just as much a sudden death as if he were apparently well and attending to his accustomed duties, and should without apparent cause expire.

"The end of life may still be said to occur suddenly, even when the death struggle has lasted several hours. What is essential in the conception of this mode of death is the unexpectedness of the event relatively to the previous condition of the individual. The symptoms of agony last from a few minutes to a few hours, consisting often only in insensibility, insomnia, convulsions, and difficult breathing. A rapid death is frequent in the earliest years of life. . . . It is once again as frequent in males as in females."

"The causes of death are numerous, but the following divisions may be recognized:

Cessation of the circulation, through various diseases of the heart and great vessels.

Cessation of the respiration, through the action of various causes (suffocation and strangulation, deficient nutrition, either produced by albumen or water).

Extreme or long-continued elevation or diminution of the temperature of the body, and simultaneous deficiency of regulating means.

Severe physical and chemical agencies (purulent infection).

Severe psychical impressions.

Those organs which are the instruments of the most important activities of life, and whose injury most quickly causes death, were called by the ancients *atria mortis*; they are the heart, lungs, and the brain, or, to be more exact, the medulla oblongata. Hence we have, especially for the laity, three different modes of death: 1st, death from the brain called apoplexy (as in concussion of the brain, large extravasations); 2d, death from the respiratory organs, by asphyxia, or, more properly, by suffocation (as by the breathing of irrespirable gases); 3d, death from the heart (as, for example, in rupture of the heart). These three modes of death seldom, and only in sudden death, occur in pure forms. Usually, especially in slowly produced death, they combine with each other in various ways; for example, interference with the circulation alters the composition of the blood, and this, in its turn, impedes respiration, and both together diminish the heart's activity. In individual cases, even when a well-conducted autopsy is made, it is often difficult to determine the exact way in which death has been brought about."

By a study of these general causes of death, and comparing them with the history of my own case, I am compelled to exclude from the probable cause the last four. I cannot believe that my case died from deficient nutrition, for emaciation had not taken place to any marked degree, and at the time of death there were no evidences of exhaustion sufficient to cause death. Owing to the age of the child, it was impossible to obtain any urine for chemical analysis, so that I do not know whether albuminuria existed or not. If there was an excessive amount of water in the system, there were no evidences of it, for the pulse was good, and there was neither circumscribed nor

general edema. It was not until the death struggle set in that edema pulmonum began.

The variation in temperature could not have produced death, for the daily excursion was sufficient to give the nervous system a chance to recuperate.

There was no physical agency to account for the death. If it was due to the chemical agency (purulent infection), it would seem from the age of the patient as though death should have occurred some days sooner, and then too that there would have been more evidences of the system being overwhelmed by the poison.

In one so young, psychical impressions cannot be considered a factor in the causation of death. In the young lady referred to by Ferguson, the fright and grief at the death of a sister produced psychical changes sufficient to cause death in a very few moments.

Then I think in my case that I am warranted in concluding that death was the result either of cessation of the circulation or of the respiration, and that, from the history of the case, the heart held out longer than the lungs.

J. Lewis Smith says that death may result from: 1st, diphtheritic blood-poisoning; 2d, septic blood-poisoning; 3d, obstructive laryngitis; 4th, uremia; 5th, sudden failure of heart's action; 6th, suddenly developed passive congestion and edema of the lungs probably due to feebleness of the heart's action, or to paralysis of the respiratory muscles. In support of this last cause, he cites an instance where edema pulmonum occurred a month after the disappearance of the faucial membrane. The patient revived, but the following day the edema recurred and the patient died.

In order to understand how death might be produced by the destruction of the function of either of these organs it is necessary for us to study the nerve supply of the heart and lungs in a physiological sense. For this purpose, through the valuable assistance of Dr. Louis Kolipinski, of the Children's Hospital, D. C., I am enabled to give the generally accepted views of physiologists upon the complex workings of the nerve supply of these important organs.¹

The following are the nerves of the heart: The plexus

¹ Landois, *Lehrbuch der Physiologie des Menschen.*

cardiacus is composed of the following nerves: 1. Ramus cardiacus of the vagus trunk and the branch, of the same name, from the ramus externus of the superior laryngeal, of the inferior laryngeal, and sometimes also from the plexus pulmonalis of the vagus—more often on the left side.

2. Of the ramus cardiacus superior, medius, inferior, and minus of the third cervical and first dorsal ganglia of the sympathetic.

3. The inconstant branch, the ramus descendens hypoglossi, which is said to originate from the first cervical ganglion (Luschka). From this plexus pass out the deep and superficial nerves; the latter, as a rule, receive a ganglion at the division of the pulmonary artery under the arch of the aorta. We can distinguish as going out of this plexus: *a.* Plexus coronarius dexter et sinister (Scarpa) which contain the vaso-motor nerves (physiological knowledge of these is still wanting), and also descending fibres (sensory to the pericardium) (?).

b. Nerves lying in the grooves and in the substance of the heart, which are richly provided with the ganglia (Remak) which we recognize as the automatic motor centres of the heart. A ring of nerves rich in ganglia borders the edge of the inter-auricular septum; another is found in the auriculo-ventricular margin; where these meet they interchange fibres.

The ganglia are situated principally near the pericardium. In mammals, the two larger lie near the insertion of the superior cava. In birds, the larger mass, containing thousands of ganglia, lies at the posterior intersection of the sulcus longitudinalis and transversalis. From these masses fine nerve fibres (bearing on them ganglia) penetrate the muscles of the auricles and ventricles.

In the frog, near the vagus fibres in the walls of the sinus of the cava, lies a large ganglionic plexus (ganglion of Remak). From this ganglion the vagus fibres pass as the anterior and posterior nerves of the septum, and each of these has a second ganglion (ganglion of the ventricle: Bidder's), from which can be traced, for a short distance only, nerve-fibres, so that the greater part of the ventricle seems to be without nerves.

By further analyzing the complex functions of this nerve-supply, we find that there are accelerator, automatic excitor, inhibitor, and vaso-motor nerves with presiding centres.

The action of the accelerator centre can only be tested after division of the splanchnics. This centre is not tonically excited, because section of the nerve does not slow the heart's impulse, and destruction of the centre in the medulla is without effect.

The heart has within itself automatic excitor centres, probably in its ganglia. These centres are connected with each other. They are regulated by the chief centre, which is in the cord. By irritation, these centres are stimulated into spasmodic action. The dominant centre is in the auricle. By depressing this centre (opium topically), the impulse may be transferred to the ventricles, and contraction may start from them. The centres of the auricles are the more irritable. Reflex irritation from the interior of the heart (endocardium) acts most markedly and rapidly. Blood-supply is a necessary part of the acting heart. In removing the auricular sinus, the remainder of the heart does not contract, because the ganglion of Bidder has not in itself the power of exciting the contraction, and also because the section irritates the inhibitory (vagus) nerve. Some hold that, in the heart, there are three ganglia—Remak's and Bidder's excitors and Ludwig's inhibitor. The first and second together are stronger than the third, but the third is stronger than either of the others.

The centre of cardiac inhibition (vagus) is seated in the medulla oblongata. The fibres of the vagus, when moderately irritated, diminish the number of heart beats; strongly irritated arrest them. They are from the spinal accessory. This centre is acted on directly and reflexly.

Directly: 1, Sudden anemia of the medulla; 2, sudden venous hyperemia; 3, increased quantity of carbon dioxide in the blood; 4, during inspiration; 5, increased blood-pressure in the cerebral arteries. Reflexly: 1, Irritation of sensory nerves; irritation of cerebral end of the cut vagus on one side; 3, irritation (blow on the sensory nerves of abdomen).

It is generally acknowledged that the vaso-motor centre is situated in the medulla near the calamus scriptorius. Irritation of this centre produces constriction of the arteries and rise of blood-pressure. It is directly excited by: 1. The composition of the blood as regards its gases; an excess of carbon dioxide produces constriction of the blood-vessels and engorge-

ment of the veins and heart, whereby the rapidity of the flow is increased. The same effect is produced by anemia of the medulla, and also by stagnation of blood in venous hyperemia. Reflexly excited by : 1. Pressor nerves which increase the vaso-motor action, inferior and superior laryngeal, trigeminus, cervical sympathetic, and any sensory nerve of the body (Lovin).

2. Depressor nerves, which inhibit the action of the vaso-motor centre; depressor of Cyon; vagus. Also said to be found in all sensory nerves.

Paralysis of the vaso-motor nerves causes the heart to contract slowly and feebly, the reverse when the vaso-motors contract the blood-vessels. Paralysis of the splanchnics may so fill the abdominal vessels with blood that death may result from anemia of other organs, *i. e.*, intra-vascular death from loss of blood. The phrenic, spinal, spinal accessory, and vagus nerves are the principal ones concerned in the mechanism of respiration, and their distribution must be understood in order to intelligently study the action of the respiratory centre.

Le Gallois recognized that the respiratory centre must be contained in the medulla. Flourens determined more accurately its seat to be behind the origin of the vagus, on both sides of the calamus scriptorius of the fourth ventricle, and between the vagus and spinal accessory nuclei. He called it *Point, or Nœud, Vital*. This centre is bilateral, and can be separated by longitudinal section (Longet, Volkmann, Schiff), and thereby respiration is performed symmetrically on both sides. When a vagus trunk is cut, respiration is slowed on that side; on double section bilateral respiration is the same. Irritation of the cerebral end of a cut vagus stops the respiration on that side; the same result is attained on irritating the trigeminus (Longendorff). On transverse section of one centre, respiration stops on that side. This centre is composed of two alternately acting centres, one inspiratory, the other expiratory. It is automatic. Its excitability depends on the quantity of oxygen and carbon dioxide in the blood; hence we have apnea, empnea, dyspnea, and asphyxia.

Dyspnea may be caused by : 1. Any agent preventing the action of the respiratory organs, as stenosis of the air-passages, pneumonia, etc. 2. Prostration of the circulation, as

heart disease, obstruction to the flow of blood from the brain, etc. 3. Hemorrhage.

Irritation of the peripheral end of the cut vagus stops the heart and also the respiration for a short time, because of anemia of the medulla.

The centre is excited reflexly by the pulmonary branches of the vagus, the nerves of the eye, ear, and skin.

It is reflexly depressed by the superior and inferior laryngeals, nasal branches of the trigeminus, sensory nerves of the skin, of the thorax and abdomen, and by pressure on the brain.

It must be recognized that, if paralysis of either the cardiac, respiratory, or vaso-motor centres takes place, death quickly follows. These three centres are situated in the medulla oblongata. Their powers of resistance must be different, for only one seems to be affected at a time. Whether this sudden overpowering of the centre which causes the sudden dissolution is due to the millions of micrococci, to a specific unknown poison, or to an individual idiosyncrasy incapable of combating with the ordinary diphtheritic poison, remains to be discovered.

Let us suppose a case of sudden death from paralysis of the heart. According to Dr. J. Lewis Smith, it must be due to one of three causes—anemia, granulo-fatty degeneration of the muscular fibres of the heart, or to ante-mortem blood clot. The first is the potent cause which, by enfeebling the heart, causes less oxygenated blood to circulate through the respiratory centre, which is paralyzed, and death results from asphyxia. The respiratory centre is composed of the nuclei of the vagus, trigeminus, spinal accessory, and glosso-pharyngeus (Meynert), and hence asphyxia means paralysis of these nerves. But suppose it be due to the degeneration of the muscular fibres. We are well acquainted with the fact that the muscular fibres of the heart are different from those of the general muscular system, for, by irritating the sympathetic, it is impossible to produce a tetanic contraction of the heart's muscles, when by a similar irritation this contraction can be produced in muscles generally. Further, by irritating the pneumogastric, we are not able to stop a contraction of the muscles already begun, but merely to prevent the succeeding beat. If, then, from any cause, this peculiar muscular fibre be destroyed,

the central mass must lose its influence, and the cause of death will certainly be failure of the heart.

Or let it be due to ante-mortem blood-clot. The pneumogastric nerve might be irritated by food taken into the stomach, and the heart stopped just long enough to form a clot. Energetic treatment may overcome this attack of syncope (as in Dr. Peter's case), but a second attack probably carries off our patient by pulmonary embolism.

But if it be due to passive congestion and edema of the lungs from feebleness of the heart's action, the same central mass is affected, the primary cause being from without. In this case the heart stops after the respiration. Asphyxia irritates the inhibitory nerve, thereby slowing the heart's action, but stimulates the vaso-motors, whereby the venous system and heart are full of blood, and arterial tension is increased.

This destructive element, whatever it is, may be circulating in the blood and be continually irritating the cardiac centre. This central mass may, through its power of resistance, protect itself for a long time, but constant irritation must necessarily reduce the resisting power, and paralysis is the result.

If this destructive force is concentrated on the inhibitory centre, what will be the result? This nucleus being the cardiac moderator, if its power is lost, the heart will soon wear itself out by a succession of rapid pulsations. But the influence of the vagus on the respiratory movements must not be lost sight of; paralyze it, and the respirations become slower and slower, until they finally stop in expiration.

The automatic centres may be the ones seriously affected. They are governed by a chief centre in the cord, whose power of resistance ought to be equal to that of the central mass.

Destruction of the accelerator centre seems to be without effect, so its paralysis cannot be considered a prime factor in the causation of sudden deaths.

Let the vaso-motor centre be the paralyzed element, and what is the result? The heart contracts slowly and feebly, arterial tension is diminished, and the venous system is engorged, and death may result from congestion of the internal organs.

But let this destructive element expend its force on the respiratory centre. The effect will most likely be paresis of the

diaphragm and lungs, edema of the lungs rapidly supervening. This infiltration causes rapid superficial respirations (dyspnea), the blood is not sufficiently oxygenated, and, surcharged with carbon dioxide, is conveyed to the central masses. Hence the respiratory centre, being constantly irritated by this non-oxygenated blood, calls on the auxiliary centres presiding over the muscles of forced respiration, which for a short time supply the place caused by the paralysis of the phrenic and vagus. But as the quantity of carbon dioxide increases, the vaso-motor centre is stimulated, the heart-beats become slower and stronger, the vascular tension increases, and thus nature endeavors to compensate for the impairment of the lungs.

ABSTRACT.

1. Lœri: Symptoms of Gastric Disease due to Inflammation of the Larynx in Children (*Jahrbch. f. Kindhikde.*, XXI. B., 1 u. 2 H.).—The combination of disease of the stomach with pharyngitis, laryngitis, etc., has been long known, as well as their causes. Disease of the larynx or pharynx may often also extend to the stomach—either per continuitatem, by swallowing of the secretion of catarrhal, ulcerative, or malignant inflammation, by reflex irritation, or in consequence of the constant desire to swallow, causing a frequent clearing of the throat and forcible swallowing of a small quantity of mucus, accompanied each time by a little air, which may be thus pumped into the stomach in quantity sufficient to cause serious derangement. Yet there are no cases recorded such as Lœri now reports—namely, where in children inflammation of the posterior surface of the posterior larynx wall caused severe gastric symptoms. In one case a boy eleven years old had suffered from persistent vomiting of everything he ate for about three weeks, in spite of careful treatment. All food was vomited a few moments after it was swallowed. The boy was intelligent and complained of nothing but slight pain in the stomach. On examination the posterior wall of the larynx was intensely congested and swollen so that it projected like a half hazel-nut. On the right side of the swelling was a raised, white point, which proved to be a fish-bone five cm. long. This was removed and all symptoms of stomach trouble vanished. Another child, five years old, had complained for three weeks of pain in the larynx and stomach, and during the last two weeks had had constant eructations, occurring ten to twenty-four times a minute and only partially ceasing in sleep. The appetite was poor and the child pale and thin. Examination showed the larynx to be normal except on the posterior wall. Here there were two raised points of inflammation. Tannin was insufflated, and in two days all symptoms were improved, and after repeating the insufflation daily for two weeks the case was entirely cured. The author reports several cases of the same tenor, to show that in children it is not extremely rare to find gastric symptoms arising from inflammatory disease of the posterior wall of the larynx.

J. F., JR.

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ORIGINAL COMMUNICATIONS.

A NEW METHOD OF UTERINE FARADIZATION.

BY
DR. G. APOSTOLI,
Paris.

(With 4 cuts.)

THE induced current as a therapeutic agent in uterine disease is beginning to enter as a factor into the practice of medicine. Opposition, technical ignorance, or the unwillingness to make a start are on the point of disappearing, and we can already foresee the day when the method shall have become classic. All that it lacks is official recognition, and I trust it will soon obtain this, for in France, unfortunately, we are under and we remain under the ban of authority. In order that a discovery may obtain scientific diffusion, real and lasting, it must needs have sanction from above. Such, in fact, is the history of the many innovations which have originated from amongst those whom I would call the "great irregulars" in medicine; and, to cite but one example: Do we not all remember that great ignored who was obliged to await almost his dying hour before his name and his deeds received scientific recognition, and who, alas! cannot be present at the apotheosis which hands formerly at enmity would now rear for him,

smothering him beneath their laurel wreaths? I refer to Duchenne, of Boulogne.

The chapter of the ignored is a long one in the world medical, and there exists, perhaps, another of whom I, more than any one else, can speak, as both pupil and friend ;

I mean Dr. A. Tripier. For twenty-five years he has written paper after paper, some more original and of a higher standard than others, and his recent book on the diseases of women is perhaps one of the most marked appearing during the past ten years. It is here that Dr. Tripier elaborates and generalizes the whole of his extensive experience in uterine electrotherapy. I say *his practice*, for it is his creation, absolute, and without opposition.¹ I will not attempt here a long description² of this book. I wish simply to call attention to an instrumental modification of his operative method, and at the same time to endeavor to clearly define the clinical importance it has, from my point of view. Tripier's method,³ in short, even though its theoretical aim be a perfect one and its

¹ *Leçons Cliniques sur les Maladies des femmes.* Par le Dr. A. Tripier. Paris, 1883. (Thérapeutique générale et Application de l'électricité à ces maladies.)

² Vide my book on *Technique Gynécologique* (A. Delahaye and E. Lecrosnier, Paris), in which uterine electrization is described with many figures in the text, and with minute detail.

³ "The method of uterine faradization varies according to the indications to be met. In simple congestion, it is my aim to provoke contraction of the entire uterus. We obtain this best by *abdomino-uterine* faradization. The patient, on her back, as for a specular examination, we apply above the pubes, on either side of the recti muscles, the electrodes, joined to a positive bifurcated rheophore, which close the circuit over the abdomen. The uterine excitor (Fig. 1) is then introduced within the cervix, or even, when the external os is sufficiently open, pushed to the fundus, passing it along the palmar surface of the index finger. To this excitor is joined the negative pole. The battery is then set in motion, beginning with a low current, and increasing the intensity progres-




FIG. 1.—Simple Uterine Excitor, or Mono-Polar of Tripier.

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clinical results incontestable, has suggested to me certain exclusively and almost continuously, being guided by the sensations of the patient, stopping the battery when the pain seems great, and beginning again as the pain diminishes. We can also act on the entire uterus by means of *sacro-uterine* faradization. The patient being placed in the same position as above, a metallic disk, covered with wet skin, is placed over the sacro-vertebral articulation. The negative rheophore is joined to a uterine excitor.

"This method, which requires neither the presence of an assistant nor the help of the patient, is the one I most frequently have recourse to in obstetrics, either to check hemorrhage or to promote involution.

"*Lumbo-pubic* faradization localizes the action less than the above-described methods. A large wet electrode is applied to the lumbo-sacral region and another above the os pubis. The former is attached to the positive pole, the latter to the negative. The patient is sitting, and the handles of the electrodes can be held by her. This is the method I use in case of virgins. It is practically serviceable in case of amenorrhea and dysmenorrhea. I often resort to it, the uterine method not being indicated, in the case of women at the time of the menopause to avert congestion of the lungs and of the brain. In such cases, the séances must be longer than when the object is to produce contraction of the uterus. At least five, or, better, ten minutes are requisite.

"Such are the methods which I ordinarily employ in order to cause contraction of the entire uterus or to bring about congestion of the pelvic cavity. I formerly used others which I will simply recall here without recommending them. For instance, it has happened that I have been consulted by ladies on the eve of their departure from town, and in consequence could give them only one or two séances, and in such cases I have introduced the negative sound within the uterus, and at the same time introduced one sound into the bladder and one into the rectum, the two being joined to a positive bifurcated rheophore. This recto-vesico-uterine method has the disadvantage of being relatively complicated and requiring the presence of a skilled assistant; and, finally, it does not always give rise to more energetic contractions than the methods which I have already indicated. Indeed, it is the pain caused by the contractions, or, rather, the pain felt beneath the external electrodes, which guides us in regard to the degree of intensity of the current to be used; therefore, by making the external excitor or excitors sufficiently large to cause the cutaneous pain to be sufficiently feeble, we have simply to take into account the uterine pain, which we can excite as much as it is useful, and prolong as long as the first séance requires, at which time the pain ordinarily is slowest in declaring itself.

"I have also, in case of great uterine congestion in the virgin, practised *recto-vesical* faradization, introducing first the vesical sound and then the rectal excitor. The uterus is thus fixed between two excitors, the one anteriorly and the other posteriorly. The presence of an assistant to operate the induction apparatus complicates this method, and the sounds are also easily displaced. It has given me good results. I recommend it, however, with diffidence. The *lumbo pubic* method will answer as well in the majority of cases"—*Leçons cliniques sur les maladies des femmes*. Par le Dr. A. Tripier, page 95. Paris, 1883.

periments which have led to a complete change in the technique of the operation.¹ These are the reasons for the change and their significance.

¹ The 20th of February, 1883, I presented to the Academy of Medicine the instrument which furnishes the subject matter of this paper (double uterine excitor), with the following description—Fig. 2:

“This instrument is intended to substitute double or bi-polar faradization, the two poles being within the uterus, for the method which up to to-day has been exclusively used, namely, uni-polar faradization, where one pole is within the uterus and the other most frequently on the abdomen. The new method, as simple in practice as the old, is destined to make the operation:

“1st. *Easier*, since the presence of an assistant is not necessary.

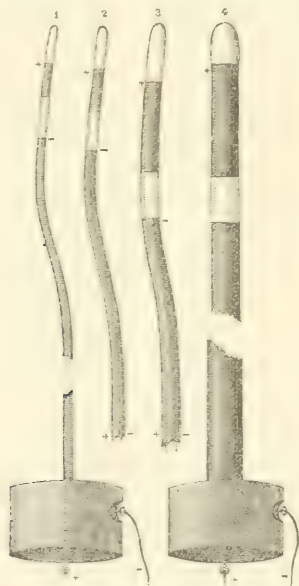


FIG. 2.—Double Uterine Excitors or Bi-polar of Dr. Apostoli.

- (1) Small model.
- (2) Medium model.
- (3) Large model.
- (4) Uterine excitor after confinement, and double vaginal excitor.

“2d. *Less painful*, since all application to the skin is done away with, and, in consequence, the pain caused by placing the electrodes over the pubes.

“3d. *More active*, since the action of the current is localized within the uterus, and we are thus able easily to use the highest electrical powers of medical batteries, which was only rarely possible formerly.

“4th. *More efficacious*, since uterine contractility is increased, and, in consequence, those therapeutic effects which are its direct results.”—*Gazette des Hôpitaux*, No. 26, page 206, March 3d, 1883.

Whoever has often resorted to uterine faradization, and I think that after Tripier I have done so oftener than any one else, in number nearly several thousand, must have been struck by the opposition offered by certain patients, at times very great, and by the operative difficulties met with.

The method, in fact, is painful, and I am speaking now particularly of the utero-pubic or abdomino-uterine and sacro uterine¹ methods applied especially to the treatment of simple congestion and of metritis. It is at the cost of suffering that the patient attains amelioration and cure; but can we always draw the line? Such is the burning question. Tripier answers yes, and the fact that he is father to the method makes him perhaps deaf to the cries of his patients. And yet, to rebel a trifle against my teacher, my independence obliges me to confess that at times the method is *painful*, even *very painful*, and that in some patients it is *intolerable*. The pain indeed is a double one, for each electrode has its own, the one within the uterus and the other on the skin. We must consider each in turn.

Let us consider, in the first place, the uterine pole. Given the same faradic action, given the same electric intensity, and one uterus will behave very differently from another.² One patient bears the pain without complaint or opposition; another utters piercing shrieks, moves about, and at times renders any faradization impossible; between these two extremes we have every shade of behavior, and this is no surprising matter, knowing, as we all do, from experience, how differently women behave during the pains of labor, and how variable this factor is with all, varying markedly in the same woman at different times.

But the *uterine* is not the sole pain provoked by the electric current; side by side, and to a greater degree, ranks the *cutaneous* pain arising from the application of a pole, through

¹ That is to say, the most active of Tripier's methods, where one pole is always within the uterus and the other either above the pubes or over the sacrum.

² A special chapter of my *Technique Gynécologique* is devoted to this important question, clinically as well as therapeutically. I here consider the influence, respectively, of tension and electric quantity on the uterus. The differences in individual receptivity in health and disease. The irritable uterus of the ancients. I trace the general law of normal and pathological reactions and their varieties.

the medium of large tampons, most frequently above the pubes, at times over the sacrum. This pain is the most intolerable and acute, and I have, above all, endeavored to dispense with its necessity; for around it gravitate almost every objection, as well as each desideratum belonging to the method, as we will shortly readily perceive.

Again, whilst the method is in itself painful, a further objection is that the patient is obliged to hold within her hands the instruments of torture, as is the case in the *pubic* method.¹ At the hospital or at the clinic, assistants are always handy, but in one's office the patient, being alone, must perforce hold the tampons, and this entails a number of disadvantages. The minimum sensibility from the side of the skin is obtained by an energetic application of the tampons, the pressure being strong enough, even, and identical during the entire séance; any variation—if one tampon be lightly lifted or unequally applied—determines at once a considerable increase in the pain, because the cutaneous resistance becomes stronger, these two terms, cutaneous resistance and pain, being here considered proportionate.

The obligations thus laid on the patient, and which the operator cannot second, seeing that one hand holds the uterine sound and the other operates the battery, are thus at times beyond her power and render the perfect execution of Tripier's habitual operative method most difficult, the pain being in no sense lessened.

Is this all? No, indeed; and here follows the most annoying clinical result:

If the patient suffers, and at times unbearably, the operator can risk nothing, he must proceed slowly and with great care, and must limit himself to very small electric doses; or, since the uterine contractility is, in general, in direct ratio with the intensity of the electricity used, we can practically lay down the law—from a small dose (the proportions the same) results a small contraction, and this last increases with the intensity.

Therefore we are often obliged to face the following dilemma:

¹ My argument takes account particularly of the *utero-pubic* method, since it is the one which Tripier habitually uses, as being more powerful than the other methods.

Either cause pain at all hazards, in order to obtain the desired end—uterine contraction; or else cause as little pain as possible and render the operation almost illusory in its clinical and therapeutic results.

Such, then, in strict truth, is Tripier's method, perfect in its aim, but frequently lame in its method of action.

To resume now, uterine faradization applied to the treatment of simple congestion or metritis has consisted almost exclusively, up to the present, in the application of the *utero-pubic* or *abdomino-uterine*, at times the *sacro pubic* method.

Well! I claim that these methods are often *too painful* and, consequently, *difficult in execution*, and entailing often only a *partial result*.

Can we do better? I think so, and here is the modification I propose:

In place of unipolar faradization, exclusively used up to the present, a single pole being within the uterus, substitute double faradization, the two poles being within the uterus.

This is the sound¹ which I have had constructed; it is identical in shape and dimensions to Tripier's sound, the extremity of which is uncovered; but it differs in that this extremity contains two poles separated by a non-conducting material. The current enters, as usual, at the top of the sound, and its stem is composed of two metallic cylinders separated from one

¹In Fig. 2 I show four different models, each of special value. Models 1 and 2 are oftener used and are daily applied by me to the treatment of metritis. The difference in thickness depends on the variable calibre of the internal os and cervico-uterine canal. Since the sound must be introduced with all gentleness, that one must be chosen which has the right calibre.

Model 3 is larger still and with a greater space between the poles. It is intended for the voluminous uterus with widely dilated canal, and for use either during the puerperium or to hasten involution.

Whilst it is important that the sound should not be too large, lest its introduction be difficult or do harm, it is equally important it should not be too small, for the two poles must be in contact with the walls of the uterus. This is my apology for the large sound. Model 4 is also, for the same reason, of use after delivery. It can also be used for vaginal faradization (one pole on the cervix or in the cul-de-sac, the other in the vagina). It must be used after this fashion in the virgin.

It will be noted that I have adapted to all my excitors a knob, which Tripier's instrument lacks. Thus they are more readily held, and the sound cannot slip from the hand.

another by a non-conductor, and each appearing separately, uncovered, at one of the metallic ends of the sound.

The manner of using it is identical with that of Tripier; the sound is introduced into the uterus and held as far as possible in contact with the anterior wall of the organ.

All the advantages accruing from this modification are at once apparent:

1. *Suppression of the cutaneous pole.*
2. *Concentration within the uterus of the entire electrical action.*
3. *Ease of operation, neither an assistant nor the patient being required to hold the tampons.*
4. *Diminution of pain, owing to the absence of any application of the current to the skin.*
5. *Wider applicability of the method, owing to its greater ease and completeness of execution.*
6. *Its greater efficacy, since the highest degree of uterine contractility is obtainable with ease and the least pain from the use of stronger currents, of greater intensity, and consequently more active.*

Clinical experience accords entirely with my theoretical expectations. For a long time, I have used this method exclusively both at my clinic and in my office, and many a time I have determined by comparison how much, in the first place, it is preferred by my patients, from the single fact that pain is thereby lessened, and that it is more convenient, and by how much, in the second place, it appears to *hasten the cure of metritis*.

This last consideration requires further comment. We all know the difference which exists between the contraction of smooth and striated muscular fibre, how active and immediate the one is, and how the other is often slow and tardy.

The contraction of the uterus when empty has been the subject of much discussion, more recently through the publications of Dembo and Onimus (*Académie des Sciences et Société de Biologie*). To deny its occurrence is, to my mind, to deny the evidence of one's senses, particularly when one has applied electricity to a number of women, and has recognized the contraction, as well as felt it at the end of the sound.

It is incontestable that the contractility is variable, both as

to time and as to intensity. With one woman, a few minutes are requisite for its manifestation; with another, it is present at once. The same differences are noted as to its strength and power. Thus, then, we have established the first clinical fact.

Experiments on animals have taught us another very interesting clinical fact, that this contraction rarely occurs *en masse*, as is the case with striated muscular fibre. It occurs, so to speak, from point to point, progressively at the outset, localized where applied, and thence spreading little by little, according to the electrical intensity, until the whole organ has become invaded.

From this fact, uniformly granted, is it not allowable, by way of analogy, to draw the following clinical consequence in the case of woman—that, by multiplying the points of current-contact, the electrical action is increased, and that, by concentrating the two poles within the organ, we will realize the greatest possible maximum action from a given dose? This conclusion, which I propose to prove by means of experiments on animals, clinical experience would seem to fully justify in woman, and so I believe that my method is *less painful* and *more active*.

As regards pain, I have learned something previously unknown to me, and which I can nowhere find noted; there is an absolute difference of *electrical sensibility* between the *body* and the *neck* of the uterus. Often have I made the following experiment: In the case of the same woman, I introduce my small sound up to the internal os; it thus is astride of the cervix by its two poles, and is not applied directly to the body of the uterus. I note the pain caused by a certain strength of the faradic current, and then, at the same sitting, without changing the current, I push my sound up to the fundus, so that its two poles are concentrated within the cavity of the body. The resulting change is constant and immediate; the pain, which was always acute enough with the cervix, became tolerable, and at times *nil* with the body; and if, inversely, the experiment be performed, passing from the body of the uterus to the cervix, pain immediately increases, as the patient tells us after a more or less energetic fashion.

This fact is of interest for more than one reason, and ob-

liges us, in order to obtain the minimum of pain and the maximum of action, to push the sound into the cavity of the uterus whenever it is possible.

As to the greater activity of this operative method, it is manifest to me, and besides easily understood when we consider that the current strength employed is frequently double that in use by the other methods.

In Tripier's method, the mean faradic current is the induction obtained by the withdrawal of one-half the coil from the apparatus which bears his name.¹ Rarely have I seen women support the maximum, and in this place I mean always the entire coil of *large and short wire*, or the quantity current, the only one of use in the treatment of metritis. As for the *fine wire* coil, it has far different indications and uses.²

In the method which I propose, the reverse is the rule; the maximum strength is often used, and only rarely, particularly when the sound is astride the cervix, am I satisfied with a medium dose.

But it may be asked, Is it useful to thus increase the electric intensity, and thus force the strength of the induced current?

I may answer shortly that, if uterine faradization has sometimes failed to cure metritis, it is on account of the weak application of the curative agent."

To cite an example from obstetrics, uterine inertia, and in particular metrorrhagia, are more happily influenced by the doses I recommend than by those which are in daily use, and which, to my personal knowledge, have at times been insufficient.

Such is my method. Is it beyond reproach? Tripier, although ignorant of my experiments, has, by anticipation, made

¹ This apparatus is constructed by GaiFFE, and is run by two Leclanché tension elements. It has the advantage of giving an intensity at will from the zero to the maximum. It is the zero which it is important to use at the outset in uterine faradization.

See my *Technique Gynécologique* for a description and the respective advantages of the induction machines of *Trouvé*, *Chardin*, etc., etc.

² Consult my *Technique Gynécologique* for a full development of this question.

³ In the treatment of perimetritis, I shall show, in a paper soon to appear, that we must proceed inversely both as regards electrical intensity and tension.

certain objections in the book he has just published. Here is his objection, and my answer follows:

After a description of the different methods of uterine faradization, he adds (*Leçons Cliniques*, page 99): "There is still another method of faradization, entirely uterine, which I will recall without recommending.

"In certain cases of prolapsus, where the congestion of the



FIG. 3.

FIG. 3.—Dr. A. Tripier's Double Uterine Excitor.

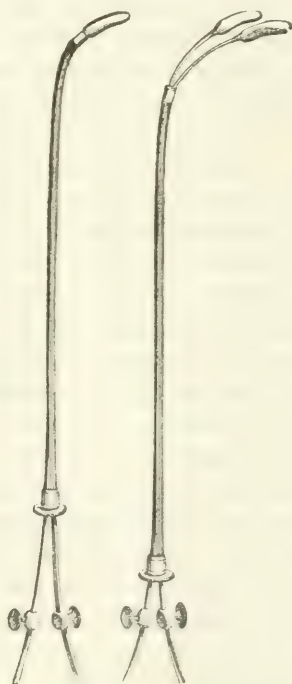


FIG. 4.

FIG. 4.—Double Uterine Excitor of Duchenne.

cervix was out of proportion to that of the body, and in other cases where the dilatation of the internal os was very marked, I have introduced both the electrodes within the uterus at times by means of a double excitor, shaped like the sound (Fig. 3), and again by means of an annular excitor fitting over the external os, in the interior of which slides a single uterine excitor.

"This method has the disadvantage of giving the patient very

unequal sensations. The two excitors being very near one another in a place where they are bathed by liquids, the currents are with ease derived unequally; whence variations in intensity, the more marked because, from the very fact of variation, it is necessary to employ stronger currents. This is a fault in the method which has led me to relinquish this manner of operating. There exist, indeed, no indications for total uterine contraction which cannot be met as well by *abdomino-uterine*, *sacro-uterine*, or *lumbo-sub-pubic* faradization. These methods are not at all or only a trifle painful (?); they are in application more convenient (?). Finally, it has not been proven that they are inferior in rapidity of result to those which I have just described to you. And so, after a trial of both, I would advise you to practise the first."

This criticism, however, applies only to the two instruments he describes and of which he is the inventor, and does not touch the third, the one I propose.

These instruments are in fact essentially defective, and the reason why they have been abandoned is explained by his own reasoning, as well as by the following:

His first double excitor, in shape like the sound (Fig. 3), has the great disadvantage of containing the two poles in the thickness of the sound only a few millimetres apart and almost touching one another; the current, therefore, is interrupted or suffers constant variations during the application, owing to the difficulty of holding the sound in place; for, at the least displacement, one pole will be in contact with one wall of the uterus.

His second excitor, the annular, one pole of which surrounds the cervix, has, in addition, the great disadvantage of being very painful, owing to its application to the cervix and the possibility of contact with the vagina.

My instrument, on the contrary, has the advantage of the two poles being far enough apart to prevent any variation or interruption in the electric intensity, attached as they are to the sides of the instrument.¹

¹ I would compare the double excitors of Tripier to that of Duchenne (of Boulogne) (Fig. 4) which has the following objections: It is composed of two flexible metallic branches introduced within a double-current sound, and isolated. These two branches end anteriorly and superiorly each in a knob which are applied to the sides of the uterus. The other extremity is connected through screws with the apparatus generating

To obtain increased action, I have had several models constructed in which the poles are at variable distances, and which are also of different calibre.

Still further objections have been urged against my method. M. Thèvenot, in a recent communication, claims: 1. Whilst my method entails less suffering than Tripier's, this is not reason enough for abandoning the latter. 2. In Tripier's method the electrode is introduced into the cervix, in my method into the uterine cavity. Now, the uterine mucosa being most frequently diseased, the introduction into the cavity is often painful, and there is greater danger of harm being done than when the electrode is cervical. 3. It cannot be granted that the electrical reaction is concentrated within the uterus; on the contrary, this reaction spreads to the periuterine tissue, and thence acts favorably on chronic inflammations. Both Tripier and I, he says, seem ignorant of this, and apparently dread the action of the faradic current on tissues which have antecedently been inflamed. The future will prove them in the wrong.

I answer these objections categorically as follows: 1. A diminution of suffering is a great advantage. The woman fears pain, and hesitates to subject herself to a method entailing much suffering. That method which inflicts the least suffering will hence most likely become popular. In Tripier's method, it is the cutaneous electrode which provokes the greatest pain. This electrode is dispensed with by my method. Further, the truth is that the deeper the electrode is placed within the uterus the less the pain, and the cervix is more sensitive than the fundus. These assertions any one may prove by experiment.

2. If Thèvenot will refer to Tripier's book, he will find that in his method, as well as in mine, the electrode is introduced as deeply as possible into the uterine cavity, and only remains the electricity. The rheophore, therefore, is entirely outside of the uterus and can only be applied to the cervix. Its essential fault, hence, is that all intrauterine action is eliminated, the very object aimed at. It further has the disadvantage of requiring the use of a speculum, not only for its introduction, but especially during faradization, in order to prevent contact of the poles with the vagina. The method is not an active one and is often painful; for, even though the vagina be protected whilst acting on the cervix, we are never sure we are not at the same time acting on one or another cul-de-sac.

within the cervical canal in those cases where it is impossible to pass the internal os. As to the dangers accompanying my method, they are none other than those which accompany the introduction of the uterine sound, and yet this is no argument against the use of the sound, an instrument so useful, and, in certain cases, so necessary to exact diagnosis. Similarly, for the cure of a chronic metritis, for instance, it is necessary the electrode should be introduced into the cavity, and, if this be carefully done, the chance of damage is slight. Of course, the method has at times been unsuccessful, but so has ovariectomy; and yet this is not sufficient reason for damning the operation. It is always well to remember that a method may be excellent, but the operator very bad. As for the fatal result following the application of the method to M. Tarnier's patient, it should be borne in mind that this patient had been subjected to the method several times without evil result; that, on the last occasion, the Cusco speculum could with difficulty be removed, and may have done injury; and finally, that this patient had been for months in the service without an exact diagnosis having been made, and that she may have been suffering from a latent periuterine inflammation which was re-awakened and rendered acute by the manipulation incidental to the specular examination and the application of the current. It is at any rate scarcely fair to throw the whole blame on the method. And finally, as to the danger of causing miscarriage by the application of the method: If the treatment be commenced as soon as possible after a menstrual period, and if the patient be impressed with the necessity of abstaining from intercourse during the treatment, and if we are careful always to eliminate, as far as possible, pregnancy in any case, then there is no more objection to the method on this score than there is to any other uterine application.

3. It is not claimed that the electrical reaction is concentrated within the uterus. On the contrary, the beneficial effect of the disseminated current on the periuterine tissue is recognized and aimed at. And, further still, it is hoped to prove clinically that we can and ought to faradize the uterus, under certain given conditions, even in acute inflammatory periuterine attacks.

And now to answer the objections of M. Onimus.

He claims that the electrode is too large, and yet he could hardly dream of a smaller one, since it is identical in dimensions to every hysterometer used in gynecology. M. Onimus would substitute another, his own, it is true, but for all that larger than mine. Another objection is that the electrode is difficult to introduce. So is the uterine sound; but it is supposed the operator will be familiar with the rudiments of gynecology before he attempts to practise uterine electrotherapy.

A third objection is that the double electrode is bad in principle, namely, both the poles are applied to the uterus. This objection is purely gratuitous. He makes no attempt to prove it.

Finally, I would say to my critics that my method depends for its results on the fact that through it the uterus is caused to contract quickly, safely, and to the greatest possible extent. It is a scientific fact that the uterus, even when empty, is capable of contraction. The faradic current can best cause this contraction. And yet M. Onimus is guilty of the following paradox, which is simply quoted without comment: In its ordinary state, that is to say, when empty, electric currents, as well the continuous as the induced, have a favorable action on the uterus, and this by stopping and by annulling the contraction of its muscular fibres!

And now is my method generally applicable? I believe that it is, if every precaution required by the method be taken.

For instance, we ought, during the entire treatment, to eliminate a possible pregnancy and to forbid all sexual intercourse.

But even pregnancy is not a contra-indication to the use of the method, as I intend to show later; there are even cases where the method is called for. Then our only care should be as to the position of the sound. *It must not be pushed into the uterus beyond the internal os.* It must remain within the cervix and be there held during the entire sitting, fixed by the index finger in the vagina resting on the posterior lip.¹

¹When the hand is not steady enough to use my method during pregnancy, recourse must be had to the *utero-sub-pubic* method of Tripier which is easier of execution. His sound having but one pole need not be pushed as deeply as mine, and it is sufficient to hold it by the index placed at the external os or pushed a trifle within.

In certain cases of *pronounced flexion* where the uterus is much congested and any replacement becomes painful and difficult, it is wise to make no attempt to penetrate, at all hazards, into the uterus. *A general rule, and an absolute one, is to beware of any violence*; faradization, which, after all, is simply a species of *therapeutic hysterometry*, will thus alone bear good fruit.

The sound should always be introduced *without the speculum*, along the palmar surface of the index finger which serves as a guide and rests in the vagina.¹

It should be held lightly, should be pushed in *slowly, very slowly*, WITHOUT ANY EFFORT, *stopping at any obstacle* not easily passed.

Such, after all, is the secret of successful hysterometry.

In this paper, I have purposely avoided reference to Tripiér's other methods of faradization applied to uterine versions and flexions, and known as the *recto-uterine* and *vesico-uterine*.²

My aim has simply been to describe an operative method applicable to the treatment of metritis, and one which is frequently indicated in *obstetrics* after *confinement*.

In a word, I have desired to do better, with *less pain* and *more quickly*.

A CASE OF VICARIOUS MENSTRUATION FROM AN OVARIOTOMY SCAR.

BY

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A CASE has just come under my observation which may have some interest in connection with Lawson Tait's theory of menstruation.

On September 12th, 1876, I removed a monolocular cyst of the right ovary from a girl aged twelve years. She had until then

¹ The disadvantages of the speculum are noted at length in my book on *Technique Gynécologique* (Paris: A. Delahaye et Lecrosnier).

² See my *Technique Gynécologique*.

never menstruated, but began to have her courses in the following winter. In the latter part of September, 1883, I received from her the following letter :

AU SABLE, September 20th, 1883.

DEAR SIR:—I can think of no better way of introducing myself to you than to say that I am the little girl whom you attended at Birmingham in the Centennial year, though of course changed in these seven years to a girl of nineteen. You will remember that a tumor was removed by you. The wound on the stomach seemed entirely recovered until about two years ago, when it became inflamed at times, and finally in the last year assumed a sensitive look. When my courses take place every month it bleeds more or less, and looks frequently as if ready to crack open in one place. Can this be prevented, or is it a natural course? I have laid aside corsets for some time, fearing the steels in front might irritate the inflamed part of the wound. My health seems perfect, so that I am in hopes that it may be something less serious than it seems, or something that can be explained away.

“Yours, etc., _____.”

A subsequent letter read as follows: “I was first unwell, I think, the following winter after the tumor was removed, when about thirteen years old. The wound never troubled me until about two years afterward, my menstruation having become fully established before. At that time the hemorrhage commenced and for a period of two or three months I was unwell every week. At the time I walked a good deal, and fancied that caused it, for on coming home from Ypsilanti, where I had been studying, I soon grew regular in my courses; the hemorrhages continued from that time on monthly, but were never so severe as in the last few months. To sneeze or cough at the time of menstruation causes violent pain in the wound, or around it, and sometimes causes the hemorrhage to be more severe than usual.

“Sincerely yours, etc., _____.”

I wrote to Miss G. to come to the city and have the trouble investigated. She did so, and arrived at my office about the 17th of October.

She was then just recovering from menstruation, and I had an opportunity to see the wound under those circumstances. I may say here that I had used a clamp in operating, and that the wound had healed without any drawback. I found a scar midway between the navel and os pubis about an inch long. At the lower end of the scar was a point about as large as the end of an uncut lead pencil, from which a drop of blood was oozing. This place seemed covered by a small membranous cap of epidermal cells from under which the blood appeared. The lower part of the scar looked red and sensitive, but the upper part was as scars usually are. The patient, however, informed me that the whole scar often grew, at these times, red and inflamed.

On considering the matter, I thought of several hypotheses

by which the hemorrhages might be accounted for. It might be a case,

First. Of simple vicarious menstruation, the general vascular tension finding relief at that point instead of the lungs, stomach, nose, or other organs.

Second. Accepting Lawson Tait's theory of the part played by the Fallopian tubes in causing menstruation, there might have been left through the scar a minute orifice communicating with the remains of the right Fallopian tube, through which the blood oozed.

Third. The scar, though imperforate, might get its nutriment from the ovarian or uterine arteries, in which case it might participate in the general congestion of the parts supplied by those vessels.

Fourth. The traction exerted on the scar by the turgid and heavy uterus at the monthly periods might cause irritation and hemorrhage.

My advice was to have the scar excised and the raw surfaces thoroughly brought together by sutures.

This was accordingly done on October 21st, when I cut out the lower half of the cicatrix to a depth of fully half an inch. The wound was closed by silver sutures, but did not heal by first intention. It was fully a month before the process of healing by granulation was completed. The excised scar was given to Dr. F. W. Brown, who kindly prepared it for microscopical investigation. I desired especially to learn whether there was any trace of mucous membrane or muscular fibre, in short any remains of the Fallopian tube to be found in the specimen.

Dr. Brown informed me that he could, after the most careful investigation, discover nothing but cicatricial tissue. It is now six months since the operation, and notwithstanding the occurrence of regular menstruation there has been no recurrence of hemorrhage from the scar.

The question whether this was an ordinary case of vicarious menstruation is one not easy to answer.

If menstruation is the especial function of the Fallopian tubes, it would seem that menstrual hemorrhages ought to have been observed more frequently from such scars as have been formed by the clamping of a Fallopian tube in an abdominal wound. I have not, however, been able in the literature at my command to find any such instances recorded—perhaps the editor or some of the readers of this JOURNAL may be able to refer me to such records.

AN INSTRUCTIVE CASE OF SEVERE VOMITING OF PREGNANCY.

BY

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I WAS called on the 22d of March to see Mrs. —, a primipara æt. 24, dark hair and eyes, fair complexion, and nervous temperament. Her last period occurred the latter part of January, so that at this time she was about the seventh week of pregnancy. Until now she had not suffered much with discomfort of any kind. But at the time I was applied to, the nausea and vomiting were most distressing, waking her up at 3 A.M. every morning, and it being late in the day before she was even tolerably comfortable; another paroxysm coming on in the evening, and lasting until nearly bedtime. Her tongue and general appearance indicated a torpid condition of the liver, so small doses (gr. $\frac{1}{4}$) of calomel were ordered every hour, she was to take a milk-punch at bedtime, and a cup of strong coffee on awaking in the morning. The calomel produced three good bilious stools. The hips were elevated and she was ordered to remain quietly in bed. Under this treatment she improved very much, but continued to have very distressing nausea in the morning, and her disgust for solid food exceeded anything of the kind I ever saw. She was now put on the much boasted ingluvin, and the result showed its utter worthlessness. On April 5th she had a well-marked chill, followed by fever and a sweating stage, and the paroxysm accompanied by intolerable headache confined to the left fronto-parietal region. The attack lasted about six hours, and on subsiding left her comparatively comfortable. There was a recurrence on the second and third days. The question arose, could it be malarial? But she had not been exposed to malarial influence. Was it due to incipient uremia? Analysis of the urine showed it to be normal. Was it, then, the beginning of septic poisoning due to some morbid process in the womb, the death of the fetus from violent effort in vomiting, or some other cause of like nature? The administration of grs. xvij. of quiniæ sulph. successfully ended this complication. But the morning nausea persisted, and the patient was becoming more prostrated, and her disgust to food increasing daily, obstinate hiccough set up. I had in the early treatment suspected displacement or some lesion of the womb as being an important factor in producing the nausea, but the patient's youth and natural delicacy made an examination per vaginam very repulsive to her. But now things began to look serious, and I began to fear that her life would be the forfeit either from ex-

haustion or hemorrhage from the stomach, unless she had speedy relief. I gave her per rectum 3 i. of potass. bromide without effect, except *that it produced agonizing pain*. On April 13th, I made a vaginal examination, and found the uterus enlarged to the size it should be at the tenth week of pregnancy, *and retroverted so that it lay flat upon the rectum its entire length, and evidently pressing on the sacral nerves*. She had, a day or two previous to this examination, complained of *severe pain in the back*, especially when *she assumed the dorsal decubitus*. Here was an explanation of both the pain and nausea. On the following day, she was placed in the knee-chest position, and the womb easily replaced, and a Hodge's double lever (closed) pessary introduced to keep it in position. She had *no more hiccough*, and *no more nausea* until a week later, when she attempted to take a bath, and becoming very much exhausted, she fainted. She had been able to go about her room since the introduction of the pessary; was obliged now to keep her bed, and on the next morning she had an attack of nausea, but neither obstinate nor violent, followed by a severe nervous headache. On the next day she was able to be out of bed, and steadily improved until May 1st. On the night before, she imprudently had intercourse with her husband, attended with great sexual excitement on her part; the next morning she felt nauseated though she did not vomit, felt badly all day, though being anxious to go to drive in the afternoon she said nothing of her uncomfortable feelings, but went to drive. She became nauseated while driving, fainted, and falling back, the contents of her stomach were ejected either through her nose or retained in her mouth and throat, threatened suffocation. She would undoubtedly have died but that her husband, taking in the situation, changed her position. I saw her at 7.30 P.M. She was then much prostrated, pulse 120 and thready. Stomach rejecting everything; skin cool. Examination showed *the pessary slightly misplaced; womb below its proper position*. Replaced pessary, and gave gtt. i. of tinct. iodine in water about 11 P.M., when nausea ceased, and she has steadily improved until now, May 12th, she is able to be out of bed and about her room. Appetite improving, spirits good, and as she is advancing into the fourth month of pregnancy, I fear no complication from nausea in the future.

Remarks.—The salient points in the above case are, first, the cause of the nausea primarily, namely, *the retroverted womb misplaced by its increased weight in a state of pregnancy*, and secondly, the recurrence of nausea under circumstances which produced nervous exhaustion when the system is enfeebled by conditions which had previously existed. *The possible aggravation of morning sickness by the performance of the marriage relation* makes it a question if it is not a thing to be forbidden in those cases which suffer most from

this symptom of pregnancy, certainly until the uterus has risen out of the pelvis, and especially in those cases which are of a nervous excitable temperament. The prompt relief from the drop dose of iodine is also worthy of note, and for this suggestion I am indebted to Hewitt (Diseases of Women, Vol. I., p. 398). As to the causes of this symptom, we are much in the dark (Hewitt, quoting McClintock, Vol. I., p. 401). The above author offers as an explanation flexion and versions of various kinds and degree, contending that the pinching of the nervous filaments on the concavity, and the stretching of the filaments of nerves on the convexity is the cause of this troublesome symptom. This may be so, though McClintock, Hewitt says, denies his position. Undoubtedly the above case proves that *misplacement, so far as version goes, is a cause*, or one of the causes. Tyler Smith says that pregnancy exerts an almost poisonous influence on some constitutions, and we have all seen vomiting a troublesome symptom where no displacement was to be found. Hewitt quotes many cases in his own practice in which this symptom was caused by displacement, and the causation is certainly sufficiently determined to make it *incumbent upon us to carefully examine* the womb in every case of obstinate vomiting. *Ulceration or erosion* of the cervix, or an *indurated unyielding cervix* may be causes, or the causes may be located in the body of the womb, simply the dilatation of the womb under the growth of the fetus. Or there may be some diseased condition of the chorion, or of the placenta in its early formation. In my experience, a psychological cause may be the one at the root of the matter. I heard a brother M.D. say he had a patient who was violently nauseated even by the rattling of the dinner plates, and I had a case that could not bear the sight of a new spring dress without nausea. To conclude the whole matter, we of course feel no apprehension except in those cases in which life is endangered. Even mild cases may be aggravated to this point. So we cannot be too much on the alert, or too quick to discover the cause of the trouble in any and every case of vomiting in pregnancy, and I would earnestly urge the importance of keeping the patient free from perturbing influences of all kinds. And in those cases where there is even a *tendency* to prostration, stimulation and a generous diet.

FIVE CASES OF DYSTOCIA FROM COILING OF THE CORD
AROUND THE NECK OF THE FETUS.

BY

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New York.

ALTHOUGH the occurrence of dystocia from simple shortness of the cord, as well as from shortening of the same by one or more coils passing around the child's neck, is a well-established fact in obstetrics, it does not seem to be sufficiently dwelt upon by most authors in the text-books, as well as in the journals. Five cases in point which came under my observation during the last two years are, therefore, perhaps of sufficient interest to be recorded. They are well suited to illustrate the most important points relating to diagnosis and treatment of this anomaly, points about which, as some obstetricians¹ declare, we require more definite information than we have heretofore possessed. To this latter proposition, however, I am not inclined to subscribe, since I have found, while reading up the subject a little of late, that Cazeaux, in his admirable Treatise on Midwifery, has laid down clearly and concisely the principles which must guide us in the management of this peculiar complication of labor. To other papers treating on the subject I shall refer further on.

CASE I.—I was called to see E. E. (aged twenty-two years, U.S.) on the 26th of January, 1882; she had been normally delivered, about two years previously, of her first child. She had had pains off and on for about six or seven hours, and when I examined, I found the os sufficiently dilated to rupture the membranes. This I accomplished after being with her for about three-quarters of an hour; the pains then increased in number and in vigor, the posterior part of the left parietal bone presenting. I expected a normal labor that would speedily terminate, and did not examine again for some time. All at once, during the pain, the woman complained of severe pain in the left side of her stomach, which she assured me was most excruciating, and which was unlike anything she had felt either during her first confinement or up to the time she was speaking, during the present one.

¹ Dr. D. Berry Hart, in a note to his abstract of the Transactions of the Obst. Soc. of Edinburgh. AM. JOURN. OF OBSTET., May, 1883, p. 541.

On examination, I found that the head had descended well down into the pelvis, although the occiput seemed to be higher up than it should have been, and the hollow of the sacrum was consequently not filled out as well by the head as is the case under normal conditions. While a pain came on, I felt that the head descended down into the pelvis and with its termination it receded somewhat upwards. This condition of things lasted for nearly an hour, and my efforts to discover the cause of the difficulty by pushing up between occiput and symphysis as well as posteriorly being unavailing, I concluded to apply the forceps, if some further observation did not disclose a cause. During my efforts to gain some definite idea about the real nature of the obstruction, I suddenly conceived the idea that either too short a cord or one shortened by coiling around the child's neck was the cause of the dystocia. For this was the only way in which to account for this peculiar combination of symptoms.

After waiting through several other pains, and seeing the head proceed very slowly, the pain in the woman's left side became so severe that I proposed the use of chloroform, which was readily consented to. At the same time I prepared for the application of forceps, and informed the relatives of the probable cause of the trouble. While I was thus engaged, the head had been descending further, and was very near the vulvar opening. I could see the hair covering the vertex plainly when separating the labia. I applied the forceps and by steady traction lasting several minutes, I delivered the child's head, the nurse at the same time following the descending womb with her hand—*Credé* fashion.

After unlocking and taking off the forceps, I vainly tried to extract the trunk of the child, and had to tie the cord, which was coiled around the child's neck twice, before the delivery could be completed. Child, female. Duration of labor, eleven hours.

The placenta was adherent in the left horn of the uterus, and I had to remove it by introducing the hand, and scraping it off with my fingers.

The woman made a good recovery, and was up on the eighth day.

CASE II.—E. T. (aged eighteen years, U. S.), primipara; Jan. 27th, 1883. In this case, the child, after rupture of the membranes, was found to present in the third position, the anterior portion of the left parietal bone being touched by the examining finger. The pains were very strong and frequent, and it was only after the head had engaged in the superior strait that untoward symptoms occurred. These consisted in the escape of a little blood after each pain and in the inverted type of the movements of the head during each pain: descent during the pain and ascent on its cessation. At the same time, the woman complained of "horrible pains" during every uterine contraction, mostly in the left side. My suspicions were aroused at once that the same trouble was there as I had found in case I., just a year previously. I endeavored to examine per rectum, in order to convince myself

of the truth of my suspicion, but this was indignantly refused, the patient being a very capricious young lady. The same flat refusal was given to my proposal of administering chloroform, because "it is so dangerous," in spite of all the persuasive efforts of her mother and another relative. I therefore desisted, especially since immediate delivery did not seem called for, and encouraged her to bear down well with every pain, while I exerted considerable pressure over the fundus and left side with my hands. The bloody discharge after each pain became less noticeable, and the head proceeded well downward towards the inferior strait, until it reached the perineum. Now, the resiliency of the latter structure, in strong, healthy primiparæ of that youthful age, is well known to every accoucheur; it may be compared to that of the strongest and most elastic India-rubber. It added another, though natural and unavoidable source of impediment to the complicated labor. I therefore communicated to the woman's mother my intention to apply the forceps under chloroform, after I had watched the progress of the case for a quarter of an hour. But the patient had observed us, and although she could not hear what was said, she imagined it and went into hysterics, crying and begging piteously, promising to do her best, etc. Severe and almost incessant pains now set in, and I hastened to the bedside. Placing both my hands on the fundus, and exerting firm pressure (for the perineum was now sufficiently stretched), I exhorted her to be quiet and continue her efforts, and after another half-hour the head of the child was slowly expelled, face upwards. The same difficulty obtained as in the first case; the trunk could not be delivered, and since the child's head was of a dark purplish hue, I quickly tied the cord, which was also coiled twice around its neck, and cut it. The placenta was expressed by Credé's plan, after the delivery of the (female) child. Duration of labor, eight hours.

On the third day after delivery, a chill set in, and the woman complained of severe pain in the left side. She had a temperature of 104° and suffered very much. A parametritis soon developed which kept her in bed until the 2d of March (thirty-third day). All vestiges of the exudation were gone by the 27th (fifty-eighth day), and she has since then been in perfect health.

CASE III.—L. G. (aged thirty years, U. S.), primipara. Labor commenced on the morning of June 11th, 1883. I saw the case several times in the course of the day, and it was not until about 9 P.M. that the os was sufficiently dilated to allow of my rupturing the membranes. After this was accomplished, the pains, which had been few and far between, increased in power and frequency to such an extent that I administered chloroform, at the urgent request of the patient. The position of the child was the second (vertex), the posterior part of the left parietal bone presenting. Very soon the same conditions obtained that I had observed in the first case described in this paper, viz.: incomplete flexion of the head, the occiput standing comparatively higher than is to be expected when the head has entered the true pelvis; the anterior portion

of the head not completely filling the hollow of the sacrum posteriorly; descent of the head with each pain, and ascent of the same during the intervals. In this case I also observed for the first time a state of things which seems to me to be of some importance in connection with our subject. Because I administered chloroform, I examined much more frequently than I am wont to do since the great puerperal-fever scare¹ set in, some years ago. I found that almost at each examination there was a slightly changed position of the head; while at one time the sagittal suture was felt to occupy a position very near the axis of the pelvis, the next time the obliquity of the head would have considerably increased, and the sagittal suture would thereby be felt somewhat more to the woman's right side. In the same way, flexion would sometimes exist to a greater and then to a lesser degree, the lesser fontanelle being in the latter case nearer to the pubic arch than in the former. These signs of abnormal mobility of the head about its lateral and antero-posterior axes became less marked as the head descended, and at last were imperceptible, because the head appeared to be fixed and after each pain seemed to retreat to its former position, although situated very near to the outlet of the pelvis. The perineum was moderately rigid, considering the age of the patient, and, after observing the condition just described and being firmly convinced of the correctness of my diagnosis of coiled cord, I applied the forceps. After ten minutes' steady traction, while the nurse was firmly compressing the womb with both hands, I delivered the child's head and found that the cord was wound twice around the child's neck. Again delivery of the trunk was impossible, in spite of the strong pains, and I was forced to tie the cord before it was accomplished. Then a healthy (female) child, of average size, was speedily delivered. Duration of labor, eighteen hours. There was a slight laceration of the perineum; but the woman was able to be up on the 22d of June (tenth day), and the posterior vaginal wall and commissure were completely healed by July 10th (twenty-eighth day).

CASE IV.—S. C., aged twenty-six years, Germany; two children, last two years previously.

I saw the woman first early on the morning of January 23d, 1884. She had quite severe pains since the 21st at intervals of from one to several hours. Since the previous evening they had become more steady and regular, although not frequent enough to necessitate my being called in. I found the os sufficiently dilated and the bag of waters protruding far enough to warrant its rupture. I decided to await the occurrence of a few more pains before proceeding, when the break occurred spontaneously. The head presented in the first position and all went on smoothly until it had descended somewhat into the pelvis. Then there

¹ I may perhaps take occasion to state that the force of this disease was broken—at least with myself—after we passed through the last stage of hyperpyrexia.

was the same train of symptoms that has been described heretofore. The slight changes of position after each pain; descent of the head with the pain and ascent during the interval, and most prominent of all, a cutting and tearing pain of great intensity in the abdomen, a little to the left of the median line. This pain the lady assured me was totally different from the sensation caused by actual labor pains, "and," added she with grim humor, "I ought to know, for I am an old hand at it!" I proposed the use of chloroform to which she gladly consented.

I tried to convince myself of the correctness of my diagnosis of coiled cord by examining through the rectum, but was not able to reach up sufficiently far to do so. Nor was I more successful by insinuating my fingers between the occiput and the pubis, although I could feel the nape of the neck distinctly as I thought. I confess I was somewhat puzzled; the more so since I had given my diagnosis to the nurse in the most positive manner. For, if it would have been a hand lying by the side of the head, I should have felt it while examining per rectum, the chances being that it was the left arm that had slipped down.

Nevertheless, after observing the further progress of the case, I fell back on my former convictions. The patient being once more put under the full influence of chloroform,¹ I applied the forceps while the nurse had instruction to press on the fundus with both hands. After considerable traction I delivered the head; again, on trying to extract the body, I was baffled. The child's head was of a very dark purplish color and the cord was found coiled twice around its neck, pretty far towards the body, just above the sterno-clavicular articulations. (This furnished the explanation why my examination failed to reveal the coils.)

I again ligated the cord *in situ*, and was then able to deliver a healthy (male) child. The difference in color between the head, which, as I stated, was purple, and the body and limbs, which were only slightly livid, was striking, so much so that the nurse spoke of *strangulation*. I shall speak about this further on.

The placenta was found to protrude partially from the internal os which was firmly contracted; I introduced my left hand into the vagina and up the external os. After some time, I succeeded in insinuating my hand between placenta and internal os. At last my hand had entered the uterine cavity; I found the placenta to be partially attached to the anterior wall of the womb, somewhat to the left of the median line, and succeeded in removing it. Duration of labor, forty-two hours. Length of cord, twenty-nine and a half inches.

CASE V.—M. K. (aged eighteen years, U. S., primipara). I was called on January 26th last, by a midwife who, immediately upon my arrival, informed me that she believed the cord was too

¹ It has been my experience that women only need a kind of semi-narcosis during natural labor, which may be kept up without doing any harm for many hours; before operating, however, full narcosis should always be obtained.

short, very probably by coiling. I asked her what made her think so, and she replied that it could not well be anything else, since the head was not very large, the woman broadly built, and although the head had been at the outlet of the pelvis for nearly an hour, and the perineum was sufficiently stretched, it would not pass the vulva. I found the conditions exactly as she had stated them, and when 'taking a pain,' felt the head (in the first position) descending, while it receded immediately after the pain was over. I asked whether the woman had complained of a localized pain in a particular spot, but the midwife had paid no attention to this, and the woman was in such distressing agony from the almost incessant pains that I did not question her. I applied the forceps and extracted the head; the trunk did not follow because the cord was coiled around the neck by one turn. While I put my finger under the lower shoulder in order to help it along, the midwife, in spite of my protests, slipped the cord down over the shoulders and the body was expelled. The child was a healthy female, somewhat livid, but not much so. The placenta, the edge of which was visible between the labia, was easily expressed by Credé's manipulation, combined with moderate traction on the cord; the laceration of the perineum was too trifling to call for any interference. Duration of labor: twenty-two hours. Length of cord, sixteen and three-quarter inches.

I have given the foregoing histories somewhat *in extenso* because I think that the subject justifies such detailed descriptions; while it enables the reader to draw his own conclusions from the conditions described, it furnishes a modest contribution to the "more definite information" which Dr. Berry Hart¹ declares is required, "before one would venture on the diagnosis and treatment of such cases." Spiegelberg is equally sceptical in this respect; he thinks that all the symptoms given as justifying a diagnosis of short or accidentally shortened cord "are wholly unreliable and may be referred to various other and more plausible causes." He thinks that "we can only be certain about this when we are able to feel the cord, and thus convince ourselves of its abnormal tension, *i. e.*, after the head is born when it is coiled around the neck. . . . Occasionally the coiling around the neck may be recognized from the rectum before the delivery of the head."² The latter mode of exploration has not been successful with me when I attempted to employ it as stated above, while I may safely contend that the symptoms mentioned are of great value. They have been first

¹ L. c.

² Lehrbuch der Geburtshülfe, p. 551.

exhaustively treated of by Devilliers¹; Cazeaux, who follows D. closely, confirms all his statements and makes valuable additions from his own experience. The latest contribution to the subject is a paper read by Dr. A. D. L. Napier at the meeting of the Obstetrical Society of Edinburgh on January 10th, 1883.² The report, as given by Dr. D. Berry Hart, is the following :

1. Retraction of the fetal head during the pains, with sufficient uterine action and a roomy pelvis.
2. Gradual cessation or long abeyance of the pains; and
3. Insufficient head-flexion, followed by over-rotation of the occiput; all pointed to coiling of the cord round the child's neck. The second factor above stated he believed to be the most important.

For diagnosis of this condition when the head was in the pelvis, he held that it could be made out by digital examination when the pelvis was roomy. If he suspected its occurrence in the first stage, he would employ sedatives; in the second stage, he would hasten the labor in the usual way or cut the cord with a pair of probe-pointed scissors and then employ forceps.

It is evident that there is some mistake in the statement of the first point; it should be just the reverse; for the sentence as it stands applies to the normal conditions of labor. The head in normal labor, it is almost superfluous to mention, is retracted somewhat during a pain while it descends on its cessation by the force of gravity. Aside from the observations recorded in the foregoing histories, this point is explicitly dwelled upon by Spiegelberg and by Cazeaux. In the passage, partially quoted above, the former mentions as one of the symptoms given: "The advance of the child with every pain, and its retraction when the pain ceases, provided that neither the width of the outlet of the pelvis nor the rigidity of the perineum are at fault."

Cazeaux³ says: ". . . When a very short cord is forcibly stretched between the placenta and some part of the child's body, a particular phenomenon can be recognized by the touch; that is, the finger, when applied on the head, finds it advancing during the pain, and retreating as soon as it is over, because at

¹ Recueil de Mém. et d'Observ. sur les accouchements, Paris, 1862.

² AM. JOURN. OF OBST., May, 1883, p. 541.

³ A Theoretical and Practical Treatise on Midwifery; 5th Am. Edition, p. 835.

this moment the fundus of the womb, which had been depressed by the contraction, regains its primitive position and draws after it the placenta, cord, and fetus. *But this sign will evidently be wanting where the after-birth is attached to the lateral parts of the uterus.*" (The italics are mine.)

This symptom was present in every one of the five cases here related, and it is certainly hard to imagine why Spiegelberg considers it a "wholly unreliable sign and one that may be referred to various other causes." There could be only two other conditions able to accomplish such a result; one is given by him, viz., rigid perineum, the other has been alluded to before in connection with Case IV., viz., a hand lying by the side of the head, more especially Simpson's' "accidental dorsal displacement of the arm." I may say in this connection that I was deceived by this symptom myself in a case of this rare complication which I attended only last month (February 15th). Here also there was this propulsion of the child with the pain and retraction after it; but by pushing up between pubes and occiput, I was enabled to feel the arm and arrive at a correct diagnosis. Instead of short or shortened cord, it was the child's left arm thrown around the back of the neck which, resting against the pubes, acted as an obstruction in the same manner as does the short cord, more especially that shortened accidentally by coiling.

And this brings us to a statement by Cazeaux which I took the liberty to put in italics, because I think it is disproved by those of my cases (2) in which I was able to locate exactly the attachment of the placenta. The great Frenchman thinks that the only manner in which the symptom under consideration can be produced is a depression of the fundus by the traction exerted on it through the cord. Therefore he believes it impossible that the symptom is present where there is a more or less lateral attachment of the after-birth. Now, in Case I., the placenta was found to be attached laterally in the left horn of the uterus, and in Case IV. it was attached to the anterior wall, and even in Case II. there is a strong presumption that it was attached laterally and anteriorly, and not to the fundus. How can we account for it in the

¹ Sel. Obst. and Gynec. Works of S., Ed. by J. W. Black; Appleton, N. Y., 1871, vol. I., p. 381.

light of Cazeaux's statement which tends to contradict us? I think that one factor must be taken into consideration which is of considerable importance in this connection, *i. e.*, the stretching of the cord. Of this it is very easy to convince ourselves by grasping a newly-born placenta with one hand and pulling at the end of the cord with the other. I have tried this repeatedly and have always been able to lengthen a cord of average length—twenty-odd inches—by from one and a half to two inches. This, of course, is due to the uncoiling of the convolutions, and almost complete restitution takes place after the traction on the cord is stopped. This resiliency of the cord is aided by the slight abnormalities in position (sub-flexion and super-rotation) which in turn are rendered variable, as I have pointed out in the histories, by the retraction which the elasticity of the cord causes.

The "*gradual cessation or long abeyance of the pains*" which Dr. Napier mentions as the most important symptom I have not observed in my cases; on the contrary, the pains in all the cases were as strong and as frequent as one could wish for.

To the third point: "*Insufficient head flexion, followed by over-rotation of the occiput,*" I can give fullest assent, at the same time adding *the peculiar variability in the position of the head within narrow limits.*

A fifth point must be made of the *distressing pain at the point of attachment of the placenta* which was unmistakably present in the first four cases.

A sixth point (which was observed only in case II.) is *the discharge of a little blood immediately after each pain*, a sign mentioned by Spiegelberg.

The latter symptom may sometimes assume most threatening proportions. This is illustrated by the history related by Dr. Barker in the meeting of the Obstetrical Society of New York, on April 18th, 1882,¹ while discussing a case described by Dr. Lusk. The latter gentleman had seen a case of dystocia from coiled cord which ended fatally, because the delivery had been delayed too long (five days) and the external genitals became gangrenous.

Dr. Barker's patient had been confined seven or eight times previously by eminent accoucheurs, and the child had been

¹ Suppl. to AM. JOURN. OF OBST., Nov., 1882, pp. 324-326.

born dead at full term every time. He learned that there had been invariably profuse hemorrhages, but could not guess from the history why the children had always been born dead. When labor came on, he found no disproportion between the size of the fetal head and that of the pelvis. . . . When the head distended the vulva, there were severe pains for an hour,¹ without the slightest progress being made. Chloroform was given and the forceps applied; but just before extracting he withdrew the instrument, passed up his finger and found the cord wound three times around the neck, and very tense. He divided the cord and extracted the child alive. It was perfectly evident to him that the short cord had retarded labor; and had he proceeded with delivery by the forceps, the placenta must have come away, and, in all probability, the child's life, and perhaps the mother's, would have been destroyed. The cord was eleven inches long, or but two after allowing three circles round the neck.

Dr. Barker is convinced that coiling of the cord is oftener a cause of difficult delivery or retarded labor, of death of the child, and of violent hemorrhage before the expulsion of the placenta than is commonly supposed. He makes it a point to pass a finger up every time, and if he can feel the cord encircling the neck, cuts it, if necessary, before delivering.

It is interesting to contrast this positive and unequivocal statement of Dr. Barker's with the view advanced by Spiegelberg as to the frequency of this condition. He says²:

" . . . And yet, strong dragging and causing considerable impediment to labor, as well as premature detachment of the placenta, and hemorrhage, *is certainly extremely rare.*"

There is, however, another point in the history of Dr. Barker's case which it is necessary to dwell upon, as it would seem to have some bearing on our subject. He gives the length of the cord as eleven inches and calculates nine inches for three circles around the neck, which would make the circumference three inches. I have measured since last year the circumference of the neck in every child that I delivered, and have found that the lowest figure was six inches and the highest a

¹ This would tend to confirm my observations as to "the gradual cessation or long abeyance of the pains" so prominently mentioned by Dr. Napier.

² L. c., p. 550.

little over eight inches. I have at the same time measured the distance from the umbilicus to the side of the neck, and have found that it averages seven inches and a half, the lowest figure being six and a half and the highest a little over eight and a half inches. Taking the lowest figures found, a threefold circle—which in reality requires, in face presentations, only two and a half circumferences—would give: 15 inches

Length from umbilicus to side of neck, $6\frac{1}{2}$ “

$21\frac{1}{2}$ inches

The remaining portion of the cord from the neck to the placenta is not taken in account, and be it ever so short, it would still add several inches to the calculated figure. I think that even the tightest constriction around the neck, coupled with the strongest traction short of rupture of the cord, would be unable to reduce the number of inches by one-half, *i. e.*, to eleven inches. As to my own cases:

For case IV. we get the following figures:

From umbilicus to neck, 8 inches

Two full coils, 15 “

Together, 23 inches

which leaves for the length of cord from neck to placenta: $29\frac{1}{2} - 23 = 6\frac{1}{2}$ inches. This would about correspond to the distance from the vulva to the placenta which was situated anteriorly to the left of the median line.

For case V. the figures are:

From umbilicus to neck, $6\frac{1}{2}$ inches

One full circle, 6 “

Together, $12\frac{1}{2}$ inches

which would leave for the length of cord from neck to placenta: $16\frac{3}{4} - 12\frac{1}{2} = 4\frac{1}{4}$ inches. Mother and child in case V. were both much smaller than in case IV., sufficiently so to account for the differences given in the measurements which were actually taken as far as the children are concerned.

I regret not having been as particular in the first three cases.

The shortest cord ever measured was two inches long, and it was torn just before the expulsion of the child;¹ but, of course, in this case the cord went directly from the umbilicus to the

¹ Cazeaux p. 837.

placenta. The shortest cord I have had occasion to measure was ten inches, and it gave comparatively little inconvenience during confinement. But this may have been due to the fact that the child was very small and the woman broadly built. About twenty minutes after the discharge of the amniotic fluid, the head of the child, which had descended very quickly, was suddenly expelled, the trunk followed, and the placenta was found lying in the vagina, a portion even protruding from the vulva. There was no abnormal hemorrhage.

The lividity of the child, especially of its head, was most marked in case IV. The difference in color between the head, which was purple, and the body and extremities, which were somewhat livid, was very marked, showing that the impeded reflux of venous blood from the head, and not general asphyxia, was the cause of the condition. There could have been no strangulation, because there had been no attempts at respiration, and the lividity of body and limbs was simply due to some slight obstruction in the cord, produced by the tense state of the latter—from traction or compression, or both. The spiral turns in the cord certainly tend to protect the child's life in such cases, as they enable the blood-vessels to stretch sufficiently when the cord is subjected to dragging. It would, therefore, be an unjustifiable procedure to pull at the coils around the neck, in order to protect the child from 'strangulation,' as the officious nurse in case IV. volunteered to do; almost as bad as the practice of the midwife in case V. who stretched the cord to let the child's body pass through the coil. Happily enough, she did not produce umbilical hernia, as has happened not infrequently from such attempts.¹

To recapitulate results as to diagnosis and treatment, the following must be stated:

Diagnosis.

1. Descent of the head during the pains and retraction during the intervals.
2. Insufficient head flexion and over-rotation of the occiput.
3. Variability of the position of the head within narrow limits.

¹ Lehrbuch d. Geburtskunde für Hebammen in den K. preussischen Staaten, p. 286.

4. Distressing pain at the seat of the placenta.

5. Discharge of some blood immediately after each pain.¹

If rigid perineum, dorsal displacement of an arm, or head and arm presentation can be excluded, the first symptom, especially when combined with some or all others given, point to true or accidental shortening of the cord.

Treatment.

1. Anesthesiation of the patient.

2. Extraction of the head by the forceps and division of the cord to allow the delivery of the body. Or, in extreme cases,

3. Division of the cord within the vagina, followed by the application of the forceps.

The early division of the cord may be urgently required to save the life of the child, as is illustrated by Dr. Lusk's case and by a case cited by Cazeaux² where it was delayed two hours after the birth of the head. It also militates against two other formidable accidents, viz.: inversion of the uterus and flooding.

A CASE OF EXTRAPERITONEAL RUPTURE OF THE PARTURIENT UTERUS.

BY

G. E. GOODFELLOW, M.D.,
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MRS. C., aged 39, was taken in labor with her tenth child on Nov. 30th, 1883, at 8 o'clock P.M.

I saw her first about one hour later. Examination disclosed breech presentation with feet down close to the buttocks, back to the R. acetabulum. Temp. not taken, pulse 100 to 110 and small. The pains were regular between five and ten minutes apart, but, as the patient described them, of a peculiar lancinating character.

Her sufferings were apparently so great that the use of chloro-

¹ Cazeaux (p. 836) mentions another sign dwelt upon by Naegele, Sr., *i. e.*, a bellows murmur with the fetal pulsations which he assumes to be caused by the circular turns of the cord around some part of the fetus. But he thinks that "further research is required to establish the absolute value of this new means of diagnosis."

² L. c., p. 836.

form was commenced and continued until after delivery, which was accomplished with the blunt hook early on Dec. 1st.

There was no hemorrhage—the placenta following the head immediately. She rested well until early on Dec. 2d, when symptoms of peritonitis supervened and death ensued on the morning of the 3d.

Post-mortem showed a transverse rupture of the cervical portion of the uterus on the right side, above the vagino-uterine junction, about two inches in length and a large hemorrhage in the cellular tissue of that side extending into the iliac region beneath the peritoneum which only covered the upper half of the uterus.

There was no blood in the abdominal cavity and the peritoneum was not torn. Near the fundus the uterus was thick and firm, but in the cervical region and near the os internum the thickness was from one-quarter to one-half that above, and the tissues were softened.

The noteworthy points of the case were the extreme rapidity and smallness of the pulse, 100 to 120 and more, from almost the beginning of labor; the apparent serenity, and towards the last the length of the pains with their regularity and inefficiency.

The child was a female weighing over ten pounds. The position of the feet was, as stated, close to the nates, but not crossed, and the right foot was in a state of extreme dorsal flexion which might possibly account for the delay in the labor independently of the rupture.

During the night there was vomiting of a greenish-yellow fluid, particularly worse about the time of delivery.

The most difficult point to determine is the period of time at which the rupture occurred.

From a consideration of the symptoms, I conclude that it happened or began shortly after the commencement of the labor, for then the pulse, rapid and small as it was, increased its rate, and the suffering from the uterine contractions was referred more to the right side. That it did not happen during delivery I am positive, though considerable force was used, still not as much as in many other preceding cases. The rapidity and character of the pulse, the intense and inefficient pains with the vomiting during the course of the labor, makes the diagnosis of rupture in the early stages certain.

I would add that while these symptoms were noted and commented upon, only a suspicion of the possibility of such an accident existed until after the post-mortem.

When the hook was applied the child was dead, and the breech just distending the vulvar opening, where it had been wedged for about three hours. No ergot was given until after the delivery.

INTERSTITIAL UTERINE MYO-FIBROMA SPONTANEOUSLY
CURED BY SLOUGHING.

BY
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Philadelphia, Pa.

MRS. MARY SMITH, aged forty-five and one-half years, a very robust Irishwoman, is the mother of thirteen children. In May, 1880, she noticed a mass, about the size of the fetal head, in the region of the womb. She now flooded for two months. Severe, constant, burning pain was felt in the mass, from this time, lasting throughout the entire summer of every year, but disappearing as cold weather approached.

February 28th, 1883, a chill occurred, with fever, severe pain in the tumor, and hemorrhage from the vagina. A careful examination was made, with the following result:

The lower part of the abdominal cavity was occupied by a hard irregular mass, extending to the umbilicus and into both inguinal regions. The tumor measured, superficially, nine inches vertically by eight and one-half inches from side to side. It was continuous with the anterior wall of the uterus, but did not project into the uterine cavity. The sound entered the uterus ten inches.

Hemorrhage persisted until April 28th, when the discharge became watery and extremely fetid. The patient now took to her bed. In one month, sloughing tissue was noticed in the discharge, and this continued to pass, almost daily, for five weeks; on several occasions sloughs half the size of the hand being seen. During this time pain was very severe, and fever, sweating, and emaciation were noted (sapremia).

On the cessation of sloughing, the patient improved very rapidly. At the present date (April 16th, 1884) she enjoys perfect health; no traces of the tumor can be detected, and the sound enters the uterus only four inches. Menstruation is normal.

The treatment (which, however, seemed to produce little good effect) consisted of carbolized vaginal and intrauterine injections, the mechanical removal of sloughs, whenever possible; together with the exhibition of morphia and tonics. An extremely unpleasant incident was the occurrence of a prolonged attack of syncope during one occasion when I was injecting carbolized water into the uterus. It may not be out of place to mention that fainting during the injection of fluid into the womb is always (or almost always) preceded by tinnitus aurium. The physician can therefore avoid this accident by discontinuing the injection as soon as the "noise in the ears" is noticed.

The removal of the tumor by the vagina was not feasible, as

the finger inserted into the uterine cavity could detect no projecting portion; and hysterotomy was not advised on account of the high mortality of the operation, especially in this country. Moreover, the surroundings of the patient were such as to forbid an operation at her home, and she sturdily refused to be removed to a hospital. Therefore, with a full appreciation of the risks incurred, the case was, virtually, left to nature.

280 EAST CUMBERLAND ST.

ENUCLEATION OF LARGE SUBMUCOUS FIBROID OF UTERUS PER VAGINAM.

BY

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I WAS first called to see the patient from whom this growth was removed in the summer of 1881. She presented the appearance usually associated with malignant disease. The following is the history elicited:

She has been married twenty-eight years; never been pregnant; enjoyed good health until about eight years ago, when she was taken with a flooding. Her menses since then have been regular but profuse; she attributes this fact to her almost constant working of a sewing machine for a livelihood; appetite is poor; bowels regular; has lost flesh.

Upon examination, the uterus was found to be symmetrically enlarged and filled up entirely the pelvic cavity. The external os was patulous, through which the tip of the index finger could be passed. A semi-elastic body was felt which, owing to its resiliency and the peculiar history of the case, was supposed to be a sarcomatous tumor of the uterus. She has continued under observation to the present time, and various methods have been used to alleviate her suffering, such as the hypodermic injections of ergotin, the muriate of ammonia, gr. x., t. d.; and the oil of erigeron, which latter agent I have found most efficacious in controlling hemorrhage. Her condition has steadily grown worse and her last menstruation proved almost fatal from the profuse hemorrhage which occurred, and its long duration—two weeks. She was during this period at times pulseless, and resort to hypodermic injections of whiskey and rectal alimentation was the only means of tiding her over. Upon examination it was found that os uteri had dilated to the size of a silver half-dollar. When she had sufficiently rallied I determined to remove the growth if possible, as the only means of saving her life.

Accordingly, Saturday, May 10th, at 2 P.M., the patient being

fully etherized, I proceeded to dilate the cervix by means of the finger. This was accomplished without much difficulty, and the growth encapsulated as far as its attachment to the fundus uteri. I then applied Thomas' straight forceps to the growth, and having made sufficient traction to bring it to the ostium vaginae, I severed, means of Yarrow's scoop, the pedicle, which was about two inches wide. Little hemorrhage followed the operation. I then washed out the uterine cavity with Churchill's tincture of iodine, carbolized $\frac{3}{4}$ ii. to two quarts of water at a temperature of 110° Fahrenheit. The injection was continued until the water returned clear from the uterine cavity. The patient soon came from under the influence of the anesthetic, was put to bed, and given suppositories of morphine to relieve pain. Temperature half an hour afterwards, 98.6° , pulse 108.

May 11th, A.M. Very comfortable all night and slept well; temperature 98.4° , pulse 88.

P.M. Temperature 99° , pulse 95. Not much appetite, but is doing well; no pain.

May 12th, A.M. Temperature 98.4° , pulse 80. Slept all night and appetite good.

P.M. Temperature 98.4° , pulse 85. Feels better than she has for months.

May 13th. Morning and evening temperature normal; appetite good; wants to sit up.

The following is the macroscopic and microscopic examination of the tumor, made by my assistant, Dr. H. L. E. Johnson:

The tumor irregularly spheroidal, somewhat nodulated and fissured, as if composed of several smaller growths agglutinated, or different degrees of development in the various aspects; not very hard, but exceedingly tough: of very pale white color; weight eight oz.; measures three and a half inches in its long and two and three-quarter inches in its short diameter; large circumference ten and a half inches; short circumference nine inches; pedicle measures one and a half inches in diameter.

The microscope shows the presence of a large number of unstriated or spindle-cells, which, treated with acetic acid, bring out more prominently long rod-shaped nuclei.

The cells take an irregular course through the mass, in some places are in a state of fatty degeneration, are bound together by connective tissue, in some parts so abundant and hypertrophied as to present the character of fibrous tissue. The growth is not very vascular, only here and there small arterial *twigs* in the *interspaces*.

May 22d. Patient is up and walking about her room, and improving in strength every day.

REPORT ON THE PROGRESS OF OBSTETRICS AND GYNECOLOGY IN GERMANY.

BY

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CONTINUED attention is still devoted to the question whether *corrosive sublimate*, as hitherto employed and largely recommended, is quite harmless in its application to *obstetrics*, or under what conditions the danger in its use may possibly be increased. Owing to the manifold advantages in the employment of the sublimate over that of carbolic acid, the question is indeed of the greatest practical importance. In my last report I mentioned an indubitable case of sublimate poisoning at our clinic and a similar one by Stadtfeldt, which latter has since then been patho-anatomically confirmed in a communication by Dahl (Copenhagen). Another case of this class, fortunately terminating favorably, has since been reported by Stenger, of Mannheim; and I am able at present to give the histories of two additional cases from our clinic which may perhaps throw some light on the circumstances under which the danger of this agent is augmented.

The first case was that of a primipara, æt. 25, who had been long in labor and showed some symptoms of fever during the delivery. The soft parts were very unyielding, requiring some slight incisions, and the patient was delivered with forceps. There being some atony, hot injections of 1:1000 sublimate solution were given.

During the first days of the puerperium, the patient presented some very peculiar symptoms: general depression, then a comatose state, together with a certain hyperesthesia of the whole body, subnormal temperature, and offensive diarrhea. About the fourth or fifth day these symptoms abated; the patient had some puerperal abscesses for which she continued under treatment for some time at her house. Albumen was present in the urine a considerable time, from the first day on.

The second case was that of an eclamptic primipara, æt. 25; the eclampsia on the whole ran a benign course; the patient was delivered by forceps without material difficulty. After delivery, there was some degree of uterine atony, for which was given a hot irrigation with about three litres of a 1:1000 sublimate solu-

tion. The hemorrhage ceased, and the patient soon recovered consciousness. On the days succeeding delivery, there appeared, besides general depression, great hyperesthesia, subnormal temperature, profuse fetid diarrhea. The patient became somewhat somnolent, and death ensued on the fourth day. The autopsy again showed the most extraordinary alterations in the intestinal mucosa which was enormously swollen and partly gangrenous as far as the transition into the ileum, but especially so in the rectum. The kidneys showed marked changes, although calcareous deposits, as in Stadtfeldt's case, could not be found.

If we ask ourselves the question why, with conditions otherwise nearly alike, in the one case the intoxication was overcome, and not in the other, it seems that in the latter case the pre-existing kidney disease had materially affected the condition for the worse. Owing to the intense influence on the kidneys in poisoning by bichloride of mercury, it appears to be exceedingly dangerous to employ corrosive sublimate in patients whose kidneys are not absolutely sound. At any rate, in Schroeder's clinic the use of the sublimate has been strictly eschewed in cases of such complication.

These cases were also of great value in determining the clinical symptoms: aside from great general prostration, reduction of the cutaneous temperature, hyperesthesia, general restlessness, and profuse, partly bloody diarrhea. Certainly, these observations admonish us to be doubly careful.

Favorable results continue to be reported by writers from all quarters regarding *Crede's method for preventing ophthalmia neonatorum*. Zweifel (Erlangen), Krukenberg (Bonn), and Schatz (Rostock) witnessed the best results at the various clinics after employing instillations of two-per-cent solution of silver nitrate. Of course, most authors are also agreed that, for various reasons, the general introduction of the method is not to be recommended, especially not into the practice of midwives, nor would its general employment be necessary.

In connection with the case of *tubo-ovarian pregnancy* mentioned in my last report as having been observed at our clinic, I should like to refer to an analogous one seen by Vulliet, of Geneva, the conditions of which were quite similar. The patient presented a clear history of pregnancy, and required operation on account of very grave symptoms. The patient died soon after the operation; the specimen, after a most careful examination, showed unquestionably the presence of a tubo-ovarian tumor in which an ovum had developed. Although it was not possible in this case to demonstrate follicles in the wall of the ovisac, still there can be no

doubt, according to the anatomical relations, that the condition was the one stated.

I am enabled to report two additional very interesting observations in the field of *extrauterine pregnancy*. The one was made by Martin, who performed laparotomy in a diagnostically very obscure case of tumor in the lesser pelvis. When the firmly adherent tumor had been enucleated, it appeared as a rather massive firm body, on the external surface of which—that is, originally projecting free into the peritoneal cavity—lay the skeleton of a fetus of about three months' development. All the soft parts, excepting the peritoneal sac and that of the dura mater, had been absorbed so that nothing remained but the superficial, partly embedded bony framework. The whole presented a most peculiar aspect and vividly called to mind the fossil skeletons from the tertiary period as exhibited in museums.

The second observation was made at our clinic, to which the patient came with pronounced symptoms of ileus which had been present for some days and were traceable to a quite definite cause. The history pointed with great probability to an extrauterine pregnancy recovered from two and a half years before, although neither the patient nor the attending physician laid any particular stress on this point. It was possible to feel in the abdomen, which was greatly distended and tympanitic, at about the level of the umbilicus, a slightly movable tumor surrounded by tympanitic resonance, and having a peculiar consistence. Inasmuch as the entire condition was evidently growing worse, Prof. Schroeder decided to perform laparotomy, when it was found that the above-mentioned tumor was a full-grown fetus, lying perfectly free in the abdominal cavity in a natural attitude, and absolutely well preserved. The fetus was in part covered on its anterior surface by the omentum from which vessels extended into it. The entire fetus was as if wrapped in a delicate membrane; the oral, nasal, and optic foramina being completely covered, and fingers and toes as if glued together. After the omentum had been ligated, the ovum could be readily removed; no trace could be found of the funis. On further examination to discover the real cause of the ileus and the location of the placenta, a tumor the size of a fist was found on the left side of the uterus; it was hard and firm, and from it extended a much twisted and tense cord to a part of the mesentery of the ileum. The occlusion was due to torsion of this cord. The tumor was detached from the uterus with considerable difficulty. Owing to the advanced general peritonitis and great disintegration, the operation did not save the patient, who died of exhaustion after twenty hours. The original site of

the pregnancy could not be accurately determined from the specimen. At all events, these two cases offer very interesting proof as to the possibility of variable termination of an extrauterine pregnancy.

An interesting *complication of labor* came likewise under observation in a patient in whom the pelvic inlet was completely occluded by a *myoma* of the anterior wall, larger than a child's head. In the further course of the labor the tumor receded far enough to permit of the easy delivery past it of the child which was dead, owing to prolapse of the cord. The mucosa having been lacerated over the myoma, the attempt was made to enucleate it, which succeeded with great facility. But all the more difficult was the removal: forceps and cephalotribe were tried in vain, until finally, after fixation with large Muzeux's forceps, conjoined with strong pressure from without, the tumor was crowded through the pelvis. After the delivery, however, there was found communicating with the cavity of the myoma a rupture of the cervix to which the patient succumbed. According to our opinion, the lesion was due to the forcible attempts to express the myoma through the pelvis and, in this respect, has been a warning to us to be careful with this method.

In the field of *gynecology*, it is mainly technical questions relating to operations which keep the general interest engaged. The persistent demand for an absorbable and still as strong as possible suture material, especially for all the plastic operations, seems about to be supplied. The very favorable results obtained by me *with dropped catgut sutures* in an extensive series of cases (34), in the most formidable plastic operations on the vagina and perineum (comp. *Berl. klin. Wochenschr.*, 1884, No. 1), determined our institution to resort very largely to the simplified mode of suturing, employed especially by Schede, of Hamburg, with the *running suture* and the particularly well-prepared *catgut*. According to the proposition made at the last Surgical Congress by Kuester, of Berlin, the catgut prepared by Schede's method (immersion for twelve hours in sublimate solution 1-2 : 1000) is soaked in juniper oil, whereby it acquires extraordinary flexibility and strength. On various occasions, unfortunately also by autopsy, we have been enabled to convince ourselves that the sutures were indeed quite firm even after seven days. Since employing this running catgut suture, we have had, even in the most complicated plastic operations on the perineum and the vagina, such excellent results as regards first intention that anything better could hardly be expected. Schroeder has employed this suture with the best results also in the closure of intra-abdominal sacs

in complicated laparotomies. The method is very simple: the tissues are merely sewed together with the running suture, the thread being moderately tense, without any necessity for special care to secure accurate coaptation.

In view of the increasing reports of speedy relapses after attempted *radical operation for carcinoma of the uterus*, and of the opposition made particularly by Americans at the last Gynecological Congress against the bloody radical extirpation of carcinoma in general, it seemed to us to be indicated to collect the results obtained by this operation, and to subject the views as to its value or the reverse to a critical examination by the light of larger numbers. However, we did not enter on this labor with much confidence, for the general impression we had as to the frequency of relapses was not favorable. But when, at Professor Schroeder's instigation, I came to carefully sift the entire material observed since his arrival here, at the policlinic and clinic as well as in his private practice (812 cases of carcinoma), very much better results were found than we had anticipated. Without entering too minutely into details, I shall briefly state that of these 812 patients 160 were adjudged fit subjects for the radical operation which was performed according to the most various methods. Altogether, 31, or 19.4 per cent, died of the immediate consequences of the operation. In 105 cases, simple vaginal or supravaginal amputations were performed, with 13 deaths. Among them were 13 cases in which the actual cautery was applied to the wound surface after the amputation. In 8 cases, Freund's total extirpation was done, with 5 deaths; in 34 cases, vaginal total extirpation, with 9 deaths; and in 13, supravaginal amputation for carcinoma of the body, by laparotomy, with 4 deaths. In order to have a definite measure of time after the lapse of which the subjects of the operation might be looked upon as virtually saved, we took two years as the limit, and found that of the 129 still surviving, 28 were healthy after two years. But as it appears to be more correct to place these 28 recoveries of two years' standing in juxtaposition with those operated on up to two years before, rather than with those operated on to the present time of whose certain fate nothing definite is known, we obtain the following result. Total number of operations to the beginning of 1882, 92. Of these 15 died, hence 77 survived. And of these 77 survivors, 28, or 36.3 per cent, were still well after two years; but in many of them, good health had been demonstrated for three, four, and even five years. We hold this percentage to be very favorable, and at any rate sufficiently so to vie with the results obtained by surgeons under generally more favorable con-

ditions. According to the several modes of operation, the results appear as follows: There remained in good health after two years:

1. After the supravaginal amputation of the body of the uterus 80 per cent.

2. After Freund's operation (only 3 cases surviving), 33 per cent.

3. After the vaginal total extirpation, 33 per cent.

4. After the vaginal and supravaginal amputation, 30 per cent.

5. After the amputation with subsequent actual cautery, 42 per cent.

On the whole, we have concluded from our results that the bloody radical operation of uterine carcinoma is to be attempted by all the operative methods in cases which still offer any prospect of success; but that in doubtful cases it is better to confine one's self to a palliative operation with an energetic application of the actual cautery.

Finally, I shall report two *interesting operations* recently performed at our clinic by Professor Schroeder. The one was that of a young woman, aged twenty-three, with *bilateral ovarian tumors*, each the size of a child's head. Inasmuch as she had married young, it appeared exceedingly desirable not to remove both ovaries. On the right side it was impossible to leave anything behind; but on the left there was mainly a large cyst, around the starting point of which some ovarian tissue was still present. Schroeder circumsected the cyst at some distance from its origin, succeeded in enucleating it, and then united the remnants of the ovary with fine catgut sutures. This is the fourth operation of the kind performed by Professor Schroeder under equal indications. Three of the patients recovered; the fourth was hemophilic and perished. The other interesting operation was that of a patient with extraordinarily large, multiple *myomata*, who had become *pregnant* after her marriage had been sterile for many years. The uterus, before almost as large as near term, had increased so rapidly in size through the pregnancy that the patient's symptoms had become unbearable as early as the third month, making her clamorous for relief. Of the two modes of relief—artificial abortion with subsequent myomotomy, or operation in the presence of pregnancy—Professor Schroeder chose the latter, because it would not materially increase the gravity of the operation and at the same time avoid the not inconsiderable danger of artificial abortion. The operation, in the first place, proved the impossibility of removing the myomata while sparing the gravid uterus, as the latter was interspersed with nodular tumors.

Uterus and tumors were turgid and livid with blood. The operation could be done in the typical manner, viz., after the appendages on both sides had been carefully ligated, the rubber tube was laid low around the cervix. Then the tumors above the tube were ablated, during which a colossal venous hemorrhage ensued. The incision was carried just deep enough to preserve the ovum intact in the uterine cavity. All the vessels were secured singly; the lower deep sutures were of catgut, the upper of silk. After the tube was loosened, there was some hemorrhage here and there, but not much. The operation lasted about an hour and a half. This was the second operation of that description performed by Schroeder; in a third case the pedunculated large myoma was tied off the gravid uterus. The pregnancy continued uninterrupted. Two patients recovered, the third is now convalescing.

BERLIN, June, 1884.

CORRESPONDENCE.

ON THE RELATIONS BETWEEN MEDICINE, SURGERY, AND OBSTETRICS IN LONDON.

BY
ROBERT BARNES.

DEAR DR. MUNDÉ:—I promised to send you a sketch of the actual relations between medicine, surgery, and obstetrics, including gynecology, in London especially. These relations are somewhat peculiar, even anomalous, and, it appears to me, adverse to the sound progress of all three departments of the healing art, separately and in their solidarity.

If we start upon the general principle that medicine is a whole, each part of which stands in vital relation to the rest, we cannot escape from the conclusion that the tendency, so marked in this town towards disintegration into specialties, is pernicious in its influence upon science and upon those who pursue it, and therefore unjust to those who are the subjects of that science when applied. I do not enter upon the task of exposing this evil with much present hope of removing it, but rather with the purpose of explaining how it is that in this, the greatest centre of population in the world, abounding in clinical materials, with two Royal Colleges of Physicians and Surgeons and a University, with numerous flourishing medical societies, and a body of teachers

and practitioners that may fairly challenge comparison with those of any other community, the condition of gynecology is not one to which we can point with pride.

The unsatisfactory position of obstetrics and gynecology in this country, and more especially in London, may be traced to several causes, some traditional, some of our own creation or tolerance.

In the first place, there is the old tradition that obstetrics, having been almost exclusively practised by women, was an inferior branch of medicine, one hardly deserving to be ranked with medicine and surgery proper. Although this old prejudice has been shaken in modern times; although obstetric science and practice have been illustrated by such men as Harvey, William Hunter, Smellie, and a long succession of men not inferior to those who have adorned the other branches of medicine, it still lingers amongst us, and exercises an unjust and depressing effect upon the present representatives of obstetrics. Thus, for example, even now, eminent physicians and surgeons esteem it a point of honor, if not of self-congratulation, that they are ignorant of midwifery, as if a little knowledge of midwifery could hurt or disgrace their special medical or surgical skill.

This notion once received a merited and characteristic rebuke from Robert Lee. At a full meeting of the Medical and Chirurgical Society, the late Mr. Skey, bursting with an exultant sense of his surgical dignity, exclaimed: "Thank God, I know nothing of midwifery!" The rejoinder was prompt and crushing. "If," said the veteran obstetrist, "the gentleman who has just spoken is thankful for his ignorance, he has much to be thankful for!" Yet to emphasize this boast of obstetric ignorance, and to illustrate our text, I afterwards saw Mr. Skey perform a Cesarean section at St. Bartholomew's Hospital. Although a good surgeon, he himself perhaps would not at that moment have contended that he was the better able to deliver a woman by Cesarean section because he knew nothing of midwifery. I am of opinion at any rate that the then obstetric physician to Bartholomew's, Dr. Greenhalgh, would have done the operation better because he knew something of midwifery. But the traditions and rules of this great hospital forbade their obstetric physician to carry out to its consummation a strictly obstetric case.

The same force that works against obstetric surgery is found in the constitution and traditions of our colleges. The relation of obstetrics to the colleges is remarkable. It would naturally be supposed that obstetrics, being more surgical than medical, the teachers and consultant practitioners of obstetrics would find their home in the College of Surgeons rather than in the College

of Physicians. But the reverse is the case. I believe there is no example of an obstetrict having had a seat on the Council of the College of Surgeons unless he came as a provincial representative of surgery. This exclusion is not justified by the charter of the college. The qualification laid down in the charter is simply that the councilman shall be a Fellow of fourteen years' standing in the bonâ fide practice of his profession as a surgeon, and shall not be practising as an apothecary. Thus to an expressed disqualification there is added, by a tacit conspiracy, an unauthorized disqualification, inflicting a stigma upon obstetric practice.

It took a long time and considerable pressure to induce the Council to acknowledge by action that a little knowledge of obstetrics was a useful qualification for its membership. When it instituted a midwifery board in 1852, and enlisted as examiners Arthur Farre, Oldham, and the late James Reid, and later, in succession, Charles West, Robert Lee, Robert Barnes, and Priestley, the diploma authenticated by these examiners was not held to be an essential qualification for the membership. Candidates for the membership were not compelled to pass the midwifery examination. A man might be registered as licensed to practise all branches of medicine, surgery, and obstetrics on the diploma of M. R. C. S. without having given to the public any guarantee that he knew anything of obstetrics. The men who came up for the midwifery license came of their own free will to supplement a deficiency in their qualification which they had been made to feel under the pressure of public opinion. Down to 1876 this state of things continued. None were admitted as candidates but men who already possessed a registrable qualification. Thus the college midwifery license fulfilled a useful purpose. It afforded an opportunity to many men who suffered in their own estimation and in that of the public to repair in some degree the consequences of the laches of the College. At the same time, however, this Board was a standing impeachment against the conduct of the College. In the first place, they ought never to have neglected the plain duty of basing the title to practise on a true and complete examination. They ought not, by this negligence, to have gone on inflicting upon their own members, whom they had sent out into the world as fully competent to practise, the indignity of having to come back to the College to seek a special testamur of obstetric competency. Yet this indignity was cast upon many men of mature age, some of whom were indeed older than the examiners themselves.

Notwithstanding the self-condemnation conveyed by this flagrant inconsistency, the Council of the College was still so op-

pressed by the ancient prejudice that, instead of making obstetrics an integral part of the examination for the membership, it took the very opposite course, of detaching obstetrics altogether, by endeavoring to create a special license in midwifery distinct from any surgical or medical qualification. And since it was a part of the scheme to admit women to the examination for this bare midwifery license, the College, so far from honoring obstetrics or advancing medicine in the public estimation, was really degrading it back to the position it occupied in the dark ages. But in this scheme the Council happily failed. The Midwifery Board refused to be agents in this ignominious work. I resigned a seat no longer one of honor. Dr. Farre and Dr. Priestley took the same course. The Board was broken up. No teacher in London would accept the vacant posts. And so the College went on without a Midwifery Board for some years, until in 1881, the principle I had all along contended for, of making obstetrics, like surgery and medicine, an integral part of the examination for the diplomas of Fellow and Member, was conceded. Then special examiners in obstetrics were appointed.

The wisdom of the course adopted by me in refusing to aid in the unnatural scheme of divorcing obstetrics from medicine and surgery has quite recently received emphatic recognition at the hands of the Council, Fellows, and Members of the College of Surgeons. On the 24th March of this year, for the first time in the history of the College, the Fellows and Members met the Council to deliberate on certain proposed "alterations in the charters of the College adopted by the Council on the 10th of January, 1884, to be reported to the meeting of the Fellows and members of the College, convened for the day named." The sixth alteration submitted was as follows: "That Section 17 of the Charter of the 15th Victoria be abrogated, it being inexpedient to confine the examinations for the license in midwifery of the College." The chief argument urged in favor of this abrogation was that, the section standing, any *person* might compel the College under *mandamus* to examine for this anomalous license, and the Council might find itself in a very awkward and undignified position. It need not be said that the proposal to abandon this unpleasant duty was unanimously assented to by the Fellows and Members.

So ends one scheme for degrading obstetric science and for injuring the profession and the public, by adding one more to the nineteen already existing titles to register as qualified to practise.

Since the Council of the College of Surgeons is composed almost exclusively, and by a kind of prescriptive right, of surgeons

to the general hospitals, it is but natural that the policy of the council of the college should be reflected in the government of the hospitals. The governing bodies of the London hospitals are variously constituted. Three principal forms may be noted. First and worst is the autocratic form in which an absolute ruler is nominated by the corporation of London or in some other irresponsible arbitrary way, under the name of treasurer. It is not considered necessary that the person chosen should be conspicuous for ability, or for knowledge of hospital work, or the needs of medical education, although he is suddenly invested with considerable power over both. What the qualifications are can only be known by those who elect. Absolute power should be coupled with absolute wisdom, and certainly such union has rarely been seen at the three great hospitals. Power the treasurers have had, the correlative faculty has generally been conspicuously absent. A second form of government is a representative one of the body of governors or subscribers to the hospital. A committee practically self-elective is appointed by the governors. This holds at the London hospital, an institution conducted with admirable efficiency. A somewhat similar constitution exists at University College and King's College hospitals. At these three institutions the medical staff is fairly if not completely represented. They hold an equally independent position; but of course are in hopeless minority. The third form of government is that in which a board of management open to all the governors rules. Since it is open to all the members of the staff to qualify as governors, they meet the lay governors with equal voice. Thus the representation of the medical staff is complete. This form obtains at St. George's and the Middlesex. It is the most free, liberal, and satisfactory, and offers the best security against back-stair influence and jobbery.

Still, under all these various forms of government, it may be truly said that obstetric medicine is tolerated rather than represented. The obstetric physicians occupy a position scarcely better than that of supernumeraries. They have a few beds assigned to them for diseases of women, far less in number than those assigned to the physicians and surgeons, and ridiculously inadequate to the needs of the poor women suffering from diseases peculiar to the sex. And the cases admitted to these beds are rigorously defined and controlled by the surgeons. The obstetric physician is at liberty to treat surgically a uterine polypus, for example; but he has no monopoly even in this. The surgeons admit into their wards any cases they please, including

gynecological diseases. But cases touching ever so slightly upon to border-line between the uterus and the adjoining regions are jealously denied to the obstetric physician.

This is most strikingly exemplified in the regulations existing at St. Bartholomew's. At this hospital, which, from its magnitude and antiquity, is naturally looked to by those in search of precedents to support the policy they are inclined to, the relative position of the obstetric physician illustrates in a remarkable manner the tyranny of the surgeons. By a law or equivalent authority passed in 1855, it was ruled that "all surgical operations as may be requisite in Martha's (the gynecological) ward shall be done by the surgeons in rotation." This very comprehensive law, it seems, was infringed by Dr. West who, in 1859, was summoned to attend a committee of the treasurer and almoners to explain how it was that he had taken upon himself to perform an operation of a surgical nature upon a patient admitted into Martha's ward. Dr. West explained and apologized. He stated his intention, not only to abstain from performing such operations, but "would at once send to a surgical ward such cases as might require surgical operations." The committee accepted Dr. West's explanation, but declined to sanction any such proceedings as he contemplated, and "required him to admit to Martha's Ward the same class of cases hitherto sent there, and then to send to the surgeon to operate." This order was confirmed in 1861, when Martha ward was declared to be a ward set apart for the obstetric physician who is to call in a surgeon to operate. In 1870, it was ordered that, in lieu of the surgeons operating in turns every three months, the performance of surgical duties in the ward be intrusted to the fourth surgeon and the first assistant surgeon. It will be readily imagined how hard it must have been for a man of high spirit, as Dr. West is well known to be, to brook such insolent tyranny as this. The rules under which he was called upon to perform his duties actually compelled him to admit cases into his ward which he was not allowed to treat. At a certain point, or whenever anything which the surgeons might declare to be surgical treatment was indicated, his hands were tied, and he was obliged to call in a surgeon to take up the case. He was not even permitted to decline to receive patients whom he was declared incompetent to treat. And yet he was expected to teach.

It could not be expected that things would go on smoothly under such a régime. Dr. West retired. But the rules under which he writhed are still in force upon his successors. The interpretation of what falls under the expression "surgical treatment"

does not rest with the obstetric physician. The words, if rigorously interpreted, might forbid him to remove a polypus or to apply cauterly, actual or potential. If he goes so far as this, it is by tolerance, not by right. Latterly some relaxation has been granted. The obstetric physician is now allowed to deliver a woman by Cesarean section. But he must be very sure that the woman is pregnant, for he is forbidden to remove ovarian or uterine tumors, and *à fortiori* other abdominal tumors, by abdominal section. He is not allowed even to make an exploratory incision. Like Minerva, he is expected to come forth fully equipped, without going through the ordinary processes for acquiring knowledge. And even this concession of Cesarean section is liable to be revoked or limited. For example, it remains to be seen how far the surgeons would recognize the propriety of performing the Porro-Cesarean operation. As this involves the removal of the uterus and ovaries, it would be prohibited under the strict interpretation of the rule which forbids the obstetric physicians to practise ovariectomy. And even if liberality were stretched to the point of permitting such an operation in a hospital, it must still be obvious that practice under a system of surveillance and control by men necessarily unable to appreciate fully the history of these cases, must be destructive of that spirit of independence and enterprise which is the very life of medical progress.

The surgeon at St. Bartholomew's also performs the operation for vesico-vaginal fistula. A curious arrangement was made with regard to perineal operations. If required for relief of prolapsus uteri, the obstetric physicians might do them; if for incontinence of feces and flatus, then the surgeons claim the operation. What a nice distinction! how ingeniously the line is drawn just between wind and water! Is it never transgressed by the surgeon on the one hand or by the obstetric physician on the other?

The utility of these rules may be tested from three points of view. *First*, the patient's. Can it be contended that it is good for a patient to change her doctor at the critical moment of treatment? Is it reasonable to suppose that the knowledge of the case in its origin, development, and progress, which had been gradually acquired by the first doctor, can suddenly be transferred to the second doctor who steps in to take possession of the case for the purpose of surgical treatment? Is it not clear that each of the two, studying half of the case only, will be imperfectly fitted to do justice to the patient? Is this divided care consistent with the responsibility which properly attaches to the

doctor? Does it not tend to weaken the interest he might feel in the patient's welfare, if she were in his sole charge? Does it not encourage both doctors to shift the blame of error in diagnosis and failure in treatment upon each other? The obstetric physician may say, "I diagnosed the disease, and there my responsibility ended." The surgeon may say, "I operated on the judgment of the obstetric physician, and upon him must rest the responsibility."

Secondly. Does this division of work, involving partial knowledge of the case, conduce to the scientific and clinical improvement of either? Take first the position of the obstetric physician who begins. He studies the case up to a certain point, and there must stop, and stop just at the point when the luminous experiment of treatment is made, when the seat of the disease, the relations of the diseased structures are exposed to closer and more precise observation than was possible before. It may be said that the obstetric physician may look on and see the surgeon do all this. But surely no one of experience will contend that the information so acquired can be compared with that which comes to the man who operates for himself. The operation is the culminating point which brings all the theories hitherto formed as to the nature and complications of the disease to the test. How often do we find, whilst carrying out an operation, things not suspected before, cropping up under our fingers and eyes, throwing new light not alone upon the case in hand, but suggesting new thoughts to be developed into fuller knowledge of cognate pathological problems. An instance in point, full of interest, lately came home to myself. During an ovariectomy at St. George's Hospital, after having removed the tumor, I had some trouble in finding the other ovary for inspection. I found it at last low in the pelvis, nipped under the fundus of the retroflected uterus. Having uncurled the uterus, I was able to draw up the left ovary into view. The lesson to be deduced from this direct physical observation is this: The ovary so imprisoned would, under the engorgement of the menstrual nix and the attendant swelling of the uterus itself be so compressed that pain would be caused; that is, there would be ovarian dysmenorrhea. It may be said that all this might be made out under ordinary circumstances by vaginal and rectal examination. Perhaps, but not so clearly or in such a convincing manner. To me it has been a source of profitable physiological, pathological, and therapeutical speculation.

Take ovarian tumors for further illustration:

How can a mere man-midwife arrive at a full or accurate knowledge of the manifold varieties in these tumors, in their

origin, development, history, complications, and the many accidents they are liable to, unless he enjoys frequent opportunities of seeing and handling these tumors as exposed in operations? How otherwise can he possibly cultivate the diagnostic skill which is necessary for the differentiation of ovarian tumors from the numerous other tumors of pelvic and abdominal origin? As well might he pretend to understand the anatomy of the pelvic and abdominal organs without dissection.

Wanting the knowledge of what can be accomplished by operation, he will hardly appreciate the conditions that indicate the expediency of an operation. He will be apt to postpone advising an operation until the most favorable opportunity has been lost. I do not hesitate to express my conviction that the opinion of a man who does not operate, upon a case of abdominal tumors, is entitled to scant respect.

If that is the position of the fettered obstetric physician, what is the position of the surgeon? It is no better. In the first place, by his neglect and practical ignorance of gestation and of pelvic diseases, his diagnostic faculty fails in a vital point. This objection, of course, applies with varying degrees of force to different surgeons. Exceptionally trained men, like Sir Spencer Wells, may, by special experience, be armed at all points. And Sir Spencer Wells' experience was not attained in a general hospital. Our concern is chiefly with the ordinary hospital surgeon, who denies to the obstetric physician the right to operate. To the majority of these the objection applies in full force. Acting under their own imperfect light, they incur the most imminent danger of opening the abdomen to reveal a gravid uterus, of tapping a solid fibroid, and of overlooking some serious pelvic complication. So paralyzing has been the influence of this system upon the knowledge and practice of abdominal surgery that, although an occasional ovariectomy was performed in the general hospitals, the fact remains that no advance of importance in this direction was made in these institutions until they felt the impulse of enterprise from without.

The system, then, is injurious and degrading to both. The obstetric physician is degraded by his subjection to the surgeon. What sense of independence can remain in the man who can tamely accept the duty of feeding the surgeon with operations, of playing the part of jackal to another man's lion? What honor can he reflect upon his department when he declares to the public his incompetency to carry out the most important part of the treatment of the cases naturally falling to his care?

Thirdly. How does this system lend itself to the great work of

teaching? It must be assumed that one main duty of the obstetric physician to a hospital is to teach. But how can he teach who is not permitted to practise and learn?

And if we would see absurdity in the superlative degree, we have only to reflect that the obstetric physician is called upon to teach gynecology to students who, by simply declining to take the style of physicians, resting content, as the majority of London students are compelled to be, with the style of surgeon, thereby become qualified to do what their teacher cannot do! And they are truly more fortunate than their teacher. They are free to learn, free to practise, and in this freedom they find the opportunity of advancing science, of applying it to the benefit of mankind, and of earning an honorable reputation.

But this freedom they will seek elsewhere than in a London school-hospital.

Another evil consequence of the neglect of the pure physicians and surgeons to study the physiology and pathology of the female generative system by the objective method, is that they are necessarily incapacitated from judging with precision of the nature of most cases of disease of intrapelvic origin; and for the same reason they are unable to judge fairly of the merits of the men who more especially study gynecology. They yield to the natural bias of believing in those whose dicta reflect most closely their own imperfect knowledge. In this way the most grievous wrong is often done to the more enterprising and able obstetrists. These are branded as specialists by men, many of whom are themselves specialists in the closest meaning of the term. They who refuse to apply the recognized methods of objective observation to the pelvic organs, for whom there is no pathology below the hypogastrium, have surely no right to denounce as specialists those who, not neglecting other regions and the body as a whole, embrace the pelvic organs within their field of research.

I have submitted a definition of the term "specialist" which I venture to repeat: "A specialist is one who specially neglects important factors in the causation and process of disease." Tried by this definition, which is the specialist; he who so neglects essential factors in the problem, or he who, studying with particular care those factors which the other neglects, does not neglect all recognizable factors in their individual and collective characters? For my own part, I emphatically repudiate the title.

Not less erroneous than the current idea of a specialist is that of the correlative so-called "all-round man." In its large and true sense, "the all-round man" would be the highest type of

physician. Where are they? There are indeed a few who well deserve the distinction. A man is not necessarily a good "all-round-man" because he is not credited with especial skill in one class of diseases. By far the greater number of so-called "all-round men" would more correctly fall under the description of "specialists." Their knowledge is not represented by a circle, but by a segment only. The arc that is wanting in their estimate of disease in woman, is often precisely the most important part of the circle, without which there can be no completeness in diagnosis, no rational indication for treatment.

This argument might not without profit be extended beyond the discussion of the more strictly female diseases. In the study of gestation, the true pathologist might get nearer to the origin, and therefore to the nature, of many diseases which now baffle his research. He might trace back some diseases to mere excess of physiological action. He might trace others to the influence of gestation in evoking latent and unsuspected morbid conditions. This theme, so rich in suggestiveness, I cannot here pursue. It is discussed and illustrated in the "System of Obstetric Medicine and Surgery" I have recently published in conjunction with Dr. Fancourt Barnes.

In the whole range of medicine there is no experiment more fertile in physiological and pathological knowledge than gestation. It is an experiment that is being continuously repeated, with the certainty of a natural law. It is beyond the control of the shrieking crew of unwomanly women and unmanly men who, impelled by the cruel bigotry begotten of ignorant dilettanteism and vanity, appeal to the lower reflex and emotional qualities of the unthinking, to help them to propagate loathsome diseases, and to trammel science in her efforts to prevent them.

I have cited the special hospitals for women as practical protests against the exclusive pretensions of the general hospitals. And this is true. But even in some of these the prejudices of the surgeons are reflected.

Quite recently this came into prominence at the Vincent Square Hospital for Women. Dr. Culver James brought a complaint against his colleagues, Messrs. Skene Keith and Smythe, before the Metropolitan Counties Branch of the British Medical Association. It appears that Dr. Culver James, being physician to the hospital, acting temporarily for Mr. Smythe, one of the surgeons, had the audacity to perform an ovariectomy. Mr. Keith, holding this to be an unjustifiable invasion of surgical functions, accused Dr. James of "performing an operation contrary to the laws of the hospital; and of knowingly and wittingly inducing him, Mr.

Skene Keith, innocently to take part in a forbidden operation, thereby rendering Mr. Keith guilty of a breach of the rules of the hospital." The most touching feature in this charge is the making the surgeon an unwitting accomplice in the crime of "*lèse-majesté chirurgicale*." How very shocking thus to entrap unsuspecting innocence!

The decision of the committee who sat in judgment is tainted with the surgical prejudice. They declare that the dispute "has arisen from an unfortunate union *in one and the same person* (Dr. Culver James) of two distinct and separate functions, viz., those of physician and of surgeon at *one and the same hospital*." They regard this union as unnatural. Their judicial faculty is on a par with their grammatical perception. How can Dr. Culver James be other than "one and the same person"? It might have been expected that a tribunal, that could lay down this remarkable proposition, might also have grasped the oneness and sameness of medical science. If Dr. Culver James could be other than "one and the same person," then the medical and the surgical qualities might be disjoined. But I must give up the task of subjecting this decision to further analysis. The decision may do for the Vincent Square Hospital, whose governing body compelled Dr. James to resign.

At the Soho Hospital for Women, the first I believe of the kind, the physicians also act as surgeons. At the Samaritan Hospital, there are physicians and surgeons, as in the old hospitals. I believe the surgeons alone operate. At the Chelsea Hospital for Women, the titular name applied to all the ordinary staff is that of "physician." The term is used comprehensively. All are physicians; and the impossible task of splitting up "one and the same" case into medical and surgical is not attempted. The consequence is that no intestine or unseemly disputes arise; there is no fighting over the beds of the patients; and scientific investigation is prosecuted in the best and happiest spirit.

The first effective blow struck at the monopoly of the surgeons over abdominal and pelvic surgery in our London hospitals was dealt by Tyler Smith, on the foundation of St. Mary's Hospital. He, being appointed obstetric physician, asserted the duty and right of the obstetric physician to carry through, even to surgical treatment, the cases that by common consent fall under the domain of obstetrics and gynecology. He initiated in this hospital, which, being a new foundation, was unfettered by precedent, the practice of ovariectomy by the obstetric physician. This practice is continued by his successors, Dr. Meadows and Dr. Wiltshire, and it is to be hoped will be perpetuated. It is a distinction of

which the youngest hospital-school in London may well be proud. We believe the example thus set has been turned to account at University College Hospital and Kings College Hospitals, where Dr. Graily Hewitt and Dr. Playfair respectively perform ovariectomy and other cognate operations. By special favor of my surgical colleagues, I enjoy and practise the like privilege at St. George's Hospital. This right, although affirmed by the governing body of the hospital, rests on a personal concession made to me by the surgical staff, which has declared that it shall not be extended to my successor.

At Guy's Hospital, I have seen both Dr. Hicks and Dr. Galabin perform the Cesarean section; and I believe that the obstetric physicians are still permitted to practise obstetrics, to the extent of delivering a woman by this operation. They have also practised abdominal section for delivery of the extrauterine fetus. Until recently they even performed ovariectomy. But against this the surgeons revolted, and bringing their influence to bear upon the Treasurer, who occupies the position of dictator, they succeeded in depriving the obstetric physicians of this privilege. It was not because it was shown or contended that it was better for the patients, or that the obstetric physicians were not competent operators. That would have been too absurd. The surgical staff of Guy's includes, it is true, very able surgeons; but, we are well assured, not one who is more able to treat an ovarian or other abdominal tumor from beginning to end, than Braxton Hicks. The question of oöphorectomy or of Tait's operation has not been raised; but probably the surgeons would object to these operations being done by the obstetric physicians, which is tantamount to stopping all advances in this direction.

It is a remarkable and very significant fact that, although the obstetric physicians attached to some school-hospitals are thus prevented from the practice of abdominal and pelvic surgery in some of these hospitals, they all, with at most two or three exceptions, who thus practically confess their want of surgical aptitude or of independence, perform ovariectomy and cognate operations outside their hospitals. In this way, they give unmistakable evidence of their convictions upon the subject. And since they cannot acquire the necessary experience and skill to excel in these operations by practice in their hospitals, several of them have knocked at the doors of the special hospitals for women. Thus Dr. Galabin has taken office at the Bolingbroke pay hospital. Dr. Edis, obstetric physician to the Middlesex, where the old superstition reigns supreme, and Dr. Horrocks, assistant obstetric physician to Guy's, have attached themselves to the Chelsea Hos-

pital for Women, avowedly for the sake of the opportunities for operating which are denied them in their school-hospitals. This mode of emancipating themselves from the thralldom of the surgeons is not altogether satisfactory.

The school-hospitals which fetter their obstetric physicians, the general medical and surgical staff, and the patients lose the great benefits which could not fail to accrue from the mutual impulse of generous emulation and reciprocal instruction. The surgeons especially are losers. They might usefully reflect that under their jealous monopoly, hardly a step in the enormous progress made of late years in the knowledge of abdominal and pelvic surgery is due to them. The initiative work has sprung up as almost all initiative work does, under the conditions of freedom from arbitrary restrictions. This freedom is found outside the general hospitals. The longing for freedom of research is not to be repressed. Hence what are called "special hospitals" have been established; and in these, the departments of medicine and surgery, which were comparatively neglected or tabooed in the general hospitals, have been successfully cultivated. Amongst the first instances of this movement was the establishment of the Moorfields Ophthalmic Hospital, founded, I believe, by the father of Arthur Farre. Now there are numerous ophthalmic hospitals. Institutions for the cultivation of orthopedic surgery, nervous diseases, diseases of children, diseases of women, have sprung up. Under the natural pressure of need for hospital relief, some extension of accommodation beyond that afforded by the general hospitals was no doubt inevitable. But it is not the less certain that the greater number of special hospitals have been founded to supply a field for the exercise of surgical energies excluded from the general hospitals. And they have abundantly justified their existence. Ophthalmic surgery, neglected or discouraged in the general hospitals, has flourished at Moorfields, making great reputations and conferring endless benefits upon the community. Special hospitals having thus achieved an independent position, it began to be felt that the general hospitals would be strengthened, made less special, if they too cultivated ophthalmic surgery a little more carefully. At first, one or two of the junior surgeons were charged with the care of eye-patients in addition to their ordinary duties. Gradually this half-measure was found to be unsatisfactory. Students called for more accurate instruction. And now, I believe, every general hospital in London has attached to its staff a distinct ophthalmic surgeon, and in some cases an assistant also. Upon these devolved, not alone the care of out-patients afflicted with eye-diseases, but also of an ophthal-

mic ward, and the duty of giving instruction clinically and by lectures. Notwithstanding this recognition of the claims of ophthalmic surgery, it is a fact not without significance, that almost all the ophthalmic surgeons attached to the general hospitals find the position and the opportunities granted them in these hospitals insufficient to satisfy their ambition, or to give them full scope for clinical activity. Almost all hold appointments in special ophthalmic hospitals as well.

This relation of ophthalmic surgery to the great hospitals, then, presents a parallel to that of obstetric medicine and surgery. It has been felt that the curriculum of education dictated by the progress of science, the demands of the examining boards, and those of an instructed public, made it imperative to institute separate courses of lectures on ophthalmic surgery and on obstetrics and gynecology within the walls of the hospitals; and so special chairs have been instituted. So far the parallel holds. But points of difference must be noted. The ophthalmic surgeon is allowed to treat the eye according to the therapeutical indications presented; that is, he may treat it throughout medically and surgically, not harassed and thwarted by the case being taken out of his hands at the moment of greatest interest; whereas the obstetric professor is crippled in his treatment of the ovaries and uterus; he must not operate beyond the point which the surgeons may declare to be proper.

Why is this? The answer, if answer it be, lies in the fact that one is called "surgeon," the other "physician." And these titles, the one given by a college of surgeons, the other by a college of physicians, are held to distinguish and to limit the range within which each shall be considered competent to practise. Until recently the examinations of the colleges respectively were based upon this distinction of functions. The College of Physicians did not examine in surgery, and the College of Surgeons did not examine in medicine. This division, entirely arbitrary, artificial, and injurious, received to a certain extent the assent of the profession. But it has always been more strictly observed by the physicians than by the surgeons.

The physicians rarely operate. The surgeons on the other hand have been less punctilious. They find it very wrong that a physician should handle a knife; but they find it quite right that surgeons should prescribe for medical cases. They accept Sir William Lawrence's definition of medical and surgical cases: "Surgical cases are those that pay fees, the rest are medical."

So long as the physician failed to study surgery, and the College of Physicians did not require evidence of surgical skill, it was

right that the "pure" physician should abstain from practising surgery. But the disability is now simply arbitrary and traditional. The physicians who operate are competent surgeons who have gone through a surgical education and surgical examinations. The University of London degrees embrace a complete curriculum in medicine, surgery, and obstetrics. The Scotch Universities, although less exacting in degree, require the same qualifications in kind. Besides possessing this title to practise surgery, many of the obstetric physicians are also members or fellows of the College of Surgeons. It is, therefore, on the intrinsic merits of the case, simply absurd to declare that because being surgeons they add the qualification of physician, they become thereby incompetent to practise surgery. The case of the obstetric physician, as contrasted with that of the pure surgeon in presence of an obstetric patient, is briefly this. "The obstetric physician is a surgeon and something more. The pure surgeon is a surgeon and nothing more." As the whole includes the parts, so the obstetric physician is the better qualified *ad hoc*.

I have not learned that the College of Physicians as a body has ever expressed a formal objection against its fellows, members, or licentiates practising surgery. Certainly the college has not inflicted the penalty of disability for high office upon those of its fellows who practise obstetrics, including the correlated surgical operations. Arthur Farre, Charles West, and myself have filled the office of Censor. And members who practise obstetric surgery have been raised to the fellowship. The college charter covers the right of surgical practice. Still the liberality of the college of physicians in extending hospitality to obstetric practitioners, receiving them within its fold, and giving them a status in the governing body has not been without disadvantage to those whom it has so honored. Adopting them, it made them nominally physicians, ostensibly detaching them from the orthodox surgical faculty. And thus the traditional law, strong by custom, and jealously enforced by the small ruling body of the College of Surgeons, declaring that a physician is not a surgeon, leaves the obstetric surgeon to fight for his right to carry out the surgical treatment of the obstetric cases that fall within his care.

It would be difficult at the present day to trace the steps that led the obstetric practitioners to the portals of the College of Physicians rather than to those of the College of Surgeons. Certainly, the element of surgery predominates over that of medicine in the amalgam of obstetrics.

The obstetric practitioner is necessarily a surgeon. He might almost dispense with the pharmacopeia; but his hands and in-

struments he must use. To use them skilfully, he must exercise them frequently and in the greatest variety of operations that fall within the range of his department. Not only is this exercise necessary to the acquisition of mechanical skill, but it is not less necessary to the acquisition of full and accurate knowledge of the art he practises.

Let us now define the scope of obstetric medicine. In my address as President of the Obstetrical Society in 1865 (*Obst. Trans.*, 1866), I submitted the following as the proper definition: "The work of the obstetric physician embraces the treatment of the diseases of the female generative organs, including the disorders and lesions, general and local, which result from pregnancy and parturition." I do not see how limitation can be made narrower than this.

An exactly parallel definition accords to the ophthalmic surgeon co-extensive command, medical and surgical, over the eye. Why should not the like measure be dealt out to the obstetric physician? Why should the one be privileged to roam over the entire pathological field of his department, uncontrolled in his operations, and the other be crippled in his clinical work of research and treatment?

Again the only apparent plausible argument crops up: Is it because the one possesses the rank of surgeon, whilst the other is a physician? In every essential respect, the arguments in favor of the one apply with equal force to the other. It has been urged that the surgeon, by general exercise in operations, acquires more skill in operations than can be attained by a man whose operations are restricted to a particular region of the body. The proposition is not without plausibility, but it does not cover the case in contention. Why is it that the eye has, of late years, been almost exclusively intrusted to a special surgeon? Is it not possibly because he is supposed to acquire peculiar skill by exercising it upon a single organ? Does not this argument apply with at least equal force to the obstetric physician?

If the surgeon is permitted to say: "The obstetric physician must turn over to us all operations," by parity of reasoning the physician might say: "The obstetric physician must turn over to us all that requires medical treatment," for example, puerperal fever, which is not more a consequence of labor than is a slough of the vagina resulting in cicatricial atresia or in vesicovaginal fistula. Now, this mode of reasoning, logical though it be, would lead to the annihilation of the obstetric physician—a manifest *reductio ad absurdum*. It leaves the obstetric physician without an apology for his existence.

Thus, looking at the question from whatever point of view, we are driven to this explanation, that a surgeon is free, by natural or prescriptive right, to do as he pleases, whilst the physician is only free to what the surgeon declares is right. Now, having gone thus far in the argument, I will not shrink from the logical consequence, audacious and revolutionary as it may appear, of questioning the right of the surgeon to dictate to the physician the limits within which he shall practise. Where is his commission?

The truth is, that the surgical dynasty rests entirely upon arbitrary conventional rules which have become obsolete by the progress of medicine, and the juster and broader system of medical education now prevailing. So long as physicians never made surgery a serious study, it was natural that surgery should fall to those who did. But now that surgery forms an integral part of medical education, by the same reason the physician who has so studied surgery possesses equally with the surgeon the right to practise what he has qualified himself to practise.

I do not venture to ask how long this injurious subjection is to endure. I will content myself, having no personal interest in the matter—for I, long ago, like Tyler Smith, emancipated myself—with pointing out that what some have done the rest may do, if they only have the courage to do it. They may reflect that almost all the men who have held or who hold office as examiners in obstetrics in the University of London and in the two Colleges are operators. This means that the University and the Colleges are obliged to accept operators. They have no choice. The same men are wanted in the hospital schools to teach. Is not the deduction clear? *Verbum sapientibus.*

I imagine that this sketch of the relations of medical, surgical, and obstetric practice in London will strike the professional mind in America and the Continent of Europe as somewhat remarkable. It is certainly exceptional. It offers a striking example of the evils resulting from the maintenance of two independent colleges, one medical, the other surgical. This splitting of the healing art into two divisions lends strength to the pernicious tendency now especially prevalent in this town to further subdivisions into so-called specialisms. This tendency is now so rooted in the popular mind that not only is the body regarded as a community of organs each of which is an independent constituent, and is therefore assigned to the care of a special doctor, but it has gone further still, so that now we have not only liver, kidney, and uterine doctors; but these are not secure in their nominal domains. One cannot have so much as an organ to one's

self. One kidney doctor, for example, is good for Bright's disease, and another for diabetes, and so with the rest. It seems not unlikely that there will be doctors for the right kidney, and doctors for the left.

The difficulties under which the obstetric physicians work in London are greater, I believe, than exist anywhere else. In the provinces, in Scotland, and in Ireland, their position is far more catholic and independent. It may be doubted whether the ardent spirit of Sir James Simpson, which led to such brilliant results, would not have been cramped, had his career been run in London. That career was essentially surgical. In no hospital in London could he have found scope for his genius, unless he had consented to divorce himself from obstetrics. And it is unnecessary to point out how intimately surgery and obstetrics were linked together in his work.

So, I believe, even the bold enterprise and the generous emulation of my American brethren, who have shed such lustre upon the medical history of your country, would have been crippled and chilled, had they been compelled to work under similar discouragement to that which afflicts us here.

Believe me yours truly,

ROBERT BARNES.

LISTERISM IN ABDOMINAL SURGERY.

BY

J. KNOWSLEY THORNTON,
London, England.

TO THE EDITOR OF THE AMERICAN JOURNAL OF OBSTETRICS.

SIR:—In the June number of your JOURNAL, pages 636–37, Mr. Tait, of Birmingham, publishes a letter in which he gives a mutilated extract from my address “On the Surgical Treatment of Uterine Fibro-Myomata,” read before the Liverpool meeting of the British Medical Association, and published in the *Journal*, October 13th, 1883. Mr. Tait has evidently copied my remarks from the *Journal*, but inaccurately says that they are from a *letter* written by me. He also makes a misleading reference to the results of the Samaritan Hospital practice, and gives an equally misleading comparison between my results and those of Dr. Bantock in hysterectomy. For the last purpose, he takes a

special group of Dr. Bantock's cases treated by Koeberlé's *serre-nœud*, and compares them with a small group of my cases treated chiefly by the intra-peritoneal method, and so treated because it was impossible to use the *serre-nœud*. In the paper I had the honor to read before the American Gynecological Society at Boston (*Transactions*, vol. 7), I stated at length my reasons for preferring to treat all cases in which it is possible by removal of the uterine appendages; all the cases, seventeen in number, which I have so treated have recovered, and the hysterectomies chosen for comparison by Mr. Tait were a specially serious group of cases unsuitable for the milder operation. So much for the fairness of his comparison.

In my Liverpool paper, I gave all the cases in which I have removed uterine tumors, thirty-one cases and ten deaths. Since that time, I have performed complete hysterectomy ten times, with two deaths, and in one of the fatal cases I also removed a large ovarian cyst and a cyst of the broad ligament. The other was an old woman in broken health, with a malignant tumor and ascites. Now, as to the omissions from the passage Mr. Tait honors me by quoting. I have italicized the words he left out, and it will be easily seen why he left them out, and how they alter the sense of the words "my large mortality," which is merely a comparative expression, with the almost perfect results I record in ovariectomy, and the perfect results in the removal of the uterine appendages.

"I attribute my large mortality in hysterectomy, *as compared with that of ovariectomy (under four per cent) and that of removal of the uterine appendages for fibro-myoma, hydro-salpinx, or pyo-salpinx, ovarian pain, etc. (nil)*, as entirely due to the fact that, in the majority of the former operations, it is impossible to carry out strict antiseptic or aseptic surgery." Then farther on I added: "I am also prepared to maintain that, while strict Listerism will give us perfect results in the removal of the appendages, it is to the perfecting of our antiseptic precautions that we must look for improved results in hysterectomy." My practical experience in both operations since the Liverpool meeting has demonstrated the absolute truth of this last paragraph.

Mr. Tait has, however, gone further than the mere comparison of results, and has insinuated against me a grave charge of attempting to represent the results of antiseptic and non-antiseptic abdominal section at the Samaritan Hospital as something very different from what they really are. I meet this charge by stating the facts of the cases, so far as they are known to me, both as regards Dr. Bantock's success in hysterectomy, and as to the

relative mortality in the antiseptic and non-antiseptic ovariotomy practice at the Samaritan Hospital during the time we have both worked there.

Mr. Tait goes to the *Transactions* of the Obstetrical Society of London for Dr. Bantock's results, but I will proceed to show that no one must go to those *Transactions* for this purpose, or he will get a false impression of Dr. Bantock's success in hysterec-tomy. On May 3d, 1882, Dr. Bantock showed two fibroids at the Obstetrical Society, and said "he had now had sixteen cases with only four deaths" (*Trans.*, vol. xxiv., p. 91). On December 6th, 1882, he showed five more and reported a fifth fatal case, but made no mention of a sixth fatal case which will be found recorded in the table in his Worcester paper (*British Medical Journal*, August 26th, 1882) (*Trans.*, vol. xxiv., p. 301). On March 7th, 1883, he showed another five specimens, and referred to those shown three months before, as if he was reporting a complete and continuous series, but again forgot to mention a fatal case, making the seventh death in his series. It was at this meeting that he spoke of the group of cases which Mr. Tait has so fairly selected for comparison with some of mine (*Trans.*, vol. xxv., p. 38). On May 7th of this year, he again brought forward four successful cases, but omitted all notice of four fatal cases which died in rapid succession after his cases were shown in March, 1883. All this goes to show that, in a serious operation of this kind, long and complete records are necessary before any correct opinion can be formed as to its success, and it also shows that, for such records of Dr. Bantock's results, Mr. Tait must not go to the *Transactions* of the Obstetrical Society.

Now for ovariotomy. I performed my first ovariotomy at the Samaritan Hospital in November, 1874, and Dr. Bantock followed with his first hospital case in January, 1875. We have both continued to operate there up to the present time, a period of nearly ten years, quite long enough to make our results good for comparison, when adopting distinctly different methods. Ever since Dr. Bantock attacked Listerism in a paper read before the Medico-Chirurgical Society "On Hyperpyrexia after Listerian Ovariotomy," I have kept a very close watch upon our relative results, as I felt that a very important mass of evidence was accumulating, either for or against Listerism in abdominal surgery. Where could the conditions be more equal? Two surgeons with similar accommodation and with an experience extending over an equal period of time, working under the same roof!

In the volume of the *Transactions* of the Med.-Chir. Society, in which Dr. Bantock published the above paper, with a table of 162 complete ovariectomies appended, I also published 150 antiseptic ovariectomies, which, together with some non-antiseptic cases and 25 published in a previous volume, gave 188 complete ovariectomies.

If the two tables are carefully compared, the following results are found:

	Cases	Died	Mortality
Thornton, non-antiseptic hospital ovariectomies,	33	5	15.15 per cent
Bantock, " " " "	36	8	22.22 " "
Thornton, 'Listerian' Hospital ovariectomies,	129	15	11.62 " "
Bantock, " " " "	113	16	14.15 " "

In whatever way the cases are grouped, my results are from 3 to 11 per cent better than Dr. Bantock's. But here is a most remarkable fact—Dr. Bantock's 'Listerian' mortality, which the paper was written to show up, *is actually 8 per cent lower than that of his non-antiseptic cases.*

In face of these results, he throws up 'Listerism' and uses what it is the fashion to call 'cleanly surgery,' *i. e.*, a great deal of ordinary tap water, which is known to teem with organisms.

I have steadily stuck to "Listerism," and against my 11.62 per cent antiseptic mortality in the above comparison, I now have to record 174 additional complete ovariectomies, with another 10 deaths, or a mortality of *only 5.74 per cent.* Dr. Bantock has continued "cleanly surgery" and, with much use of the glass drainage tube, has to show 92 additional complete ovariectomies, with another 16 deaths, or a *mortality of 17.38 per cent.*: more than *three times* my antiseptic mortality, and actually *3½ per cent worse than his own 'Listerian' mortality.* And in this statement of the case, I have left out three of his ovariectomy deaths which happened within a month of operation, and before the patients left the hospital, as he says that they recovered from the operation and died of something quite independent of it. I have also left out incomplete ovariectomies in which results are still more heavily in my favor.

If Mr. Tait had remained silent, this statement might have never come out, though I have often thought that the misrepresentation on this subject had continued a little too loud and too long. I think both the surgeon and the method have reason to say: "Save me from my friend."

But Mr. Tait is a great opponent of "Listerism," and I find, on referring to his published table, that his results stand thus:

	Cases	Died	Mortality
"Strictly Listerian," his own statement,	50	3	6 per cent
Non-antiseptic,	176	24	13.95 " "

These figures need no comment from me.

What Mr. Tait's recent results may be I know not, for we have not been favored with them lately; but their non-appearance is suggestive, as Mr. Tait does not usually hide his "light under a bushel."

I now challenge him to publish a complete series of his non-antiseptic cases of ovariectomy *for tumor*, which will compare with my 303 antiseptic cases given above, and until he does this, I shall take no notice of anything else he may write on the subject.

Mr. Tait's results in hysterectomy have been remarkable; in a series of 50 abdominal sections (various) published in the *Birmingham Medical Review*, vol. x., p. 30, he records 9 cases with 5 deaths, and there are also 5 incomplete fatal cases.

I am, sir, faithfully yours, J. KNOWSLEY THORNTON.

22 PORTMAN STREET,
LONDON, July 16th, 1884.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF NEW YORK.

Meeting, February 5th, 1884.

DR. H. J. GARRIGUES read a paper on

MISSED ABORTION; SCLEROSIS OF THE PLACENTA.

Mrs. M. F., æt. thirty. married ten years, in her sixth pregnancy, presented herself for treatment, having had daily slight hemorrhage for four weeks. The size of the womb corresponded to the fourth month. No sounds being audible, pregnancy, with death of the fetus, was diagnosticated. About a week later, the bleeding had ceased, and the patient felt well.

About nine months after her last menstruation, a fetus and placenta, in one mass, were expelled, with moderate pain; they were perfectly fresh. The fetus, a female, seemed to be at the end of the fourth month, with partial arrest of development. The mouth was large, the lips only partially formed, hare-lip and cleft palate existed. The epidermis had come off to a great extent, forming casts of the hands and feet. The soft parts appeared much atrophied.

The placenta measured eight by six cm. and appeared mainly hard, whitish, and lardaceous, without cotyledons. Its fetal sur-

face was normal. The cord, twenty cm. in length, had a velamentous insertion. Of the membranes, only some pieces of gelatinous tissue remained, and of the decidua only a few long shreds were found.

The death of the fetus was due to the sclerosis of the placenta which is supposed to be of syphilitic origin.

A dead fetus undergoes either putrefaction or mummification. In the former case, it causes fever, and if not expelled, must be removed; in the latter case, as in that under consideration, it may be retained for many months.

The wasting of the soft parts should be attributed, not so much to absorption of the liquids, as to starvation, inasmuch as scarcely any placental tissue was left. A similar condition is found in adults deprived of food.

Missed abortion is of some practical interest. Where the fetus is decomposed, its removal is indicated; but no treatment is called for in case of mummification. As the diagnosis will generally be doubtful, inasmuch as the uterus might contain a living fetus, it is advisable to abstain from active measures.

The possibility of this condition occurring should be borne in mind, so as to avert unjust suspicion, *e. g.*, when a married woman, in the absence of her husband, expels a fetus corresponding to a development of shorter duration than the father's absence.

The woman may require treatment for uterine disease, or perhaps both she and her husband, for syphilis.

TWO CASES OF OÖPHORECTOMY.

In the absence of the author, Dr. B. F. DAWSON, Dr. C. C. Lee had been requested to read for him the histories of two cases of oöphorectomy, and to present the specimens:

CASE I.—Mrs. F., aged thirty, for two years a widow, with no children, had had one miscarriage at the third month, occurring after a strain, about two years ago. She had been well up to that time, but soon afterward began to suffer from severe neuralgic pains in the pelvis and from dysmenorrhea. These symptoms gradually increased in severity, until she consulted Dr. Dawson, in December, 1883. He found the right ovary enlarged, tender, and prolapsed, and apparently fixed in the *cul-de-sac*. Oöphorectomy was advised and was performed on January 19th, 1884. An incision two inches and a half long was made, and the right ovary was brought into the wound with considerable difficulty. The tube and the ovary were ligated and removed with the cautery. Some hemorrhage from a small rent in the pedicle subsequently rendered enlargement of the incision necessary, when the pedicle was again ligated and cauterized. The left ovary and tube, with a small cyst of the broad ligament, were found directly under the abdominal wall, and were removed without difficulty. The patient made a very satisfactory recovery. The highest temperature—103° F.—occurred on the third day and readily yielded to the

abdominal coil. Particular attention was paid to the "peritoneal toilette."

CASE II.—Mrs. E. T., aged thirty-seven, married eight years, was the mother of two children, the last one born five months since. The labor was complicated by post-partum hemorrhage. Dr. W. A. Hammond and Dr. C. H. Brown were consulted by her husband, with the history that she had attempted suicide, eight years before, and that symptoms of melancholia had developed. When she was seen, her gait was stooping; the brows were strongly wrinkled; she had no hallucinations. She cried a great deal, would not answer questions, but often complained of pains in the head, breast, and pelvis. She was referred to Dr. Dawson, who discovered ovarian tenderness and prolapse of the right ovary into the *cul-de-sac*, and advised oöphorectomy, which was performed on January 21st, 1884. An incision two inches and a half in length was made, and the ovaries were removed without difficulty. Recovery was rapid. At the present time, the mental condition was improved; she cried less, and the brow was smooth.

Both patients were operated upon at their houses. The spray was used in the room, but not over the abdomen. As soon as the peritoneum was opened, the edges of the wound were protected by carbolized napkins, to prevent any access of blood to the peritoneal cavity, and extreme cleanliness in every detail was practised.

DR. LEE remarked that the condition which was found to exist in the first case, and which looked very much like a hydro-salpinx, certainly justified the operation, for it soon would have developed into a large cyst. The rapid recovery of the patient seemed further to have warranted the early operation, although a sufficient length of time had not yet elapsed to enable one to say that the recovery would remain permanent. With reference to diagnosis in these cases, when the abdominal walls were thin, we were sometimes able to determine the nature of the tumor with a good deal of certainty, but in other cases it was not possible to do so. Within the past year, two of his friends in this city had opened the abdomen, to find, instead of disease of the tubes or ovaries, uterine fibroids. In each instance the wound was closed and the patient, happily, recovered.

DR. POLK inferred that Dr. Lee did not consider that in cases of this kind we were justified in opening the abdomen merely for purposes of diagnosis.

DR. LEE thought that, if the case was a severe and a grave one, destroying the health of the patient, and there was no other way to account for the apparent ovarian disease, the operation was justifiable. He believed, however, that we were in danger of going to extremes, and the two cases which had occurred in the practice of his friends had taught him, in cases of small pelvic tumors the exact nature of which could not be determined, not to operate unless he felt quite sure that the tumor was the cause of the patient's illness.

DR. POLK, in this connection, mentioned a case and asked the members if they could offer an explanation. A woman came to him, having had a tumor in the pelvis for years, which he found to be easily recognizable, and which, by its presence, had caused

partial prolapsus of the uterus, with all the accompanying disagreeable symptoms of that condition. The patient assumed the knee-chest position, night and morning, and was kept at rest for two weeks, and at the end of this time the mass was found to be diminished to two-thirds of its original size. The diminution in size took place without any discharge.

DR. LEE inquired of Dr. Polk whether he felt sure that the mass was a tumor in the sense of being a neoplasm, or whether it might not have been a mass of firmly-impacted lymph exudation, which was known often to assume the form of a tumor, and under certain conditions to undergo rapid absorption.

DR. POLK said that such an explanation had offered itself to his mind, but he found the original mass very movable, so that, when the patient assumed the knee-chest position, the tumor repeatedly rose above the pelvic brim, which, of course, it would not have done, had it resulted from pelvic cellulitis.

DR. H. F. WALKER inquired whether the tumor was solid or fluid.

DR. POLK replied that it was distinctly fluctuating.

DR. WALKER said that uterine fibroids sometimes became engorged and gave a sense of fluctuation, while, perhaps a week or two later, they would be found shrunken and much reduced in size.

DR. POLK remarked that in such cases the tumor existed reasonably near the uterus, whereas in the one mentioned by him this distance was markedly increased in the knee-chest position. It had occurred to him that the case might have been one of hydrosalpinx which discharged a part of its contents into the uterus; but the patient gave no history of any unusual discharge.

DR. LEE said the second case reported by Dr. Dawson reminded him of a pamphlet in which Dr. Robert Battey, of Rome, Ga., described two cases of supposed insanity, due to ovarian irritation, which were cured by oöphorectomy, and he emphasized the fact that several months passed by before the patients showed decided improvement. He ascribed this to the fact that, when the nervous system had been so long affected as to develop decided symptoms of insanity, it required some time to regain its normal state. Probably some of the members of the Society had had a similar experience, and had also seen cases bordering on insanity in which the symptoms finally disappeared without operative interference.

DR. W. M. CHAMBERLAIN said that a lady of this city had been under his care a long time to whom he proposed oöphorectomy, and she afterward consulted other gentlemen who also regarded the operation as necessary and inevitable. The patient became suicidal and homicidal, and it was necessary to remove her to an asylum. The operation, however, was not done. She remained in the hospital six months and was then returned to her family, not cured, but she had since become perfectly well and returned to her former position in society. Dr. Chamberlain was unable in this case to make a positive diagnosis of pyo-salpinx, but he did not think he had ever seen a woman suffer more intense and protracted ovarian neuralgia than she did, or a neuralgia less amenable to treatment.

DR. POLK said this question had been before the Society on several occasions. Dr. Gillette had reported a case bearing on the subject. He did not think the question could be considered in any way settled. There were many cases of melancholia in which there was also undoubted uterine disease, and yet the patients got well of the mental trouble without the uterine or ovarian dis-

ease being cured. Interference with menstruation was one of the commonest incidents in mental disturbance, and it might appear to have special connection with ovarian trouble, when in reality that need not be the case. He now had a patient with prolapse of the ovary and retroversion of the uterus who three years ago shot herself through the lower border of the pericardium, the ball passing through the left lung and lodging near the spine. She recovered from the severe pleurisy, pneumonia, and pericarditis which followed, and was to-day as beautiful a woman as he knew, and in perfect mental condition; yet the uterine and ovarian displacements remained.

DR. LOUIS A. RODENSTEIN read a paper on

INTRAUTERINE INJECTIONS IN PUERPERAL SEPTICEMIA.

He did not desire to enter on the discussion of the use of vaginal or intrauterine injections, but merely to put on record five cases in which the injections proved of value and would serve to support the theory of the septic origin of the disease.

CASE I.—Forceps delivery. One hour after labor, severe hemorrhage; the uterus was emptied of clots by the introduction of the hand; ergot and morphine given. Three days later, chill and high fever; lochia had stopped. Gave $2\frac{1}{2}\%$ carbolyzed vaginal douches morning and evening; quinine. This treatment proved ineffectual; tympanites set in; quinine stopped; ordered poultices and opium. After consultation with Dr. Hunter, intrauterine injections of the same strength were substituted, with prompt amelioration of the symptoms. The temperature, however, continued high in spite of the coil and other measures, until the curette was employed and some decomposing material removed. The patient eventually made a good recovery.

CASE II.—Was called on the seventh day after a normal delivery, and was told the patient had had a chill three days previous. Fever, delirium, and anorexia were present. Intrauterine injections of the same strength as in the former case promptly reduced the fever; but the patient was not properly cared for by her attendants, who gave her neither food nor stimulants, and allowed her to die of exhaustion. But the good effect of the injections on the fever was unmistakable.

CASE III.—On the fourth day after delivery, the patient had severe chill and rise of temperature, with delirium and other untoward symptoms. Uterus washed out every six hours; after forty-eight hours, temperature had fallen from 105.4° to 101° , and in twenty-four hours more sank to normal. Recovery was prompt.

CASE IV.—Tenth confinement; triplets, still-born, all males; three distinct placentæ. On the third night, patient had a severe chill, with fever and delirium. The uterus was washed out three times a day with carbolic solution: gave quinine, gr. v. every six hours; milk, beef-tea, and brandy as much as the stomach could bear. The patient made a good recovery.

CASE V.—Primipara: forceps delivery; slight perineal lacera-

tion. On the morning of the fourth day, a chill occurred, together with every symptom of septic poisoning. At once gave intrauterine injections and applied antiseptics to the laceration which was too slight to require operation. Quinine, gr. v. every six hours. Result good. The Chamberlain glass tube was used for the injections in these cases.

Dr. Rodenstein wished to add one point which he had found in his experience, that where there is septic poisoning the os uteri is invariably sufficiently open to readily admit the finger, and remains open until the poison is eliminated, no matter how many days it may be: while, on the other hand, if the chill, rise of temperature, and other symptoms that might appear to be those of puerperal septicemia, are of a malarial origin, the uterus is contracted and the os closed, as they should be at that period of the puerperal state.

DR. CHAMBERLAIN said he was glad to hear one of the concluding sentences of Dr. Rodenstein's paper, to the effect that the septic uterus was a relaxed uterus, and presented a wide, or, at least, a very distensible os. Such had been his observation, and he had drawn the same conclusion which the author had drawn in a case seen in consultation the present week. He was called in consultation about the middle of the third day after labor. The patient had had a rise of temperature and a slight chill, but Dr. Chamberlain found the uterus firmly contracted, as hard as a billiard ball; and, in accordance with previous observation, that the septic uterus was a relaxed uterus, he concluded that it was not a case of septic womb, and the final result had proved that it was not. On that fact was based a point in the construction of the tube which went by his name, and which had been criticised, namely, the large size of the instrument. Believing that the septic uterus was always relaxed, and the os patulous, or at least so in the early stage, he believed that the large-sized tube was the proper instrument, and the first were so made, but those made a little later were only three-eighths of an inch in diameter. He, however, had never had any trouble in introducing the large tube, and had found that plenty of room remained at the sides to allow of the free escape of the fluid.

DR. GARRIGUES believed that we had come to a point where all of these cases of puerperal septicemia could with a great degree of certainty be avoided. Since he had introduced the treatment by bichloride of mercury, described in a paper read before the County Medical Society, December 21st, 1883, he had had one hundred and forty confinements in the wards at the Maternity Hospital, which certainly had been considered one of the worst localities for confinement to take place in, one out of every four patients having been seriously ill during his previous term of service and many having died: yet, out of the one hundred and forty cases treated there since the introduction of the method mentioned, there had not been a single case of serious illness. There was one point to which he would call particular attention, as it had been mentioned in the paper: that was with reference to leaving anything within the uterus after delivery. Excellent authorities, for instance, Credé, taught that it was not necessary to remove pieces of membrane. Dr. Garrigues, however, could not share such a view,

because his personal experience was opposed to it. He had seen both very dangerous hemorrhage and puerperal fever arise from leaving a part of the membranes. He believed that, with the perfected antiseptic measures which we now possessed, the danger of introducing the fingers, or even the whole hand, into the uterus was much smaller than the one to which we exposed the woman by leaving a part of the placenta, or only a piece of the membranes, in the uterus. He would add that he agreed fully with Dr. Rodenstein as to the value of intrauterine injections when the disease was once there, but he thought we had a drug which ought to be preferred to carbolic acid. He had used carbolic acid for ten years, but had now given it up and substituted bichloride of mercury, and he believed that there was no comparison as to the relative value of the two drugs. This had been proved experimentally and by clinical observation. The enormous difference in the results obtained at such places as the Maternity Hospital, first under the use of carbolic acid, and at present under the use of bichloride of mercury, proved the great superiority of the latter antiseptic.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF PHILADELPHIA.

Meeting, June 6th, 1884, Continued.

DR. B. F. BAER exhibited a specimen of

SUBMUCOUS AND INTERSTITIAL UTERINE FIBROID,

and read the following report of the case. Mrs. G., æt. fifty-one years, married, four children. Her first child was born after she had reached her thirtieth year, and the last one when she was thirty-eight, thirteen years ago. Her labors were very severe. Several years ago she began to lose large quantities of blood with her catamenial periods, and to suffer severely with labor-like pains. This continued with increasing quantity and severity until the cause was removed. Two years ago she had so great a flooding that it was thought for weeks that she would succumb. She gradually recovered from the immediate effects of the hemorrhage, but has been losing flesh and strength ever since. Moreover, as she would recover a sufficient amount of strength to support it, a severe metrorrhagia would recur. Recently she had rarely been free from bleeding and a profuse non-fetid watery discharge.

At the request of my friend, Dr. Fred. C. Seiberling, the family physician, I met him at the home of the patient, at New Tripoli, Lehigh Co., Pa., when I saw her for the first time. She had a sallow, jaundiced, and anemic-looking surface, was somewhat emaciated, but did not have the characteristic cachectic appearance of

carcinoma. Her pulse was 120 and not strong. As she was too weak and excited to permit of a thorough examination without it, ether was at once administered by Dr. J. W. Seiberling, when the following condition was revealed by physical exploration. The upper portion of the vagina was occupied and distended by the cervix uteri, which was stretched and expanded over a firm rounded mass which presented at the os; the latter was dilated to about the size of a silver dollar, and the body which presented resembled very much in size and shape the head of a seven months' fetus. Bi-manual examination showed the uterus to be as large as at the end of the sixth month of gestation, and rather symmetrically developed, but it was quite firm and resisting, if not hard. The sound could not be introduced for the following reason. On the right side, the tumor was firmly attached to the cervical wall all the way to the external os, and on the left almost to the same extent, though here I was able to pass the sound and finger to a depth of about an inch. I confirmed Dr. Seiberling's diagnosis of submucous fibroid, and thought, from the size of the uterine globe as outlined by abdominal palpation, that the tumor had also a deep interstitial attachment or *nidus*. I advised an attempt at enucleation of the growth as the only means of saving the life of the patient, and with the assistance of Drs. F. C. Seiberling and W. K. Kistler, I proceeded with the operation. I was at a loss at first to explain the close connection which the tumor had with the cervical walls, a connection so intimate that it seemed in part to grow from that organ, but the history of the growth of the tumor was against that idea; the patient had been under close observation at least four years, during most of which time the cervix was free from disease. There had been no evidence of abnormal growth in the cervix until six months before the date of operation; about that time the tumor was detected at the internal os. I thought that the close connection with the cervical wall might have been due to adhesive inflammation, and acting on that theory, I began my operation by trying to separate the adhesions, but I soon found that I had made a mistake, and that I was dissecting up the tissues of the cervical wall. I then concluded it must be the capsule of the tumor, which had been stripped off by the growth as it was made to advance towards the external os by contractions of the uterus, and this view was confirmed, for when I had removed the tumor, the capsule hung as a curtain from the edge of the external os. After vainly endeavoring to deliver the tumor by traction, I cut directly into it with scissors, dividing the mass into two portions. I then introduced Thomas' spoon-saw into the cavity, and with it and my fingers, I tore the growth from its nest in the fundus and sides of the uterus, removing it piecemeal. Very little hemorrhage occurred during the operation, which occupied fully an hour and a half. After trimming the ragged edges of the capsule from around the os, I injected the cavity with vinegar and packed it full of sponges saturated with the same excellent anti-

septic and hemostatic. A gr. ss. morphia suppository was placed in the rectum, the patient removed to her bed, and surrounded by bottles of hot water. She reacted nicely, and in the evening ten grains of quinine were given. On the next day, the sponges were removed, the cavity irrigated with carbolyzed water and re-packed as before. Under the care of Dr. Seiberling, the patient has recovered without an untoward symptom. The tumor probably weighed three pounds.

TRANSACTIONS OF THE WASHINGTON OBSTETRICAL AND GYNECOLOGI- CAL SOCIETY.

Stated Meeting, May 2d, 1884.

DR. S. C. BUSEY, *President, in the Chair.*

DR. J. FORD THOMPSON reported

TWO CASES OF CONGENITAL OCCLUSION OF THE VAGINA.

I. In the first case the patient was brought to Dr. Thompson's office by her mother. She was twelve and a half years of age. The mother presented a letter from Dr. Wells, her family physician, in which he says: "I would be glad to have you examine this girl and see what should be done with her. I was called to see her, say a month ago; found distress and pain about pelvic region; tenesmus of rectum and bladder; urine turbid and scanty. Ordered hot bath, rest, and anodynes. In a day or two she was about and at school. Was sent for again a few days ago; trouble much aggravated and vesical symptoms urgent; passed catheter yesterday, drawing off small quantity of turbid urine. More careful inquiry yesterday disclosed the fact that these pains and pelvic disturbances have been coming on monthly for four or five months, but not entirely leaving during the interval, leading me to believe the menstrual molimina caused the symptoms, except for their great and increasing intensity. A casual digital examination makes me send her to you for such exploration as you may find she requires. This difficulty of evacuating the bladder seems to have been a constant accompaniment during the past few months."

From the mother I learned that her daughter's trouble began a year ago; and that, at first, it was only noticed monthly, for which reason she gave no more thought to it than these symptoms of commencing menstruation usually require. In a few months,

however, her sufferings were not only more severe at these periods, but she was never entirely free from suffering in the interval. She became constipated, and her efforts at defecation were painful: but her greatest pain and distress were in micturating, which caused her to hold her water as long as possible, and when compelled to empty the bladder, she would press upon the vulva with force before the urine would flow. She was rather a healthy girl in appearance, and fairly developed.

Upon examination there were evidences of puberty in the external genitals, and nothing unnatural in the appearance of the parts. There was a fulness of the supra-pubic region as if from a distended bladder, and some tenderness. Upon examination with the finger no vaginal opening was found. The vaginal outlet was entirely closed by a thick membrane, which bulged somewhat forward and through which fluctuation was detected. In other respects the vulva was normal. The bladder was emptied by catheter, and the finger then passed into the rectum. The pelvis appeared to be filled by a distended vagina. After explaining the nature of the case to the mother, he advised an early operation, which he performed at his office, Dr. Wells assisting. No anesthetic was administered. With a small-bladed knife a puncture was made in the centre of the membrane, and a thick, black-looking fluid appeared. The incision was then enlarged, giving vent to an enormous quantity of thick, tarry-looking fluid, with clots of blood. The finger was then introduced and the membrane torn to some extent, but finding it very resisting, the scissors was used to make the opening of the desired dimensions. Twenty ounces of fluid were collected, after which the vagina was syringed with warm water until it returned clear. The opening was then filled with absorbent cotton, and a large compress and T-bandage applied. The patient stood the operation remarkably well, complaining but little of after-pain, and there was no nausea or faintness. There was more pain during the operation than was anticipated, which made its completion more difficult than it would otherwise have been. The patient was not seen again by Dr. Thompson for two weeks. She had been sick and faint on her way home after the operation, but after being put to bed became comfortable, and had been improving ever since. On examination, the index finger was found to pass, but not easily, through the opening, which felt like a dense ring, and may yet cause trouble in the exercise of another important function. There has been no menstrual period since the operation, but confidence is felt that there will be no further trouble in that respect.

Atresia hymenalis is not a common malformation. Emmet (first edit.) reports only four cases, and that above reported is the only one seen by Dr. Thompson. In Emmet's cases the amount of fluid removed averaged six ounces, had been collecting for a year or two, and it was thought the uterus was distended, more or less, in all. This point was not determined in Dr. Thompson's case.

although it is probable that such was the fact. The symptoms are pretty much the same in all cases, and it is clearly the duty of the physician to make a careful examination of the genitalia in all cases of delayed menstruation, as it may happen that by a very simple operation much trouble may be avoided and future ill health prevented. The operation is very simple. It used to be considered dangerous to remove all the fluid at once, but such is not the case now. Glass dilators are probably the best for after-treatment, but the cotton acted well in the case reported.

II. The second case reported was seen three years ago. The lady, a patient of Dr. C. E. Hagner, was eighteen years of age. Dr. Thompson met Drs. Hagner and Busey in consultation. The lady had consulted Dr. Hagner on account of the absence of the menses and periodical bleeding from the nose and mouth. No trouble in the genital region was complained of. Dr. Hagner found occlusion of the vagina to exist. On examination, Dr. Thompson found nearly entire absence or atresia of the vagina. Fluctuation was distinctly felt through the rectum in the uterine region. An operation having been determined upon, a transverse incision was made at the proper place, a catheter being held in the bladder and the left index finger in the rectum. After the tissues had been well cut, the operation was continued by the finger and the handle of the knife until a dense membrane forming the wall of the cavity containing the fluid was reached. When this was opened, a considerable quantity of thick, glairy fluid was discharged. The sac was washed out and the incision into it enlarged, and the new canal stretched to the extent deemed necessary. No uterus could be found, and it was thought that it existed only in a rudimentary state. The fluid removed resembled the contents of a cystic tumor. The patient remained under Dr. Hagner's care for a long time, during which he persistently used dilatation, but the opening contracted very much.

This belongs to a very interesting class of cases. The diagnosis is not difficult when attention has been directed to the genital organs. It is readily distinguished from mere closure of the vaginal orifice in which fluctuation is readily distinguished in the distended vagina, whereas in cases of atresia of the whole canal a condensed cord-like tissue is felt. An undeveloped uterus is usually found in the latter class of cases, and this suggests an early operation as conducing to the development of that organ at the advent of puberty. The operation for the formation of a vagina has been followed by complete restoration to health in cases where menstruation did not afterwards appear. It has happened that girls so afflicted have married, and that copulation was effected through the urethra without either party being conscious that an abnormal condition existed.

This fact suggests the interesting question, how far it is the duty of the surgeon to endeavor to perfect the vagina in case of absence of the uterus, but with active ovaries, for the mere pur-

pose of animal gratification. In our own sex we would be in favor of constructing an organ for such a purpose regardless of the power of procreation, and no reason appears why the treatment should not be extended to the female. Women so affected, unfortunately perhaps, are subject, more or less, to all the tender emotions of their sex, and are made just as unhappy without their gratification. The operation itself, unlike that for imperforate hymen, is by no means a simple one. In fact it is attended by considerable danger in its performance and immediate results, besides the great difficulty of maintaining an opening or canal sufficiently capacious for practical purposes. To obviate the latter difficulty to some extent, the new canal should be made of over size at first.

DR. C. E. HAGNER examined his patient yesterday, and was able to pass the finger one inch into the vagina. Rectal examination made him confident of the existence of a rudimentary uterus. The menses had never appeared, yet the bleeding from the nose and mouth, recurring periodically, indicated that the ovaries were active.

DR. J. T. JOHNSON, referring to the question raised by Dr. Thompson as to the propriety of operating on cases where there was absence of the uterus and vagina, merely for the purpose of enabling the woman to have sexual intercourse, thought it was not proper to subject a woman to so much risk when the parts presented a cord-like condition. Heretofore the operation had been dangerous, and looking at the list, he found that the older authors held that all the cases died.

Emmet, however, reported twenty-two successful cases, some of them leading to cellulitis. If the woman was anxious and willing to run the risk of the operation for the sake of domestic peace, it might be proper to operate. The completion of the operation for imperforate hymen at one sitting was a great improvement. Emmet claimed it as such, and held that his success was due to following that course and thoroughly washing out the uterus. Emmet also laid stress on the use of glass dilators as means of securing permanent success. On the other hand, Routh, in his operation, went more slowly, taking seven days and dilating little by little, using tents for the purpose. These cases died, and Emmet held that this method was the very one to kill the patients by inviting blood poisoning. The danger of retained menses was from the escape of the fluid through the Fallopian tubes leading to blood poisoning or peritonitis; and Barnes suggested that there might be an oozing through the uterine walls from pressure, the blood appearing beneath the peritoneum. Dr. Johnson raised the question of the obstetric management of cases where the occlusion had occurred after pregnancy had taken place, or where the semen had entered through a chink in a nearly imperforate hymen. Taylor, of New York, had asked whether it was better to operate during pregnancy or wait until labor set in, and concluded that the latter course was best, inasmuch as the softening process of pregnancy might be able to overcome the obstruction, while interference during pregnancy would be likely to result in miscarriage.

DR. KLEINSCHMIDT suggested that some of the so-called congenital

cases of atresia of the vagina might be produced by vaginitis and vulvitis in early infancy. He had lately seen two cases of this kind. Both of the infants had normal vaginae when born. In one he restored the passage by introducing a probe immediately below the meatus urinarius and splitting the parts down to the fourchette. In the other case the parents refused, as yet, their consent to operative interference.

DR. MAGRUDER said he had also noticed inflammatory adhesions of the vaginal walls in children from two to three years old. In some there was only a partial union of the labia, in others the adhesions were so firm and extensive as to require cutting. In two cases seen at the Dispensary, the mothers stated that the children had difficulty in urinating, and adhesions were found which he had to overcome with the knife.

DR. HARRISON stated that the discharge in Dr. Thompson's second case contained cholesterine, but nothing else of special interest.

DR. MCARDLE held that, in spite of the gloomy picture drawn as to the risks of the operation, we were justified in performing it if the woman so desired, for it was safe enough under modern methods of surgery. He thought, however, that Dr. Thompson should not have operated in his office, but at the patient's home and under ether. Glass dilators should also have been employed.

DR. TYLER narrated the history of a case of imperforate hymen presented in Thomas' Clinic. The woman was eighteen years of age and married. For several weeks after marriage all efforts at sexual intercourse had proved ineffectual, and dark clouds began to gather upon the matrimonial sky. Dr. Thomas demonstrated the case before the class, and then under ether rectified the trouble by slitting the curtain obstructing the entrance.

DR. JOHNSON referred to the necessity of removing the ovaries where there was absence of the uterus and vagina.

DR. THOMPSON, in closing the discussion, said that neither of the cases was under his care after the operation. In one, Dr. Hagner had faithfully used the dilator, and yet contraction followed. In the other case, the child being but twelve years old, the opening was large enough for the present, and if necessary, the hymen could be slit open. He thought the vaginal operation more dangerous in early life when there was no collection of menstrual fluid to guide the operator.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF LONDON.

Meeting, Wednesday, June 4th, 1884.

HENRY GERVIS, M.D., F.R.C.P., *President, in the Chair.*

The following specimens were shown:

Prolapsed uterus, removed with a fatal result by Dr. Malins; *ovarian cyst*, which ruptured into the abdominal cavity, the fluid being afterwards partially discharged through an ulcerated um-

bilical hernia, by Dr. Walter, of Manchester; *Meyer's dilators* for any cavity, by Dr. Aveling; *a specimen of spondylolisthesis*, by Dr. Graily Hewitt and Mr. Shattock; *a drawing of chronic papillary inflammation of the vulva*, by Dr. Priestley; *an ovary showing commencing cystic disease*, by Dr. Barnes; *fundus uteri removed for inversion* of four years' duration, by Dr. Mansell-Moullin; *placenta from a case of triplets*, by Horrocks; *Ayres' electric speculum*, by Dr. Heywood Smith.

SPONTANEOUS ABSORPTION AND RECOVERY AFTER PULMONARY THROMBOSIS.

By W. S. PLAYFAIR, M.D.—In this paper the author records a case of serious illness and recovery following a protracted and difficult labor. He analyzes the symptoms and attempts to show that they could only be explained on the hypothesis of a thrombus deposited in the pulmonary artery becoming spontaneously absorbed. He points out that the possibility of recovery under such conditions has not been sufficiently recognized. A second case is also recorded in the paper.

DR. BROADBENT had seen the second of Dr. Playfair's cases; there were no pelvic signs, but there were the following signs connected with the chest: one or two paroxysms of dyspnea, a peculiar murmur over the right auricle, followed by the signs of pulmonary embolism; the auricular murmur was replaced by a systolic pulmonary murmur and obscuration of the pulmonary second sound. The left post. tibial vein had been inflamed, and the right iliac vein afterwards became obstructed. Recovery was complete, except that the right leg was still swollen.

DR. GALABIN thought that, in view of the facts of pathology, Dr. Playfair had in his earlier writings attributed too much to thrombosis. In the case related by Dr. Broadbent, he considered that the clot, first formed in the right auricle, had become detached, and formed an embolus in the pulmonary artery.

DR. BARNES referred to a former contribution to the Obstetrical Transactions, showing that, in many cases, embolism followed thrombosis. He was sure he had seen cases recover from pulmonary embolism.

DR. WILTSHIRE had seen a case of recovery after puerperal pulmonary thrombosis. He agreed with Dr. Playfair's diagnosis of the second case, but thought the first case was one of ulcerative endocarditis.

DR. BROADBENT replied to Dr. Galabin that he did not think there had been embolism of the trunk of the pulmonary artery, but that the clot had extended into it from the auricle, and an embolus detached from it had settled in the left lung. An embolic clot if free would not lodge in the pulmonary artery. He thought clots were removed rather by disintegration than absorption.

DR. PLAYFAIR in reply stated that many of the objections had already been answered elsewhere, that thrombosis occurred within a few days after delivery, embolism not till the lapse of two or three weeks, a period required for the softening and detachment of the clot. He did not believe that either case was due to ulcerative endocarditis, the murmur must continue as long as the clot remained.

ON FETAL REVOLUTIONS.

By J. MATTHEWS DUNCAN, M.D.—The author regards revolutions, as distinct from rotation, as having been too much neglected in studying the mechanism of delivery. He shows the difficulty introduced by the peculiar curvature of the genital passages, which is nearer a parabola than a circle (circle of Carus). He shows that special mechanisms, as of the delivery of the head, extension, flexion, are imperfectly described and misunderstood, because not studied as parts of the change of fetal attitude necessitated by the revolution. Revolution generally involves extension of the whole fetal body. The various forms of revolution observed in different presentations and conditions of the fetus are then described.

DR. GALABIN criticised the accuracy of Dr. Duncan's description on the principles of geometry, and objected to the fetus being likened to a viscous mass; although it was plastic to some extent, it came mechanically under the head of rigid bodies. Version illustrated this.

DR. CHAMPNEYS pointed out the importance of accurate knowledge of mechanics in practice, for instance in the delivery of the head. The head is born by a movement of extension with advance; if the advance is forgotten and extension artificially produced, the larger fronto-occipital instead of the smaller suboccipito-frontal circumference distends the vulva, and the perineum is unduly stretched; he was convinced that laceration often occurred from this cause. The words revolution and rotation were familiar to all and were most useful for teaching purposes, a wheel rotates round its axletree, the moon revolves round the earth and also rotates. Viscosity could hardly be denied in the face of the various forms of expression of more mobile parts or their retardation, and the fetus could not therefore be regarded simply as a series of rigid levers.

DR. MATTHEWS DUNCAN, in reply, indorsed Dr. Champneys' remarks, and stated that the movements as described by him had been so described by all previous good observers, and that his descriptions were not innovations, as Dr. Galabin implied.

A report of the committee on Dr. Neugebauer's specimens of spondylolisthesis was read.

They consider that the specimens bear out Dr. Neugebauer's views, which are essentially that the anterior half of the vertebra (typically the fifth lumbar) glides away from the posterior half owing to congenital deficiency or fracture. Such predisposing causes may not result in spondylolisthesis without the operation of overweighting. Dr. Neugebauer states that Prof. Lambl has withdrawn his theory of hydrorachis as a cause of spondylolisthesis. Mr. Noble Smith's appendix to the report was read; he describes specimens showing caries as probably a consequence of displacement.

The following papers were read:

ON FIBRO-CYSTIC MYOMA OF UTERUS—SEPTICEMIA.

By DR. H. A. LEDIARD (Carlisle).—The symptoms dated twelve months back, and included pain, bleeding, vomiting, and anemia.

The tumor felt cystic in parts and a bruit was heard over it. It reached two and a half inches above the navel.

Afterwards diarrhea set in, and the temperature rose. After an examination by the sound, severe symptoms began, including the usual signs of peritonitis, and death occurred on the ninth day. The tumor grew chiefly from the posterior wall, and was partly cystic, as were both ovaries. The kidneys contained miliary abscesses; the spleen was enlarged and soft, containing several recent infarcts. Liver fatty; lungs edematous and congested. No trace of peritonitis was found, but no examination of the veins was made. The uterine mucous membrane was nowhere abraded. The case was brought forward chiefly to ascertain the opinion of the Society as to the cause of death.

THE PRESIDENT said that Dr. Lediard was not singular in his experience. A small abrasion was sometimes fatal.

NOTE ON A CONDITION OBSERVED IN THE NAVEL-CORD OF A YOUNG INFANT.

By JOHN WILLIAMS, M.D., F.R.C.P.—The cord at birth appeared natural. On the second day, the distal two inches had dried, while the proximal inch was succulent as at birth. In the next few days, it broke down and became stinking. There was some bleeding on the fifth day, the cord did not fall off till the ninth day, when it left a somewhat conical stump with an ash-gray surface and a small red central depression. A thin sheath sloughed off the stump. Sir J. Paget, who saw the child, advised its removal (if it remained) by ligature, as in the case of common wart of the umbilicus. It disappeared altogether on the fourteenth day. The early appearance of the wart was remarkable, it had not been observed before the eighth day.

NOTE ON THE INVOLUTION OF THE PUERPERAL UTERUS IN THE ABSENCE OF THE OVARIES.

By JOHN WILLIAMS, M.D., F.R.C.P.—The patient had had her left ovary removed in 1879, and her right was removed during labor in 1883, for cystic disease. Recovery uninterrupted. The involution was decidedly delayed, and on the twenty-eighth day it was still above the brim; the sound passed four inches. Seven weeks after delivery, the uterus was of the usual size in the fourth week. Seven months after delivery, it was smaller than the virgin organ, sharply anteflexed, and the cavity was only two inches long. In this case, the immediate effect of removal of the ovaries was retardation of involution, but the changes in the peritoneum caused by ovariectomy must be remembered. The ultimate result was exaggeration of the uterine involution, like senile atrophy.

THE PRESIDENT indorsed Dr. Williams' view of the case.

DR. CARTER remarked on the rarity of super-involution. There exists without doubt a condition of atrophy of the ovaries in such

cases, and their removal produced the effect which he should have expected.

ON FETAL EXTENSIONS OR RETROFLEXIONS DURING PREGNANCY.

By Drs. MATTHEWS DUNCAN and HURRY.—The authors have collected all examples of extension of the limbs or body of the fetus occurring during pregnancy, and have tried to throw light on their causation. These extensions may be of a leg, or of both legs, or of the whole body. They may be caused by malformation, goitre, amniotic bands, exomphalos, shortness of cord, and morbid conditions of the mother.

THE PRESIDENT remarked on the special value of the paper, as throwing light on some of the causes of face presentations.

DR. CHAMPNEYS said that the only variety that he had himself observed was the “*siège décomplété*” (Lefour). In this, when primary, the legs after birth at once resume the fetal attitude, and fly up over the shoulders in a most striking manner.

REVIEWS.

SPIEGELBILDER DER GESUNDEN UND KRANKEN VAGINALPORTION UND VAGINA.—THE SPECULAR APPEARANCES OF THE HEALTHY AND DISEASED CERVIX AND VAGINA. By DR. J. HEITZMANN. Three Parts, with 25 Chromo-lithographic Plates and 45 Woodcuts. Vienna: Wilhelm Braumüller, 1883, 1884.

This elaborate work has been prepared in particular for the general practitioner. The specialist possesses in his past and present clinical material diagnostic aids which the general practitioner lacks. When the latter is brought face to face with an unusual or rare case, possessing this work he may perchance be enabled to reach a diagnosis by reference to one or another of the numerous lithographic plates which enrich it. Such at least was the idea present in the author's mind when engaged in its preparation. We think that the result of his labors fully realizes his aim, and we have little hesitancy in saying that next in value to personal experience as an aid in diagnosis should be ranked these charts. For accuracy in delineation, for beauty and truth in coloring, they are unsurpassed, even though this last be often too pronounced, a pardonable fault, however, when we consider the difficulties in the way of an exact reproduction of natural tints. Of secondary consideration, though also worthy of praise, is the text. Heitzmann's description of diseased conditions is uniformly lucid, exact, and on a par with recent research. He is to be congratulated on being one of the European gynecologists who recognize a laceration of the cervix when they see one, and admit the value as well as necessity of trachelorrhaphy. He relegates ulceration of the cervix to its proper place as a rarity, and is unusually free from dogmatism in regard to pathological states which are still in dispute. Illustrative cases are freely introduced into the text—a fact which considerably enhances the value of the

work. In its scheme the work is progressive. It opens fittingly with an account of the gross and minute anatomy of the cervix and vagina, of sufficient length to enable the reader to follow understandingly the changes effected by disease. This anatomical chapter differs in but little from what is usually held to be correct to-day. As an exception, we would note that the author clings to the classical description which makes the hymen a simple reduplication of the vaginal mucous membrane. He should be familiar with the fact that the researches of Budin and others have clearly proved the hymen to consist of the same elemental tissues as the vagina itself. Following this chapter are a few pages devoted to the consideration of the speculum, its varieties, methods of introduction, and use. As is usual with continental writers, preference is expressed for the cylindrical form of the instrument, and the oft-repeated but erroneous objection is made to the duck-bill, that its use necessitates the presence of an assistant. The pages descriptive of the plates now follow, beginning with the simplest conditions—normal cervical varieties (Plate I.), anemia and hyperemia (Plate II.), variations in shape of the cervix (Plate III.), laceration and ectropion (Plate IV.), congenital and acquired cervical anomalies (Plate V.). In Plate VI. two cases of hypertrophy are figured, the one due to congestion, and the other to a deep laceration of the cervix. In the discussion of this subject of hypertrophy, the author makes a clear distinction between true hypertrophy and that apparent hypertrophy which is rather an elongation due to the stretching out of the cervical tissues, and he evidently recognizes how eversion of the edges of a lacerated cervix may simulate an enlargement. After differentiating these conditions and enlargement due to the presence of new growths, Heitzmann reaches the correct conclusion that true hypertrophic elongation of the cervix is a rare condition. On Plate VII. are figured certain conditions to which but little space is given by the generality of gynecological writers. We refer to eruptions on the cervix similar to those which accompany herpes, eczema, miliaria, etc., on the surface of the body. Plate VIII. is devoted to the cervical appearances following acute and chronic metritis. The subject of erosions and ulcerations are beautifully illustrated on the following Plates IX., X., XI., and varicose conditions of the cervix and vagina, croupous and diphtheritic inflammation, colpitis (granular, follicular) are severally typified on Plates XII. and XIII. The four following plates are descriptive of the varying effects of syphilis on the cervix and vagina (condylomata, erythema syphiliticum, hard and soft chancre, gummata), and then the great subject of tumors claims attention. On Plate XVIII. are figured the varieties of polypi; on Plate XIX., cystic tumors, fibro-myomata, sarcoma, miliary tuberculosis; on Plate XX., a large fibroid of the anterior lip of the cervix filling the vagina and appearing at the vulva; on Plates XXI., XXII., and XXIII., adenoma, epithelioma, carcinoma, and lupus. On the remaining two plates are figured examples of large vesico-vaginal fistulæ.

Such, in outline, is the scope of this work. It will form a valuable, perhaps even necessary, addition to the practitioner's library. The sole bar to any extensive circulation in this country is the language in which it is written. Owing to the faithfulness of the majority of the chromo-lithographs, however, the conditions often suggest themselves, the text being thus not strictly

essential. We hope, nevertheless, to see before long a reproduction of the work into the English language.

EGBERT H. GRANDIN.

THE TREATMENT OF BACKWARD DISPLACEMENTS OF THE UTERUS AND OF PROLAPSUS UTERI BY THE NEW METHOD OF SHORTENING THE ROUND LIGAMENTS. By WILLIAM ALEXANDER, M.D., etc., Visiting Surgeon to the Liverpool Parish Infirmary. London: J. and A. Churchill, 1884.

Any one at all familiar with uterine displacements and the difficulties in the way of their relief, will be pleased to hear of an operative procedure which promises a cure. Plastic operations on the vagina in case of descensus, pessaries, uterine and vaginal, in case of retroversion and particularly of retroflexion, whilst for a time they may afford relief, rarely effect a cure. Dr. Alexander's operation has given such brilliant results in his hands and in the hands of others who have tried it, that it must commend itself to every gynecologist as at least worthy of trial in any case where the ordinary routine means have been tried and have failed. The operation is theoretically a rational one; we can well see its simplicity and safety: and, after reading carefully the cases reported in this little book, we have no doubt of its practical value in well-selected cases. The method consists in cutting down on the round ligaments, at their point of emergence from the external abdominal ring, and then having replaced the uterus, the slack of the ligaments is pulled through the inguinal canal, and they are stitched into the incision. Whilst the incision is healing a pessary should be worn, but after complete union it may ordinarily be at once removed. We would express the hope that Dr. Alexander's modest announcement of what may prove a most useful operation may not only receive a cordial welcome and careful study, but be also put to the test of practical experience in this country.

EGBERT H. GRANDIN.

LE PIÙ RECENTI MODIFICAZIONI DEL TAGLIO CESAREO, Studio Storico-Critico del DOTTOR LUIGI MANGIAGALLI.

THE MOST RECENT MODIFICATIONS OF THE CESAREAN SECTION. An Historico-Critical Study. By DR. LUIGI MANGIAGALLI, Professor of Obstetrics at the Royal University of Sassari. Milan: Pietro Agnelli, 1884.

The reader will find in this work a complete exposé of the Cesarean section and its various modifications. The opening pages, of an introductory nature, are devoted to a brief historical sketch of the operation: Chapter II. treats of the utero-ovarian amputation and its results: Chapter III., of the modifications to which, in recent times, the classic section has been subjected; and in Chapter IV. are stated the indications and contra-indications of the utero-ovarian amputation compared with those of the modified classic section. To illustrate his subject, Dr. Mangiagalli has collected apparently all the reported cases performed after the various modified methods, and gives their essentials in abstract. Of 140 essentially Porro cases, 82 died or 58.57%; of 13 classic sections performed after one or another modification (Frank's, Kehrer's, Säger's, Gastro-Elytrotomy), 7 died or 53.84%; or, considering only Säger's method and gastro-elytrotomy, of 10 cases 5 died or 50%.

If any deduction from these statistics be allowable, it would seem as though the current must set against the Porro method,

and towards some modification of the classic section. The difficulty now, as ever, lies in securing an effective and safe method of suturing the uterine wound. Research along this line may possibly result in assigning to the classic section a higher position in operative obstetrics than it has ever yet held, for there are many who would then look to it as a more justifiable operation than either cephalotripsy or craniotomy, since thereby the interests of the child, as well as those of the mother, would receive due and proper consideration.

EGBERT H. GRANDIN.

ABSTRACTS.

1. **Wassily Sutugin** (St. Petersburg): **Hyperemesis Gravidarum** (Berlin: Eugen Grosser, 1883, pp. 32, 8vo).—The vomiting of pregnancy manifests itself in three main forms:

1. The woman vomits before breakfast, rarely during the day; no effect is produced on the health of the gravida.

2. She vomits now and then; meals are thrown up; the patient has a loathing for food and feels pain in the pit of the stomach; this form affects the patient but little and readily yields to rational treatment.

3. The emesis occurs not only after meals, but also between them; the patients are beginning to starve.

This latter form deserves the name: excessive vomiting of pregnancy, hyperemesis gravidarum. This term is preferable to "uncontrollable vomiting," inasmuch as it yields to treatment.

Most authors divide the affection into three periods:

The *first period* is characterized by some exhaustion, slight (ordinary) emesis, gradually increasing to the rejection by the stomach of all food; in rare cases the affection begins at once with excessive or uncontrollable vomiting; usually there is loathing of food, at times a craving for heavy diet. With increased emesis, there is some pain in the pit of the stomach, emaciation, anemia, apathy; often anxiety and despair. Salivation is not rare, also coldness of the extremities and night-sweats. Constipation is frequent, at times alternating with diarrhea. Hyperosmia may be present.

Second period. Symptoms of exhaustion increase; some fever, T. barely reaching or slightly exceeding the normal; pulse becomes more rapid, weaker, and changeable; respiration hurried. The patients become too weak to walk. The skin of the trunk is dry, hard; the extremities livid, cold, at times covered with perspiration. The emesis becomes incessant; mucus, bile, at times mixed with food, and all nourishment are thrown up. Absolute loathing of food; dryness of mouth and esophagus, incessant thirst, epigastric pain and headache. The mucous membrane of the mouth is reddened; teeth, gums, and lips fuliginous; tongue dry, red at the tip, and brown at the root. The breath becomes sour and offensive. The urine diminishes to 240 ccm. per day, becomes concentrated and offensive, containing albumen and casts. The emaciation becomes extreme; great exhaustion, and frequent syncope.

Third period. Syncope becomes more frequent; delirium and hallucinations; vomiting ceases. Coma and death from complete exhaustion.

The pregnancy often continues uninterrupted till death.

Complications. Stomatitis, which may become gangrenous; diarrhea; pelvic peritonitis; rarely phthisis, as well as hysterical and eclamptic symptoms.

Five illustrative cases are given.

The author thinks it unquestionable that the nephritis is the result of the starvation, as it disappears completely with the recovery.

Two main forms of the affection must be distinguished:

1. Those directly due to the changes in the uterus.
2. Those dependent on various diseases of the brain, stomach, and lungs; the symptoms of vomiting being merely increased by the pregnancy.

Most of the clinical phenomena are to be explained by the exhaustion from starvation.

The causes and the nature of the affection are still obscure. After citing and discussing the views of other observers, the author sides with Guéniot and McClintock, that various morbid conditions of the genitals, when pregnancy is superadded, increase the existing emesis, or directly give rise to the excessive vomiting. The latter must be interpreted as a neurosis of the stomach, appearing as a reflex from the uterus.

Diagnosis. After the existence of pregnancy has been established, other causes must be excluded.

The prognosis depends on the grade of development of the disease and on the treatment; in general it is grave (40.5%, Joulin); under the expectant treatment, 47%; when the pregnancy is artificially interrupted, 25% (McClintock). For the fetus the prognosis is still worse, though the ovum suffers but little by the exhaustion of the mother. Complications render the prognosis much worse.

Treatment. Any considerable vomiting of the pregnant should be treated, so as to guard against further development of the affection and especially the exhaustion of the patient. Systematic dietetic treatment. Absolute rest to the stomach; nutrient enemata. Thirst to be allayed by ice-water or selters, pellets of ice, or iced champagne. After from one to three days, systematic nutrition. At times solid food is better borne than liquids (the author recommends small meat-balls roasted over the spirit lamp). If meat is distasteful, condensed meat-juice in wafers should be given. Cold milk, a wineglassful every two hours. The dorsal decubitus is recommended by several authors. In some cases, passive exercise (carriage riding) is useful. Local causes must be removed or avoided (*e.g.*, coitus and frequent examination), malpositions corrected, over-filling of bladder and rectum guarded against. Local depletion of the vaginal portion by leeches or scarification is often of service. Local derivatives and vesicants to the epigastrium. Ether or chloroform spray to the precordial region for five minutes preceding meals.

Of drugs, the best results are obtained with anodynes and narcotics: belladonna (internally, and externally in the form of suppositories and ointments to the cervix), codeine, morphine. Tannin in pill form with confection of roses, 1-5 gms. twice daily. Others have recommended: hydrocyanic acid, tincture of iodine (gtt. iij, internally), cerium oxalate,

oxygen inhalations (often worse than useless), hyoscyamin. The author is in favor of chloral hydrate per rectum.

Locally to the cervix, silver nitrate solution (gr. x. to $\frac{1}{2}$ i. in $\frac{3}{4}$ i.), provided there is no hyperesthesia of the vaginal portion. The author had good results in a case of cervical endometritis [endotrachelitis] from painting the cervix with tincture of iodine. With symptoms of perimetritis, local applications of mercurial ointment or painting with tincture of iodine, warm compresses to the abdomen, chloral enemata.

Copeman, of Norwich, has seen good results from dilating the internal os.

Where all other treatment is of no avail, artificial abortion or premature labor previous to the third period is justifiable.

Conclusions.

1. Excessive vomiting is a very rare affection in pregnancy.
2. The disease appears from the second to the last month of gestation, more frequently in the first half.
3. It is more frequent in the better classes, though none are exempt; race has no influence.
4. It often appears in multiparæ.
5. The symptoms possess the greatest analogy with those of starvation, which latter serves to explain the diminution of the quantity of urine and the presence of albumen and casts in the latter.
6. Further observations are necessary to decide the question as to the frequency of albumen and casts in the second period.
7. Fatal issue is observed either in consequence of the exhaustion, or of puerperal diseases, or of complications having no direct relation to the pregnancy.
8. The nature or the disease is still obscure.
9. The treatment should correspond to the period of the disease and take place in the following order:
 - A. In the course of the first period, strict dietetic measures with the aid of pharmaceutical preparations by the stomach if retained, otherwise per enema.
 - B. In the second period, more restricted diet combined with absolute rest. Drugs must no longer be given by the mouth, only per rectum. Enemata of chloral and codeine are most effective.
 - C. Where symptoms of perimetritis are manifested, antiphlogistics are indicated besides the restricted diet.
 - D. Where the above treatment fails, artificial abortion or premature labor is indicated.
 - E. In the third period, stimulants and artificial nutrition of the patient, together with warmth to the body, should be resorted to.

2. H. P. Orum: Chemical Studies of the Fluids of Ovarian Cysts, with Special Reference to Diagnostic Puncture (Copenhagen, 1884, pp. 155).—This is the translation of the title of an excellent monograph published in Danish. The first part treats of the question if there is found any peculiar chemical substance in ovarian cyst fluids. The only substances which have been claimed to be characteristic of ovarian fluids are paralbumin and metalbumin, which were discovered by Scherer in 1852. Their chemical composition was, however, little known until Dr. Hamnersten, of Upsala, Sweden, published his paper entitled, "Metalbumin

and Paralbumin, a Contribution to the Chemistry of the Fluids of Ovarian Cysts" (*Upsala läkareförenings förhandlingar*, Vol. XI., 1881). He succeeded in defining their nature and indicating sure methods of discovering their presence. According to him, metalbumin is nearly related to mucin, and is not an albuminous substance, wherefore he has proposed to give up the name metalbumin, and calls its *pseudo-mucin*. Paralbumen, on the other hand, is nothing but a mixture of pseudo-mucin and albumen. Hammersten's method for finding pseudo-mucin is the following: The fluid to be examined is coagulated as completely as possible by means of acetic acid and boiling. If pseudo mucin be present, the fluid after filtration becomes white as milk, or, if the quantity present is very small, opalescent. Next the filtrate is somewhat evaporated, by which process often a little albumen is precipitated. After its removal alcohol is added, and the precipitate washed with the same substance, and pressed dry between blotting paper. The dry product is dissolved in water, with which it forms an opalescent fluid. Acetic acid is added in excess, by which all mucin is precipitated. After its removal by filtration, hydrochloric acid is added to the fluid in sufficient quantity to make a five-per-cent solution. This is evaporated until it becomes dark-brown, or, if very little is present, yellow-brown. When it has cooled off, it is neutralized with alkali, and has then the characteristic property of reducing an alkaline solution of copper.

By his own researches, Orum comes to the result that pseudo-mucin is not found in the normal ovary, but is a product or companion of the colloid¹ degeneration. Consequently, it is not pathognomonic of ovarian cysts, but still it has no small diagnostic value because the colloid cystomas are by far the most common of all ovarian cysts, and because the colloid degeneration is exceedingly rare in the abdominal cavity except in ovarian cysts.

The second part is devoted to the qualitative and quantitative analysis of ovarian and other abdominal fluids. He reproduces some old examinations, and adds a number of his own, viz., twenty of colloid cystomas, three of papillomatous cystomas, three of parovarian cysts, eighteen of serous ascitic fluid, and five of purulent peritoneal fluid, one of a fibrocystic tumor of the uterus, and one of a case of hydronephrosis. He has exclusively used fluids taken from patients upon whom ovariectomy was performed, and has taken the further precaution to have the cysts examined microscopically by an expert, by which means his analyses acquire a value, of which, for instance, those of Méhue are entirely deprived.

The character of the fluid depends always on the pathological process which causes its formation. Thus the fluid in dropsy of the Graafian follicle is entirely like that of other retention cysts; that of malignant cysts does not differ from that found in similar growths in other organs, and that of colloid cystomas is only peculiar on account of the colloid degeneration of the lining epithelium. He resumes what he has found about the fluid in the different kind of ovarian cysts in the following paragraphs:

A. *Colloid cystomas* are, as a rule, distinguished by a viscid, sticky, ropy fluid of high specific gravity (above 1.010). It contains always pseudo-mucin. In consequence of the tension of the pedicle or recent

¹ Colloid means only glue-like, and does not designate the process as cancerous.

hemorrhage, fibrin may be found. The microscope may reveal columnar epithelium, large and small cells with fat-granules, and colloid globules.

In consequence of the torsion of pedicle and extensive inflammation, it seems that the pseudo-mucin may disappear or be much reduced in quantity.

B. *Papillomatous and papillary cystomas* contain a thinner fluid, which often is mixed with blood, and which is not viscid. In the papillomatous cystomas is only found little pseudo-mucin, and in the true papillary cystomas it may disappear altogether. The specific gravity is commonly high (1.010-1.036), as albuminous substances are found in great quantity in them. If the accompanying ascites contains pseudo-mucin, and acute rupture and puncture can be excluded, it shows that the cystoma is a papillomatous one in which perforation of the cyst-wall has taken place. Microscopically are found the elements of blood, polymorphous columnar epithelium, often ciliated, rarely papillomatous villi, few or no cells in colloid degeneration.

C. *Malignant ovarian cysts*, if not complicated with colloid degeneration, do not seem to possess any chemical or microscopical peculiarities.

D. *Dropsy of the Graafian Follicle*. The fluid is thin, clear, with a specific gravity of 1.006 to 1.010. It contains only little albumin and no pseudo-mucin. By microscopical examination are found no or few formed elements, and if present, they are in no way characteristic.

E. *Dermoid cysts* contain a fluid characterized by the great amount of fatty substances (more than ten per thousand), the paucity of albumin and the presence of epidermal scales and hair.

I. *parovarian cysts* pseudo-mucin has never been found. It is not enough for the diagnosis to prove that there is an ovary besides the cyst, because the patient may have an accessory organ. In this way the author will explain the unique case of Spiegelberg, which is mentioned on page 52 of my monograph on the "Diagnosis of Ovarian Cysts by Means of the Examination of their Contents." William Wood & Co. New York, 1882. (See this JOURNAL, Vol. XV., p. 394.) Further, the author treats of the fluid in dropsy of the Fallopian tube, fibro-cystic tumors of the uterus, hydramnios, hydronephrosis, renal cysts and hydatids (echinococci), mostly limiting himself to a synopsis of what has been found by his predecessors. The last chapter is devoted to different peritoneal fluids of which the author has made a number of original analyses.

The little book under consideration, while offering much information about the chemical composition of ovarian and other abdominal fluids, will scarcely be of much practical value to ovariologists. The author shows himself that the characteristic substance, pseudo-mucin, is not found in papillary ovarian cysts, but as these develop in the broad ligaments close up to the uterus, they are more apt than any others to present great difficulties for the diagnosis. Next we learn that, under certain circumstances, the pseudo-mucin may disappear even from colloid cystomas. And finally, the process required for discovering the pseudo-mucin, even when present in large quantities, is so complicated and tedious that the examination can only be performed in a chemical laboratory and by an expert chemist. Since the microscope gives more information in much shorter time and with much less difficulties, it will probably remain the chief diagnostic help besides the history of the case and the physical examination of the abdominal organs.

H. J. GARRIGUES.

3. Gønner: The Treatment of Pregnancy and Labor when Complicated by Cancer of the Uterus (*Zeitsch. f. Geb. und Gyn.*, X., 1).—

Six cases are recorded in this paper, and, after a review of the literature of the subject, the following is laid down as the appropriate method of treatment: Laparotomy is only indicated in those cases where the disease has so implicated the neighboring parts that a sufficient removal of the growth by the vagina to allow passage to the fetus would inevitably lay open the bladder, rectum, or peritoneum. In other cases the advice is given to await the onset of pains, and to watch carefully their effect at dilatation. Thus the operator is enabled to judge at what portion the growth is to be removed and where incisions into it will suffice. The determination once reached, as much of the tumor is to be removed by the *écraseur* or galvano-cautery as is possible without opening into the neighboring organs; or else, where the disease is rather an infiltration than a distinct tumor, incisions will make the necessary opening. If the fetal head presents, the forceps are to be applied; the breech is to be extracted. Should the placenta not follow, as it is important to shorten the third stage of labor, it is to be removed and a hot carbolized douche administered. The cases recorded in this paper occurred in Bischoff's service at Basle, and the results are exceptionally good, five out of six mothers recovering from the labor, and only one out of the six children being lost, and this one asphyxiated at birth. The cases in brief were: Case I., æt. forty-one, 8para, carcinoma cervix, Freund's operation; living child: mother died in nine hours. Case II., æt. forty-one, 13para, cancer of cervix and vagina; removal of growth by galvano-cautery and sharp spoon; forceps; mother and child discharged; patient died ten and a half months after. Case III., æt. forty-two, 7para, cancer of cervix, premature labor at thirty-fifth week; extirpation of the tumor eight days after: discharged cured. In this case the growth was of small extent. Case IV., æt. twenty-seven, 2para, cancer of cervix; partial extirpation before labor; forceps; asphyxiated fetus: cauterization of growth before discharge. Since miscarried once, and once delivered of a mole. Case V., æt. thirty-eight, 4para, cancer uterus and vagina; extirpation with galvano-caustic wire before labor; forceps: living child; patient died in three months. Case VI., æt. thirty-eight, 6para, cancer cervix and vagina; extirpation and incisions in os; forceps; living child; patient died in four and a half months.

E. H. G.

4. Freudenberg: The Delivery of the After-coming Head (*Archiv f. Gynäkol.*, XXI., 1).—

The first portion of this paper is devoted to a consideration of the methods by which, since the time of Mauriceau, who first suggested a method, the delivery of the after-coming head has been effected. The methods in use to-day are five in number: 1. Smellie's, consisting in the application of two or three fingers of one hand on the occiput, the middle and the index fingers of the other hand in the canine fossæ, making downward pressure on the occiput and upward traction on the face, the child emerging face first and upwards. 2. The modified Mauriceau—downward traction on the occiput with one hand, and traction in the same direction on the inferior maxilla through two fingers in the mouth. 3. The so-called Prager method—one hand on the shoulders, the feet in the other hand, downward traction. 4. The combination of this method with downward traction on the inferior

maxilla. 5. Expression, either alone or in addition to any other method.

After describing these methods in detail, and enumerating their advantages and disadvantages, F. pronounces himself in favor of the Prager method, because it is easier of performance, carries with it greater certainty of delivery, and imperils the life of the child less. He then proceeds to speak of delivery through the forceps, the subject which forms the real basis of his paper. He desires to bring their use in the extraction of the after-coming head into greater prominence than is accorded to them to-day. He would not have them displace the hand where it will suffice, for he recognizes the fact that of all instruments the hand is the best. His plea is for more frequent recourse to the forceps in cases where the hand avails nothing or with difficulty. The material whence he has drawn his deductions was offered by the cases at Birnbaum's clinic between January, 1862, and December, 1882, nearly twenty-one years. During this time, there occurred 7,805 deliveries, with the result of 7,897 children, including 90 twin labors and 1 of triplets. Pelvic deformity was noted 665 times; 292 breech presentations, 41 in deformed pelvis.

The operative procedures were:

Version,	49 times (10 of these contracted pelvis).
Extraction,	106 " (18 " " " " "
Forceps to after-coming head,	59 " (31 " " " " "
Forceps to before-coming head,	390 " (125 " " " " "
Perforation,	29 " (28 " " " " "
Cesarean section,	25 " (24 " " " " "

Total number of operations 858, of which number 256 were in case of pelvic deformity; 1 Cesarean section was performed post-mortem. The maternal mortality is 1.74 per cent.

In the 292 breech cases:

Delivery by Prager method,	145
By other methods:	
Extraction,	102
Delivery by forceps to after-coming head,	12
Forceps to after-coming head,	22
Induced premature labors with forceps to after-coming head,	11
	<hr/> 147

292

The results in the 145 cases were: all the mothers living, 142 children living, 3 deaths (before delivery). The results in the 147 cases were: 1 maternal death, 136 living children, 11 children dying during labor, 7 dead before delivery.

The results for 292 cases:

Of 292 mothers,	291 living, 1 death.
Of 292 children,	271 " 11 deaths during labor, 10 before.

The results from version were:

(a) Version followed by Prager method, 29 times.
Four of these contracted pelvis; 2 maternal deaths, neither with contracted pelvis.

Of the 29 children: 10 living, 10 died during labor (3 contracted pelvis) 9 dead before (1 contracted pelvis).

(b) Version followed by forceps to head, 17 times.

Of these, there were 5 contracted pelvises; 3 mothers died (1 contracted pelvis).

Of the 17 children: 3 living (2 by contracted pelvis), 8 died during labor (3 contracted pelvises), 6 dead before (1 contracted pelvis).

(c) Version after mutilation of the fetus or use of the sharp hook, 3 times.

Of these, mothers all living (1 contracted pelvis); all the children dead before labor.

Version was also performed as a secondary operation after induction of premature labor 3 times, with 1 maternal death; 2 living children, 1 dying during labor.

The combined results of the primary and secondary versions :

52 mothers, mortality of 11.54%.

52 children, mortality during labor, 36.67%; before labor, 35.26%.

The general results of natural and artificial breech cases:

344 mothers, mortality 2.3%.

344 children, mortality during labor, 8.72%; before labor, 8.01%.

The ratio of infantile mortality during labor to living infants at labor is 1:10.52. The forceps were applied to after-coming head:

Breech presentation,	18
Extraction,	14
Versions,	17
Induced premature labors,	12
	--
Total,	59

Of these there were 31 deformed pelvises.

Of these 59 labors there were 4 maternal deaths (6.73%). Two mothers died with contracted pelvises out of 31 cases of contracted pelvises (6.45%). Two died out of 28 normal pelvises (7.15%). Of 17 versions, maternal mortality 15%. Of 12 premature labors, maternal mortality 9.09%.

The causes of death were: Puerperal fever, 3 cases (2 versions, 1 premature labor followed by extraction), 2 of these contracted pelvises; 1 death from anemia (placenta previa).

The statistics for the children:

(a) Breech cases, 18; living, 13; death during labor, 2; still-born, 3.

Percentage of children alive at labor, born living, 86.66%.

(b) Extraction cases, 12; living, 7; death during labor, 4; still-born, 1.

Percentage (as above), 63.63%.

(c) Version cases, 17; living, 6; death during labor, 6; still-born, 5.

Percentage (as above), 50%.

(d) Induced premature labor cases, 12.

Percentage (as above), 57.14%.

The special indication for forceps in these 59 cases were:

Head high up or side rotation in narrow pelvis, 25 times.

The same, complicated by narrow vagina, 5 times. Non-rotation of head, fixation of chin by side of pelvis, 4 times.

Head high up, no pelvic deformity, 5 times.

Head in pelvic cavity, deficient flexion, 9 times.

Birth of living child impossible from narrow or contracted vagina, 10 times.

Anterior rotated chin, once.

A study of these statistics has taught F. that the delivery of the after-

coming head, if the Prager method fails, should be immediately effected by the forceps, except, of course, where this instrument is contra-indicated. He lays down the following general indications for resort to the forceps.

1. In backward rotation of the occiput and extension of the chin under the symphysis.
2. In backward or lateral rotation of the face with lack of flexion and with impaction.
3. Where speedy delivery is called for.
4. Where the rima pudendi or vagina is narrowed or will not yield readily.
5. Where the hand is tired from attempts at manual delivery.
6. Where the head remains in the pelvis, rotated to the side.
7. Where, from the side of the mother, rapid delivery is indicated from metorrhagia, eclampsia, etc., the child being dead and manual delivery failing.

The contra-indications are:

1. Spasmodic contraction of the os around the neck of the child.
2. Pelvic deformity of high degree.
3. Great size of fetal head.

E. H. G.

5. Bayer: Cervico-vaginal Lacerations. Their Obstetrico-forensic Importance (*Archiv f. Gynäkol.*, XXI., 1).—The two cases of this lesion here reported are of rare occurrence, and of interest both from the side of etiology and treatment.

CASE I.—Patient, VIIlpara, had metorrhagia eight days before and on the day of entrance to clinic. On examination, placenta previa, os admitting two fingers, fetus transverse. The colpeurynter was introduced, one and a half hours after removed, when, there being further hemorrhage, podalic version was performed, though with difficulty on account of narrow os. Placenta followed in a few minutes; uterus contracted well under massage; considerable hemorrhage from posterior cul-de-sac. Attempts to stop it by suture ineffectual; finally resort to tampons with success. Patient restless, nearly collapsed. Died in 12 hours.

CASE II.—Æt. 24, IIpara, good personal history. First stage of labor, 26 hours. After full dilatation of os and whilst head was passing through the pelvis, patient suddenly became cyanotic and complained of pain in suprapubic region. Pulse frequent. After birth of the head, well-marked crepitation was determined above the symphysis, and blood mixed with air was pressed out of the uterus, after the delivery. The crepitation then disappeared. Hemorrhage stopped by tampon; two hours later renewed hemorrhage. Again controlled by tampon. On deep palpation a sense of soft resistance around the uterus; this organ pushed to the left. Death of patient two days after labor of septicæmia. In both these cases the cervix was torn; in Case I. one centimetre below the internal os; in Case II., up to it. The vagina also torn, though not into the peritoneal cavity. The tear in both cases on right side. Blood mixed with air had penetrated through the rents, in the second case along the psoas major up to the right kidney, where the extravasation had become converted into foul pus and gas. Cases similar to these have been reported by McClintock, Hecker, Dohrn and Löhlein, and in all except

Hecker's and B.'s the subperitoneal emphysema was anterior to the uterus, usually, too, the cervical rent is on the left side; whilst in B.'s cases and one of Hecker's the rent was on the right. As to the etiology of B.'s cases, rupture in the first may have followed on the force necessary to effect version, in the second the lesion was spontaneous. In this last case, the pathologist was unable to find a cause for the lesion. There was nothing abnormal in the pelvis, the fetus was of average weight, the presentation a favorable one, no hydrocephalus. As for the treatment of conditions of the kind, our resources are slight, more so, indeed, than in cases of complete rupture. Antiseptic principles cannot readily be applied. Neither drainage nor irrigation are very effectual; for we have to deal not with a free, open space, but with the subperitoneal cellular tissue which is distended with blood and air. For the hemorrhage, the best treatment is the tamponade with antiseptic cotton, whereby at the same time disinfection is secured and entrance of air prevented.

E. H. G.

6. Rumpe: A Contribution to the Statistics of the Induction of Premature Labor (*Arch. f. Gynäkol.*, XXI, 1).—During the past five years this operation has been performed fourteen times at the Marburg Clinic. These cases are reported in this paper and the general results stated as follows: The total number of labors at term through which these patients had passed amounted to 44, with the result of 32 dead and 12 living children. Premature labor was induced 16 times in these same patients, with the result of 13 living children and 3 deaths. The maternal mortality was 1 : 14, or 7%; the maternal sickness 3 : 14 or 21%.

Prof. Dorn, chief of the Marburg Clinic, reported (*Archiv f. Gynäkol.*, XII, S. 53) the cases of this kind occurring previous to 1877. The sum total of cases up to date of R.'s paper is 56. The deductions from all these cases are:

Twenty-six pluriparæ afforded 106 labors: 65 of these went to term, with the result of 53 dead and 12 living children, whilst in 41 premature labor was induced, and there resulted 14 dead and 27 living children. The ratio of the dead to the living children is then, for the labors at term, 81.5 : 18.5; for the premature labors, 34 : 66.

Premature labor was induced 15 times in patients who had never yet gone to term, with the result of 8 dead and 7 living children.

The maternal mortality, out of all the cases, was 7%, or 4 : 56.

The pelvic conformation in the 56 cases was:

Uniformly flattened,	43 cases.
Simple flattened,	6 "
Obliquely contracted,	4 "
Uniformly contracted (equabiliter justo-minor),	2 "
Kyphotic, transversely contracted,	1 case.

The uniformly flattened pelvis, then, existed in the large majority of cases. These cases, therefore, are further analyzed as follows:

In 8 cases the conjugata vera was below 8 cm.—4 children born dead, 4 living (50%).

In 28 cases the conjugate was between 8 cm. and 8.5 cm.—9 dead and 19 living children (68%).

In 7 cases the conjugate was over 8.5 cm.—1 dead, 6 living children 85%.

The majority of operations, then, were performed in cases where the conjugata vera measured between 8.5 and 7 cm. It is noteworthy that, in the 7 cases where the conjugate measured 8.5 cm., 10 labors at term had previously occurred with only 2 living children, only 20%, whilst, as seen above, 85% children were saved through induction of premature labor. These results, together with the fact that the puerperal period was altogether easier for these women than it was after their labors at term, points clearly to the fact that no close limit to the induction of labor should be drawn. And to-day the prognosis both for mother and child is better, because of antiseptics on the one hand, and improved methods on the other. At the Marburg Clinic, induction is first attempted through the use of hot (35° to 40° C.) carbolyzed (1%) vaginal douches. In certain cases, only a few injections are requisite; in others, particularly pleuriparæ, as many as one hundred. If injections fail, an elastic catheter is to be pushed between uterine wall and membranes. Before the onset of labor, obviously, wherever possible, the position of the fetus is to be altered to a head presentation. After the birth of the fetus, since it is premature, particular attention is to be paid to its nourishment. The mother's diet must also be attended to, and she is only dismissed from the hospital when the weight of the child is a sufficient guarantee of its future development.

E. H. G.

ITEMS.

1. THE original article on "Our Present Knowledge of the Relations between Micro-organisms and Puerperal Fever," in the July number, was by Dr. RICHARD (not Carl) LOMER. This gentleman is about to make a trip around the world as ship-surgeon, and then contemplates establishing himself in New York City.

2. THE NEW YORK POLYCLINIC will open its third season on September 22d, in the new quarters fitted up in the building hitherto occupied by it, which was bought by the Faculty last spring. The facilities for instruction are greatly improved by the new arrangements.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

REPORT OF A CASE OF ENCEPHALOCELE: SUCCESSFUL LIGATION.

BY

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of Kansas City, Mo.

THE comparative infrequency of encephalocele as a congenital malformation, the caution advised by our authors as regards operative procedures for it, and the gratifying results which I have recently had in the management of a case, lead me to make this report. It was to me a very interesting case and one which I followed closely, though I regret that, owing to the bad hygienic surroundings, as well as my unfavorable prognosis, I did not watch it as assiduously as I should have. Before describing the case, I will cursorily review the history of encephalocele, for it may be of general interest, as it certainly has been instructive to me.

The writers of the latter part of the eighteenth and beginning of the nineteenth centuries spoke of it solely as "hernia cerebri" or "fungus cerebri." It is a protrusion of the brain tissue from the cranial cavity, generally in the course of a suture or fontanelle when congenital, and in other situations when traumatic. Its size varies, and it increases gradually, becoming sometimes very large.

Dr. Gross reports a case in which the tumor was as large as the fetal head. There are then two modes of its production: one is a congenital malformation, the other is traumatic, viz., trephining, injuries, etc. In this paper we only consider con-

genital encephalocele. Being situated in the course of the sutures and fontanelles, it was supposed by the older writers to be due to deficient ossification, which is partially correct, though the real cause certainly is within the cranial cavity, and associated oftentimes with ulceration of the dura mater, as well as sometimes with cerebritis. In by far the great majority of cases reported the integument was perfect, but in a few cases even that was wanting.

Dr. Burrows, in 1810, describes a case of this kind as follows:—"The whole of the forehead, summit, and greater part of the occiput were deficient, and in lieu of them a substance projected of a light mulberry color, and of the mushroom form, except that it was proportionately broader. From the deficiency of bone, the eyes appeared to project much more than usual. The child lived six days without even taking sustenance or having an evacuation." Bryant, in his work, refers to a case by Lichtenberg, in which the tumor was hanging out of the child's mouth, through an opening in front of the sella turcica. Cooper reports a case which lived thirty-three years without any marked change in the intellect. Mr. Prescott Hewitt, in Holmes' "System of Surgery," reports two cases in which the cerebral structure protruded from the anterior nares and external auditory meatus, both of which proved fatal. He also reports two other cases, one occurring at St. Bartholomew's Hospital, and the other from the *American Journal of Medical Sciences* for 1859, wherein the masses appeared at the above-mentioned situations, which ended in recovery. Spring, in 1853, mentions a case in which the encephalocele involved the entire left hemisphere; patient recovered and lived eleven years.

Lawrence has recorded seventy-nine cases, only six of which reached adult life.

The cases to which I have just referred are certainly remarkable ones, and not by any means typical cases of congenital encephalocele. Ordinarily the tumor is either in the occipital region or at the anterior fontanelle. It pulsates, bleeds freely if irritated, grows gradually if it is progressive, creates primarily very little, if any, general disturbance, but secondarily stupor, vomiting, etc. Especially does pressure upon it produce momentary coma and cerebral disturbance. Patients may

die from asthenia, or directly from cerebral hemorrhage. Bryant says it may be mistaken for a simple cyst, and unless positive as to its character, we had better leave the case to Nature.

Various methods of treatment have been suggested from time to time, among which are, compression, complete and immediate excision, and ligation. In comparing the present treatment with that of seventy-five or one hundred years ago, we find that we are more prone to trust to Nature for the excision of the tumor by ossification, than we are to operative means. I suppose this is correct in certain cases, but I am satisfied it will not suffice in all.

On the other hand, I find the earlier surgeons favored more the knife, and I was surprised to learn that among them only one, Van Swieten, recommended, and practised with reasonable success, *ligation*. As to compression by zinc plates, caps, etc., which have been recommended, they have been found to be only of slight value, and of course utterly valueless and harmful after the tumor has become larger than the opening in the skull.

From my investigations of the subject, and my experience in this one case, I would advise trusting to Nature to check the growth of the tumor, or obliterate it by closing the fontanelles or sutures; but when she fails, the removal of it by repeated ligations. While the removal of an encephalocoele by excision is generally accompanied by copious and may be fatal hemorrhage, yet Quesnay reports a case in which the patient tore off the protruded mass himself, and the cavity healed up. I will now report the case I have recently seen.

Willis M., colored. Born August 28th, 1882. The child, when about seven weeks old, was brought to me by its mother. I found a tumor, three-quarters of an inch in diameter, situated over the anterior fontanelle. The mass was not smooth, but regularly grooved; pulsated, and had the characteristics of brain tissue. The mother is a stout, muscular, healthy woman, forty years old, and weighs about two hundred pounds. She is the mother of twelve children, all of them, with the exception of this one, without any deformity. She informed me that the week just previous to her last labor, she was daily engaged in carrying coal up several flights of steps; also that her labor lasted, to use her expression, "longer than with any three of her other children, and the suffering was much greater." I questioned her closely on this point, and from

her repeated statements on different occasions, I believe her labor was a long and tedious one. She says, as soon as she looked at the child, she saw "a small lump about the size of a split bean on the fore part of the head, bluish in color and smooth." This lump grew, and as it grew it lost its smoothness and blueness, until when I saw it seven weeks after birth, it was as I have described above. I presented the case to my class, and it was also examined by various members of the County Medical Society. Before interfering with it, and especially as the child was not suffering in any way perceptible, I decided to wait and follow the advice of surgeons, and see what Nature would do in the case. I, however, gave the mother a simple ointment to allay any irritation, and instructed that the child be returned to me every two weeks, or oftener if necessary. This course was continued until the middle of December, during which time the tumor was gradually but very slowly increasing in size; and now there was slight nausea at times, slight emaciation, and some stupor. Two days after the last regular visit, which was December 11th, the mother, very much excited, returned with the child. And I must confess that when she uncovered its head and I saw the condition of affairs, I was, until she gave a history of the last two days, surprised at its size and appearance. The tumor was now as large as an ordinary orange, and, in short, in two days' time it had increased five or six times its size. Brain tissue was distinctly marked; no indications of hydrencephalocele. There was a pedicle three-quarters of an inch in diameter, and the mass moved with each pulsation of the heart. The child now was extremely nauseated, even throwing up its milk. Upon inquiry, I found that, during the two days the tumor increased in size so rapidly the mother was compelled to be absent from the child most of the time. The grandmother was left in charge, and she informed me that during that time the child cried a great deal and very violently, and that during these fits of crying she could plainly see the tumor getting larger, or, to use her expression, "see the lump filling out."

I concluded I had given Nature a fair trial, and decided to remove the mass. It was now bleeding freely when touched, and fearing hemorrhage I ligated it. From that moment all nausea and vomiting ceased. I reapplied the ligatures about January 1st and 15th, and had the satisfaction of learning from the mother that in the latter part of January the tumor dropped off. I then visited the child and found the proximal end completely healed, thus cutting off all connection with the cranial cavity though the opening in the skull. I saw the child again in the early spring of 1884, or over one year after the mass had been removed; there was no evidence of the tumor returning, and physically and mentally it was as well developed as children generally are at its age.

HEMATOMA OF BOTH SUPRARENAL CAPSULES.

BY

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THE case of hemorrhage of the suprarenal capsule reported in this JOURNAL for July, 1884, by my predecessor in this hospital, Dr. Milroy, leads me to think that the following may be of interest. This case was on the service of my visiting surgeon, Dr. R. A. Murray, and is reported by his permission.

Julia R., aged thirty-two, multipara, was delivered by podalic version July 11th, 1884. This was her sixth child. All her other children had been healthy, and labors normal. The second stage in this labor lasted ten minutes. Considerable, but gentle, force was used in delivering the head and shoulders. Child was a male, weighing ten pounds and twelve ounces, apparently asphyxiated, but under the usual treatment recovered and cried lustily in fifteen minutes.

Both arms of the child seemed to be paralyzed, but it moved the fingers at will. With this exception it was in good health up to two days after birth, when it gradually became jaundiced and vomited occasionally.

It refused to nurse, but was apparently hungry and drank eagerly from a spoon. A green mucous discharge from the bowels began with the vomiting, but soon became much more frequent.

The jaundice on the third day increased rapidly and became very marked. Baby remained in a continual and profound sleep except when aroused to take nourishment. On the fourth day, July 15th, the vomiting had entirely stopped, and the diarrhea was markedly diminished under treatment, but the baby was becoming very weak and it was some trouble to get him to take nourishment. He was also, if possible, still more jaundiced.

July 16th. Patient became weaker and weaker and died from exhaustion, with no marked symptom, at twelve P.M., five days and a few hours after birth.

Autopsy, fourteen hours after death.

Sinuses of skull and veins of pia mater distended with dark fluid blood.

Liver of a reddish color, mottled with yellow. On section, the reddish areas are seen to be spots of congestion, and the yellow bile stained. At various places in the liver are whitish spots and streaks resembling new connective tissue. Otherwise the organ is normal.

On the right side, at the situation of the suprarenal capsule, is a dark fluctuating mass about the size of a kidney. A similar tumor on the left side was opened on removal, and about an ounce of dark fluid blood escaped. That on the right, on section, allowed an equal quantity to escape. A careful search for the remains of the suprarenal capsule showed, in the border nearest the kidney, a thin yellow zone with a broader reddish-black internal layer. The latter formed the wall of the blood sac, and was jagged in appearance. The other capsule was the same. Structurally the other organs were normal. They were more or less congested and bile stained. The mucous membrane of the intestinal tract was much swollen and congested throughout its whole extent. The gall bladder was very much distended with mucus.

There was also a small extravasation of blood in the pectoralis minor muscle, but no bruise of the surrounding tissues or fracture of the underlying rib. A slight extravasation was present in the tissues of the scalp over the right occipito-parietal region.

The child's head was abnormally large, and ossification well advanced, the posterior fontanelle being a point only, and the anterior very small. The frontal suture had almost entirely disappeared.

In the history of the Maternity Hospital since it has been on Blackwell's Island there are only three cases (out of about 6,000 deliveries) of this lesion on record. One occurred about ten years ago.

Usually the causes of infant mortality are so plain, and the reasons for having post-mortem examinations so few, that autopsies of very young babies are rarely held.

The occurrence of these two cases so near together, in point of time, would suggest that possibly this condition is not so rare a lesion in young infants as is generally supposed.

In this case the autopsy was held because of some little difficulty in accounting satisfactorily for the cause of the above symptoms, not because attention was in any way attracted to the neighborhood of the kidneys.

In this case there were also two other slight hemorrhages into the superficial tissues, neither of which were noticed during life and neither of which can be accounted for by external violence.

That these hematomata of the capsules have some definite relation to the general condition of the child is undoubted. It would be of interest to know just what this is, whether this curious pathological condition was a cause or a result of the existing disease. It is a question of interest also to know at what time this hemorrhage took place.

The child was under close observation, and at no time was any sign of internal hemorrhage noticed. It seems hardly probable that it could have taken place before or during birth. I neglected to state in the history that the temperature was throughout the disease above the normal. When the vomiting and diarrhea were most marked it was 103.9° F. and the respirations were 80.

The theory of a general vascular weakness would seem to be the most plausible in this instance, and yet it hardly covers all the points.

The mother of the baby was a perfectly healthy woman, as far as could be ascertained. The father died of acute phthisis a few weeks before the birth of the baby.

TRANSLATIONS.

THE INHERITANCE AND TRANSMISSION OF SYPHILIS.

BY

DR. M. KASSOWITZ,
of Vienna.

Translated from *Jahrbch. f. Kindhlkde.*, xxi. B., 1 u. 2 H.

By J. FEWSMITH, Jr., M.D., Newark, N. J.

(Continued from p. 784.)

HERE we have a case which we must look at earnestly. At least, Zeissel's statement that, from the beginning of wedlock to the end of the puerperal period, he examined the woman almost daily leaves no chance for reproach in regard to the carefulness of the observation. (In a foot note, Kassowitz states that he would regard this examination of a supposably healthy woman by a well-known syphilidologue as almost impossible, were it not so positively stated, and were it not that, in another case, Zeissel says that he made almost daily examinations [presumably of the sexual organs] during fully two years.) But there is another point which is not unimportant. In all other reported cases of this form of infection, the outbreak of syphilitic symptoms in the women took place during pregnancy, and generally in the early months, while here it did not occur till a week after delivery. This case would therefore seem to mark the transition to our second category, in which the syphilis of the mother appeared a long period after the

first syphilitic birth. It is a pity that in all Zeissel's publications on this theme we can find nothing to show whether this case, differing in so important a point from those of other authors, was the only one in which secondary symptoms appeared in a mother without primary lesion, or whether he had observed other such cases, and whether in these also the disease of the mother appeared after the delivery. This lack of clearness is increased by the fact that Zeissel has thrown the three classes of cases of retroinfection, which I consider it so important to divide, all together. In general, we gain the impression that he lays the weight of his argument on cases of the second and third categories. Finally, the value of this single observation is greatly lessened by the fact that Zeissel is a decided believer in the possibility of infection of a woman through the semen of a syphilitic man without impregnation, and this, too, without the appearance of a primary lesion—a possibility denied by most other authors. But if a man believes this possible, he is, in my opinion, not justified in considering any case as *proving* the infection by placental circulation, because he can in no case exclude the possibility of infection through the semen. The most he would be justified in claiming would be the *probability* of placental infection in a case where a woman had had intercourse with a syphilitic man for a long time before conception without any symptoms of syphilis, and then, soon after conception, showed specific manifestations. But even this argument does not hold in Zeissel's cases, because in them the mother's syphilis did not appear soon after conception, but usually years afterward, just as he has described the result and course of infection through the semen. Zeissel, therefore, has not yet offered strict proof that, in any single case, infection from the fetus has been known to take place.

Hutchinson may finally be noticed under this head, inasmuch as he accepts the three categories as above made. In discussing the cases which seem to belong to this first class, he makes the admission that there is a suspicion that the infection may have occurred in the ordinary way. When so strong a defender of the theory of retroinfection cannot avoid expressing his doubts thus, we may, surely, be allowed to express our opinion.

The most important consideration is directed at once against the weightiest support of the theory—namely, the reported absence of the primary lesion in women showing signs of syphilis following conception. We have seen that, in by far the greater number of the cases reported, the statement that the general disease was preceded by no primary lesion rests, not on any scientific examination of the affected individuals in the period before the outbreak of the general symptoms, because, with a few exceptions, the cases were not examined by physicians before this time; but, upon the more or less decided statements of the diseased women, or often on the word of the husband that he had had no local symptoms since his marriage. Now, even leaving out of view the frequent cases of purposed deception, the value of such statements seems exceedingly small when we listen to what various ob-

servers—and among them the most experienced syphilidologues—have to say concerning the very great difficulties which may surround the recognition of an existing initial lesion on a man and still more on a woman, and the establishment of the character of a former, but now healed ulcer, even by a skilled specialist. Thus Weil says: “Whoever has had an opportunity of examining a large number of *non-pregnant* syphilitic women will certainly agree with me that, in a large proportion of the cases, it is impossible to find any primary lesion or scar of one, or to determine, by the history, its presence or its location. This fact, which in non-pregnant women simply shows that the primary lesion has been overlooked, in pregnant women is used as an argument for the *choc en retour*.”

According to Bäumler, the majority of well-known syphilidologues declare that an induration on the female genitals is rare, and while he does not exactly accept this statement, yet he agrees with Fournier, who calls attention to the fact that one reason why an induration frequently cannot be found on the female genitals is because it occurs in positions (as in *introitus vaginae*) where we cannot feel and test it properly. In this connection, another statement of Fournier is also of interest. In a particular publication, he discusses the enormous frequency of “unperceived (unknown) syphilis,” and remarks that not only women, but also men may overlook the primary lesion, the swelling of the glands, and a roseola. In his latest publication, also, he remarks with what ease the husband affected with general syphilis may overlook the very slight cutaneous or mucous manifestation with which he brings about an infection of his better half. In women, moreover, the primary lesion may be in the cervix and hence overlooked. Rasumow has lately shown that of thirteen hundred and seventy-four cases the primary lesion was located on the cervix in one hundred and seven, or nine per cent. It is, therefore, not so great a rarity. Such an ulcer may not only run its course entirely without the knowledge of the patient, but, when healed and cicatrized, may escape the examination of the most scrupulous physician. Moreover, we all know that the primary lesion, even in other localities, often heals so entirely as to leave, after a few months, absolutely no trace behind it. Edlefsen has lately reported a case in which the grandmother of a hereditarily syphilitic child was infected from it while taking care of it. On the mouth of the child were ulcerations, and it was shown that the grandmother was in the habit of kissing it, and, when feeding it, putting the same spoon in her own mouth, so that it was almost certain that infection took place in this way; yet in the grandmother's mouth absolutely no trace could be found of a primary lesion, but undoubted symptoms of the general syphilis. This case, which by no means stands alone (think only of the so-called syphilis *d'emblée*), in my opinion, gives a heavy blow to the argumentative force of all the cases in which the acceptance of retro-infection depends on the non-discovery of a primary lesion. And, also, we cannot exclude the *possibility* that even in the very few

cases where the examination has been sufficiently complete—and such cases are *very* few—there may have been circumstances similar to Edlefsen's cases, which proved nothing except that, in spite of the non-discovery of the point of infection, such infection had nevertheless taken place from without.

A further objection, of more theoretical kind, is this. It is said that a pregnancy of even a few weeks or months is sufficient for infection of the mother from the fetus, although in most cases the fetus itself shows no symptom of syphilis itself. We must take for granted that the poison is present in some form, in such a fetus, even before there are any visible manifestations. But we are also justified in saying that in this early period it is not present in the circulation, or else, according to our present knowledge, it must produce visible manifestations on either the skin or internal organs. There can be therefore, in such cases, no transmission of the poison from the blood of the fetus to that of the mother. The same is true also of those cases in which the child is born apparently healthy, and only later shows syphilitic manifestations, while the mother may have had signs of general infection even in the *first months* of pregnancy.

The case then stands thus:—*The few observations which claim to prove infection of the mother, with syphilis marked by the outbreak of secondary symptoms, from a syphilitic fetus, are not convincing, and not sufficient to make such an infection scientifically completely credible.*

2. *The infection of the mother is said to show itself only in tertiary symptoms or a syphilitic cachexia:*—Hutchinson has always been and is to-day the principal supporter of this theory, which he first introduced in 1856 and which he has supported, with various theoretical modifications, in numerous publications since that date, the last one being in 1876. In my first article I gave the details of his earlier publications, and the arguments against his points. In this last publication he offers nothing new. He believes that “in somewhat less than half” of those cases in which previously healthy women bear syphilitic children begotten by syphilitic men, this form of specific disease of the mother results; that, often during the first pregnancy, but frequently not till afterward, such women begin to lose their health, their hair falls out, and much later, even after many years—in one case not till after the climacteric period—we find ulcerations on the tongue, the surface, the palms, and subcutaneous gummata. [This is probably the theory most generally accepted in this country and England, probably on the strength of H.'s opinion.—J. F.] But we may notice that, even in his last work, Hutchinson has not been able to bring decided proof in any case that these late forms of syphilis have not been preceded, years before, by the ordinary primary and secondary lesions. On the contrary, he makes the important confession that “in no single case has the patient been under his observation during the whole period, but that he had to rely on her memory in regard to the absence of earlier symptoms.” According to this, Weil's judgment of Hutchinson's

theory—which we now present—does not seem too severe. Weil says :—“The extremely incomplete reports of the syphilis of the children and the disease of the fathers, and the statements concerning the earlier health of the mothers—depending only on their own accounts and not sufficient to satisfy even moderate demands—give rise to the suspicion that symptoms which had previously been present have been overlooked or concealed, and make the conclusions drawn by Hutchinson seem actually arbitrary. In addition, in a portion of the retroinfected women, the diagnosis of syphilis is not strictly enough made. No one would consider a pale, faded woman with defluvium capillorum as syphilitic if her husband and children were healthy. Therefore, we have no right to use a case in which we have only these symptoms as an argument in favor of retroinfection, simply because the children and husband are diseased.” Rosenberg has lately made an even more severe and probing criticism of Hutchinson’s theory. This I will not, however, introduce here, but only refer to. For myself I may add that in the great majority of Hutchinson’s cases, in which nothing was known of the father, and the mother presented only indistinct symptoms of a cachexia, the whole theoretical structure rested simply, and only on certain symptoms in the children which Hutchinson declares with decision to be late symptoms of syphilis, but which nevertheless do not possess, for me and for the majority of German observers, by any means the decided diagnostic value which Hutchinson, and other English and French authors, have ascribed to them. I refer to the occurrence of keratitis interstitialis, and peculiar forms of the incisor teeth. During the last ten years, with a very rich field of observation, I have made these two points a matter of the closest observation, and can say in regard to them :

(1.) I have found both symptoms, but especially the malformation of the teeth, in children whom I have known from birth, and whose parents and brothers and sisters have been under my observation, so that I could positively exclude hereditary syphilis in them. I have also seen this formation of the teeth, which Hutchinson says should appear only on the upper, central, permanent incisors, on the other permanent incisors and frequently on the milk teeth. In rachitic children, to which I have of late years given the greatest attention, such teeth are a not at all uncommon occurrence.

(2.) I am so situated that I have under continuous observation a very large number of older children, whom I treated in their infancy for hereditary syphilis. Many of these children already have their permanent teeth, and I have not yet found Hutchinson’s peculiarity in a single one of the cases. In regard to the eye affections, many of these children have had the ordinary phlyctenular form of conjunctivitis and keratitis, but none of them the interstitial diffuse keratitis said to occur only in hereditary syphilis. Professor Manz (Freiburg) has, moreover, quite lately shown that even this form of keratitis is not to be considered as a specific syphilitic affection.

And thus those cases of Hutchinson's, in which nothing else was shown except that poorly nourished and cachectic mothers bore children who had these peculiar teeth or a chronic inflammation of the cornea, have in reality no value for our special question. And, in our opinion, only those cases are to be taken into consideration in which the syphilis of the children is indisputably proved, and the mothers show actual syphilitic manifestations of the tertiary period. Then we must require sufficient proof that these women have not had an "unimportant, benign, easily overlooked primary lesion" and also misunderstood secondary symptoms—a requirement, which, as we have seen, is not fulfilled in Hutchinson's cases. A thing which happens frequently enough in our ambulatorium will serve to show clearly how little reliance can be placed on the statements of the women in this respect, especially women of the lower classes. Very often, when I have at the first glance diagnosed hereditary syphilis in a child and asked the mother whether there was any rash on the body, I have received in reply a very decided No; and then, on examining the child, found the most beautiful general exanthema or striking ulcers on the usually preferred locations. Think then how reliable such statements would be in regard to a period long passed! And since, as we have heard, all the data in Hutchinson's cases in regard to the absence of primary and secondary symptoms depended only on the statements of the women, these cases, therefore, cannot be accepted as proofs.

Not less worthy of consideration is the fact that Hutchinson has constantly changed the tenor of his observations in regard to women supposed to be infected from fetuses, according to the varying standpoint of his theory in this connection. Formerly he was of the opinion that such women showed only tertiary symptoms, because the syphilis of the men, from whom they indirectly obtained theirs, was in the tertiary stage. He seemed not to notice that the fetus itself, the means of transmission, frequently showed exquisite secondary manifestations, and therefore there was plentiful opportunity for the mother to acquire them. At that time he also made the remarkable assertion that the more syphilitic children such women bore the more intensely they became diseased, because they constantly received new supplies of the poison. Not content with having started a theory which so utterly scorned all our previous knowledge and understanding of the single syphilitic infection, and ere long consequent immunity against new infection, he went on to declare that this increasing poisoning of the mother caused a constantly increasing intensity of the disease in subsequent children. For the sake of his theory, he placed himself in opposition to the universally accepted fact of the continuous decrease in the intensity of the disease in later births. These striking statements have, however, not lately been reiterated by Hutchinson, since he has found another theory to explain why syphilis acquired from the fetus differs from the usual form—namely, because the infection is received at a different port of entrance. But these varying theo-

ries and the reports of facts which, to a certain extent, are fitted to them, do not serve well to convince others of the truth of the theories themselves or the reliability of the observations.

This second form of placental infection of the mothers has in fact but few supporters and many opponents. It is even completely ignored by the most important supporters of the theory of retro-infection, Fournier and Diday. I may mention two of its supporters, Zeissel and the American, Engel. The latter has reported some observations on this theme, but, unfortunately, they are highly peculiar and greatly confused. He does not make it clearly understood when he means spermatic infection—which he says manifests itself, like the other, in simple pains in muscles, nerves, and bones—and when he means retro-infection. Moreover, he claims that such women, with no outward sign of syphilis, may infect men, not only during sexual connection, but also in the course of attendance during labor. For example, he claims to have seen three cases in which the obstetricians were infected, although the women themselves were entirely free from symptoms of syphilis, and only the children, later, showed specific manifestations. Such reports, to be believed, must be better grounded than in this publication, and since the syphilis in the mothers was determined simply by vague, non-characteristic sensations, we may allow these observations to drop out of our consideration.

Zeissel's statements are of greater weight, especially one observation which is repeated several times in writings of this author, and which runs about as follows:

At the end of his fiftieth year, a married man was treated for psoriasis palmaris. In spite of warning, he continued to have intercourse with his wife, who, up to this time, was healthy. In two successive years she bore two hereditarily syphilitic children who died soon after birth. She herself began to fade and lose her hair soon after the first conception, and in the second pregnancy she had a psoriasis palmaris specifica. The woman was examined *almost daily for two years* preceding the appearance of the exanthema, and any overlooking of the primary lesion may be excluded.

In spite of this most extraordinary feat, almost daily examination of the genitals for two years, there is still something to be said about primary lesion and secondary symptoms. It is not stated how long these two had been married when the husband first produced his psoriasis palmaris. It does not say that he was warned against entering the married state, but against having intercourse with his wife, so that probably he had been married some time; and so, after all that we have seen, the possibility is not excluded that the whole commencement period of the syphilis, including the primary lesion, ran its course as "unperceived syphilis" in the sense of Fournier. This one case, moreover, constitutes Zeissel's whole casuistic on this point. In the last edition of his book, there is a statement added that other analogous cases have been seen, but it is not stated that the same extraordinary regulations had been carried out, nor whether these examinations were

begun early enough to exclude any earlier disease. It seems to me that the question whether there is truly a syphilitic infection which manifests itself only in tertiary symptoms is important enough that we should not be sparing of proofs, and it certainly might be required of this celebrated author that he should use every means to extend his personal convictions on this point to other minds. The fact that Zeissel, up to his latest writing, always brings up this one case—which occurred over twenty-five years ago—would seem to show the material for proof of this point has not been very abundant. Finally, here in this case even more than before, we may say that, from the standpoint of Zeissel, who believes in infection through the semen, without conception, having the same abnormal course without primary or secondary symptoms, it is impossible for such a case as this to have any decided weight.

More numerous, more complete, and more cautiously collected observations are therefore necessary to a convincing proof of this method of infection, and particularly of this abnormal course of syphilitic disease.

3. *That form of syphilitic infection proceeding from the fetus to the mother, which, without any positive manifestations, makes itself known only by an immunity against new specific infection.*

Those who, of late, have come out decidedly in favor of this form of syphilitic infection are Caspary, Hutchinson, Zeissel, and J. Neumann. Caspary's view is this: We have already seen that in one case where a woman had borne a syphilitic child, and in another where a woman conceived from a syphilitic man and then aborted, he had, after most careful examination, declared both women entirely free from any sign of syphilis. But since such women are not infected by their own syphilitic children, and since an inoculation attempted by him in one such case proved completely negative, he therefore believes that the mothers of syphilitic children, even when free from all symptoms of the disease, are not to be considered healthy. He gives no credence to the other forms of retro-infection.

(To be continued.)

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF NEW YORK.

Meeting, December 4th, 1883.

SARCOMA OF THE KIDNEY IN THE INFANT.

DR. JACOBI presented a large sarcomatous tumor, involving the right kidney, removed from a child about two years of age, which recently died in Bellevue Hospital. The patient was admitted

during his absence in the summer of the present year, at which time it was very much emaciated, and a tumor was recognized in the right side. It was reported that the tumor had been first recognized only a week previous to admission. The growth increased somewhat in size during the next three or four weeks. The patient, however, improved a little and was less anemic. When Dr. Jacobi saw the patient, the 1st of October, there was a large tumor in the right side, over which there was dulness on percussion, extending from the region of the right kidney downward and forward. Between this tumor and the liver he was able to discover an area of about half an inch of tympanitic resonance. The physical signs, and the fact that the child had somewhat improved, pointed rather to sarcoma than to carcinoma. The abdominal veins were considerably enlarged, and the patient suffered a good deal from dyspnea. On the 2d there existed a slight amount of pneumonia on the right side, and on the 3d, there being signs of pleuritic effusion and the dyspnea becoming worse, five ounces and a half of bloody serum were withdrawn from the pleural cavity. Dyspnea was relieved, and the child became comfortable. The tumor remained stationary for a week or two, and then grew rapidly and became softer. There had always been slight semi-fluctuation over the region of the tumor, as usual with large sarcomata, but one day during the last week of life decided fluctuation was found. Some decomposed bloody fluid was withdrawn with the exploring needle. The child died without any return of the pleuritic effusion.

The tumor had been examined by Dr. Prudden, who had kindly furnished the following report:

Report of Autopsy.—Body much emaciated. On the right side was a large tumor, in general firm, but in places soft, filling the right side of the abdomen, and bulging out its walls laterally and anteriorly. On opening the abdominal cavity a large encapsulated tumor presented itself, nearly filling the abdominal cavity and firmly attached over the region of the right kidney, and slightly adherent to the right abdominal wall. The ascending colon ran obliquely over its lower left segment, and the remainder of the gut was crowded closely into the left side of the abdominal cavity.

On the anterior surface of the tumor the wall was thin and flaccid, and an incision opened into a cavity containing about half a pint of brownish-red fluid of ropy consistence. [Microscopical examination of this fluid showed numerous larger and smaller spheroidal cells greatly degenerated, and much granular detritus.]

The tumor was firmly adherent above the liver. The diaphragm on both sides was crowded up to the bottom of the third intercostal space. Spleen and left kidney normal. Left ureter slightly dilated.

On being removed, the tumor measured 25 cm. in length, 15 cm. in breadth, and 12 cm. in thickness.

On the inner posterior surface the slightly dilated ureter entered the tumor at a point of slight depression. It terminated in a mass of connective tissue, which was hollow, the cavity having the form of distorted calyces.

Around the above depression in the surface of the kidney was a crescentic mass of tissue about five mm. thick, which, on microscopical examination, proved to be compressed kidney tissue.

The bulk of the tumor was coarsely nodular, whitish, very soft, in most parts almost diffuent.

There were numerous small blood extravasations in various parts. Near the centre of the tumor was a large blood-clot, about five cm. in diameter.

In addition to the anterior thin-walled cyst was a smaller one near the posterior surface, filled with the same brownish-red, ropy fluid. The two cysts had the appearance of having been formed by simple softening of the tumor tissue.

The stomach contained a considerable quantity of tenacious mucus.

The opening of the thorax was not permitted by the friends, but the organs were removed from below. The heart was normal. The lungs were somewhat adherent, but otherwise normal.

Microscopical examination of the tumor showed it to be a *round-celled sarcoma*.

Dr. Jacobi quoted from the proceedings of the Society for 1874 and 1881, when he presented cases of sarcoma of the kidney and made remarks. In 1874 he had been unable to discover the records of similar cases. In 1881 he himself had seen several cases, and in literature he found the record of nine cases. At present, looking over his case-book, he found the record of about fourteen or fifteen cases in all of sarcoma of the kidney. It would seem, therefore, that the condition was not very rare, and that it had not been observed before simply because attention had not been called to it. The true condition had probably been overlooked in many instances, and called mesenteric tabes, etc. However, Dr. A. Seibert, of this city, was about collecting and recording the statistics of primary tumors of the kidney, and, while he met with but few sarcomata in literature, he could compile about fifty cases of carcinoma of the kidney, including a case in his own practice. Dr. Jacobi had seen but very few cases of carcinoma of the kidney compared with sarcoma, and thought that, after all, the observations and records were too few to permit of statistical reliability.

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ORIGINAL COMMUNICATIONS.

SOME OBSERVATIONS ON THE DIAGNOSIS OF PREGNANCY
IN ITS EARLY STAGE, WITH SPECIAL REFERENCE TO TEM-
PERATURE OF THE GENITAL CANAL.

BY

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THE symptoms and signs of pregnancy have been a subject of study for centuries past, and while it is true that investigations have yielded reward by the acquisition of many valuable landmarks indicative of that state, it is, perhaps, equally true that no other medical question has ever brought forth such a prolific offspring of ridiculous literature.

The writings of the ancients upon the subject are interesting more on account of their utter absurdity than of any practical value, but still we can yet trace the imprint of their ideas upon the superstitious minds of the lower classes. Many a woman lives with the belief that her doctor can tell when she is pregnant by simply looking in her eyes, or by feeling her pulse.

However gratifying to ourselves may be the consciousness of thinking that we are credited with the possession of such preternatural knowledge, the truth demands us to admit, on

the contrary, that the want of a positive means to diagnosticate this condition is one of the weakest points of our armor. When utero gestation has advanced sufficiently for us to detect *ballottement*, or to hear the uterine *souffle*, or the pulsations of the fetal heart, the diagnosis is rendered more easy; but even under such advantageous circumstances many and grievous are the errors that have been committed.

Pregnancy has been mistaken for other conditions, and they in turn have been mistaken for pregnancy. Pregnant women have suffered capital punishment after having been examined and pronounced "not big with child;" the trocar has been thrust into the gravid uterus under the supposition that the case was one of ascites; and perhaps no sadder case is recorded than that by Bedford, of the young lady who was driven from her native land to die an outcast, because scandal accused her of being a victim of seduction, and because medical opinion failed to establish her innocence by the recognition of a uterine tumor.

Aside from the consideration of such unhappy instances as these—than which no others tend to bring so great discredit upon our profession, the subject is of practical interest, because it is an almost every-day occurrence to be confronted and perplexed by cases of doubtful pregnancy. The earlier in gestation that the question comes up for solution the more difficult it is to be solved.

The symptoms and signs are then fewer and more uncertain. Only a few years ago, Dr. J. T. Johnson, of Washington, read a paper before the American Medical Association upon this subject.

He stated that his object would be accomplished if such a discussion of the signs of pregnancy in the early months be elicited as would place upon record some more certain way than is now generally known of determining this very important condition.

Unfortunately the discussion that followed added but little or nothing to the list of signs that was enumerated by the author of the paper, although there were present at the meeting some of the best men of the medical profession in this country.

The search for the key to this problem still continues and,

as yet, it has been as barren of result as that of the alchemist for the elixir of life. Enthusiastic men are constantly starting the medical world by the publication of the discovery of an infallible sign, but, by subsequent investigation, it either turns out worthless, or else, being of slight value, is relegated to its proper sphere of mediocrity.

An evidence of the diligence of this search may be deduced by noting the frequency with which old and wellnigh forgotten signs are dug up; the dust of oblivion is shaken from them, and they are boiled over, and served in dishes more palatable to the cultivated taste of recent research.

The writer's attention was attracted to this subject recently by the appearance in print of Jorissenne's sign, by the aid of which he claimed to have recognized the existence of pregnancy when there was no evidence present other than that afforded by the missing of a single menstrual period. An investigation of the subject was made by the writer upon ten women who were in the first three months of pregnancy, and the conclusion arrived at may be given by quoting from the report of these cases published in *The Medical Record* of January 6th, 1883.

"These results," *i.e.*, of the observations in the ten cases, "certainly do not place Jorissenne's sign very high in our scale of valuation. The number of observations is too limited to show exactly what, if any, degree of confidence can be placed in this sign, but its fallibility is proved beyond a doubt. Moreover, the same examinations, repeated at subsequent times, gave different results." Truly then, those results do not encourage us to expect much assistance from this sign.

At the time that these observations were made, with the view of ascertaining the value of the above sign, the writer took occasion to note some other symptoms indicative of the pregnant state that were presented by the ten cases, and the object of the present paper is to study the relation of these latter to the condition in question.

The only apology that can be offered for submitting a paper containing mostly negative results is, that those results were arrived at by original investigation, and that covers a multitude of faults. Any communication possessing material derived from such source (however humble that communication may be in itself) is more worthy of consideration, though less

brilliant, than one which is sired by the gleanings of different writers. In other words, conclusions derived from experiment are more valuable, though less sought for, than those by deduction. The science of medicine would make more rapid strides of advancement if the knowledge of it were derived to a greater extent from original investigation and less by the acceptance and reiteration of the opinions of others.

The symptoms and signs observed in the foregoing cases were *backache*, *leucorrhœa*, *the purple color of the vaginal mucous membrane*, and *the vaginal temperature*.

BACKACHE was a symptom in four multiparæ. Two had passed the first period, and two the second.

LEUCORRHEA occurred in five of the multiparous women; in two after one period, in two after two, and in one after three.

THE PURPLE COLOR OF THE VAGINAL MUCOUS MEMBRANE could be detected in four out of seven women examined for this sign.

It was present in three multiparæ after missing one catamenial period, and in one after missing two periods. It was absent in one primipara and in one multipara after the first month, and in one multipara after the second.

THE VAGINAL TEMPERATURE was ascertained in eight of the cases and was as follows:

	No. of Pregn'cy.	Duration of Pregnancy.	Time of Ob- servation.	Tempe- rature.
Case I.....	Twelfth	Fifth week...	11 A.M.	99.7°
Case II.....	Third ..	Eleventh " ..	2 P.M.	99.7°
Case V.....	Seventh	Seventh " ..	2 P.M.	99.8°
Case VI.....	First...	Sixth " ..	2 P.M.	99.8°
Case VII.....	First...	Seventh " ..	9.30 A.M.	99.8°
Case VIII.....	Tenth ..	Eleventh " ..	9.15 A.M.	99.7°
Case IX.....	Third ..	Sixth " ..	9 A.M.	99.7°
Case X.....	Fourth.	Seventh " ..	2 P.M.	99.7°

Any one of the foregoing symptoms presents little significance except when occurring coincidently with others indicative of fecundation, or when, owing to the absence of other causes, it appears to be the result of the pregnant condition *per se*.

The *backache*, *leucorrhœa*, and *purple color of the vaginal mucous membrane* are attributable to uterine congestion, and are important because directly connected with the physiological changes of gestation.

Among the earliest effects of impregnation are congestion

and hypertrophy of the uterus and its structures, and a recognition of this condition is consequently of the greatest aid in an early diagnosis of pregnancy.

The backache, which is most common during the first three months, is produced by the descent of the engorged uterus, dragging upon the utero-sacral ligaments. The leucorrhea and purple discoloration of the vagina can, like the former, be produced by uterine congestion accompanying other conditions than that of impregnation, and it is the difficulty of eliminating these other conditions that renders the congestion due to impregnation less available for diagnostic purposes.

When the physiological congestion of impregnation can be discriminated from pathological congestions, the value of this symptom will be greatly enhanced.

Physiological congestion of the womb is always active and due, as a rule, to venereal excitement, to menstruation or impregnation. No trouble attends the recognition of which of these produces the condition. Pathological congestions, on the contrary, are passive, unless due to inflammatory action.

When of the latter character, it can be usually ascertained by the accompanying symptoms of pain, tenderness, and, possibly, fever.

The pons asinorum, then, is to discover the means to differentiate the two remaining congestions: the passive pathological from the active physiological of impregnation. To do this, is to recognize pregnancy in its earliest stage. Does the clinical thermometer suggest a means to solve the problem? Passive congestion of the uterus, it might be reasoned, is not expected to be accompanied by elevation of temperature. Six ounces of blood will not make it any warmer than one ounce, unless there exists, in addition, some heat-producing power.

If the transformation of lifeless pabulum into living tissue is one factor of heat, would it be wrong to consider those active nutritive changes involved in the development of the ovum a heat-producing power?

In opposition to this, Wunderlich and others affirm that pregnancy has no effect in increasing the temperature of the genital canal, except in the last two months of utero-gestation.

The first step to be taken in an investigation of this kind is to ascertain positively the normal temperature of the vagina.

Our authorities have placed it at about seven-tenths higher than that of the axilla, and reckoning this at 98.6° , we have, for the normal heat of this part, a temperature of 99.3° . Accepting this as correct, all of the cases here reported registered an increase of from four to five-tenths of a degree above that point. With the exception of slight nausea complained of by one, this increased (?) heat and absence of the menses were the only symptoms of pregnancy the two primiparæ offered.

CASE VI. will be briefly stated: D., white, single, æt. seventeen, presented herself September 22d, 1882, with the statement that her catamenial discharge had failed to appear, and that it was then four days after the proper time. She felt well and indeed looked the picture of health. Her menses came on when she was fourteen years old, and had recurred every month since without a single exception. There were no sympathetic disturbances or other evidences of impregnation. A placebo was prescribed and she went away with instructions to report in about ten days. On returning at the specified time, she made complaint of slight nausea and, expressing great uneasiness about her condition, consented to have a vaginal examination made. By Jorissen's test, the pulse was 60 in a recumbent position, 66 sitting, and 70 standing. The vaginal examination was negative. The vaginal temperature 99.8° .

No accusations were made until, missing the menstrual periods of October and November, she was informed of her condition. Several months later, she had criminal abortion performed.

At first sight the thermometrical observations here reported would tend to encourage the belief that a diagnosis of pregnancy in its early stage can be readily made by this means. But while different writers agree that the average temperature of the vagina in non-pregnant women is about what has been given, many, on the other hand, allow wide limits within the range of health. It is not stated why the *normal* (?) temperature of this part of the body should vary more than that of other parts: why it cannot be fixed as accurately as is that of the axilla. The explanation is probably because the opportunity for taking the temperature of this part in *perfectly healthy women* is seldom offered. In fact, there are comparatively few women at the present day who do not have some pathological condition of the pelvic organs that would produce an alteration of local temperature. In order to obtain information upon this point, the writer made some observations to ascertain the temperature of the vagina in non-pregnant

women. The number of cases in all was thirteen. The minimum temperature registered was 98.4° : the maximum, 100.1° ; and the average of all the cases, 99.1° .

The average vaginal temperature of the pregnant cases was 99.74° . The heat of the vagina in these latter was very uniform, the variation being, in the eight cases, only one-tenth of a degree.

The temperature of the cavity of the cervix was compared with that of the vagina in about half of the cases, both of the pregnant and non-pregnant series. In a few cases the two temperatures were the same; in the remaining the difference was from two to eight tenths of a degree, in favor of the cervical cavity. The increase was about as great in one class as in the other.

The result of these observations¹ is to show that although the average vaginal temperature of a number of pregnant women is greater than the average of a number of non-pregnant cases, still individual cases of the latter class may register a temperature equal to that of the former.

One woman, two months after confinement, had a vaginal temperature of 99.9° : another, in whom no pathological condition of the genital organs could be discovered, registered 100° ; and a third, having retroversion of the uterus, reached 100.1° .

It was observed that all the cases having retroversion were accompanied by higher temperatures than the other cases of the same class.

A vaginal temperature of 100° had been reduced in five months to 99.2° , on account of the reposition of the organ and the application of a pessary.

Eliminating the cases having this pathological condition, the average temperature of the non-pregnant women would be brought down to 98.5° .

The following case bears special interest on this point.

Mrs. M., married, and a prolific mother, menstruated Decem-

¹ In taking the temperatures that have been recorded in these observations, the bulb of the thermometer was pushed deeply into the vaginal pouch, and in no case was it allowed to remain *in situ* for less than eight minutes. The instrument used was tested at the Laboratory of the Surgeon-General's office.

ber 10th, 1883. January, 1884, she missed her courses, and on the 19th of the month her vaginal temperature was 99.4° , and the cervical 100° . February 10th, she succeeded, by taking teas, etc., in bringing away the ovum. March 10th, menstruated slightly. April 27th, menses in full amount. May 13th, vaginal and uterine temperatures both 100° .

This case was not included in the summary, because the increased heat registered after the abortion was evidently due to the condition of the parts. There was marked retroversion, endometritis, and excoriation of the os.

Another point in this case is that, although pregnant when first seen, the vaginal temperature was only 99.4° . This is the only exception of a temperature below 99.7° , accompanying that condition, and the case came under observation too late to be placed with the series of pregnant cases.

Deductions.—If any positive sign of pregnancy in the first three months be discovered, it will, in all good reason, be found in some local alteration of the generative organs, and *not* in any reflex phenomena.

A constant, and the earliest change due to impregnation is active congestion and hypertrophy of all the uterine structures.

This hyperemia is necessary for the nutrition and growth of the ovum, and for the physiological development of the uterus which is to accommodate itself to the growing, and to expel the full-grown fetus.

The developmental process of the ovum and its habitat is accompanied by increased intrauterine temperature.

This increased intrauterine temperature produces some elevation of vaginal temperature.

Pathological conditions of the uterus produce an equal increase of heat in the vagina.

Might not more accurate results be obtained by using the thermometer differently, or by the use of a more delicate instrument than the ordinary clinical thermometer? If heat is generated by the developing ovum, it would be more appreciable at the upper part or body of the uterus, but, on account of the inaccessibility of this portion for examination, we have been forced to depend upon having the temperature of the vagina or cervix uteri increased by the heated blood brought down in the uterine veins. A glance at the anatomical distri-

bution of the vessels of the uterus will show that the greater part of the blood is brought to, and conveyed from the upper part of the organ by the ovarian vessels, and consequently does not pass to the vagina.

The bulb of a long thermometer might be passed through the anus, and applied to the posterior surface of the body of the uterus from the rectal side of the organ. The descent of the womb during the first months of gestation would render such a procedure less difficult than is the case at other times. Or possibly an instrument properly constructed could be brought in contact with the anterior wall of the uterus, by passing it through the urethra and into the bladder.

In conclusion a brief reference may be made to an article on the subject of uterine thermometry by Dr. Marduel.¹

Baerensprung, he says, was probably the first to make any researches upon the temperature of the fetus, which he stated to be higher than the mother's. Schaeffer confirmed these observations by taking the temperature of the infant, as Baerensprung had done, immediately after its birth. Wurster found the rectal temperature of a fetus taken during labor in breech presentation to be $\frac{9}{10}$ higher than the temperature of the mother's vagina. As a result of his investigations previous to 1872, he concluded that the fetus has a temperature more elevated than that of the mother, and that the gravid uterus has a higher temperature than that of either the axilla or vagina. He thought that this fact might serve to indicate the life or death of the fetus *in utero* as well as be diagnostic of pregnancy.

Alexeeff confirmed the observations of Wurster. He took the fetal temperature during eight cases of labor, the rectal in four breech presentations and the buccal in the same number of presentations of the face. The fetal temperature was higher than that of the mother's vagina, from $1\frac{1}{4}$ to 2.8 in the rectum, and from $\frac{5}{10}$ to $1\frac{1}{4}$ in the mouth.

In 1866 Schroeder wrote that the temperature of the gravid uterus was .34 higher than in the vagina and $\frac{5}{10}$ than in the

¹ "De la Thermométrie utérine comme Moyen de Diagnostic de la Grossesse ainsi que de la vie du Fœtus. D'après les Travaux de Cohnstein, Fehling, Schlesinger et Alexeeff."

Mém. Soc. de Sc. Méd. de Lyon (1876), 1877, xvi., p. 103.

axilla. Winckel gives for the first a difference from $\frac{2}{10}$ to .34. Cohnstein published an article in 1872, showing that after the death of the fetus the temperature of the uterus dropped. He stated that an elevation of temperature of the uterus above that of other internal organs was a proof of the existence of pregnancy, and a proof of great value in the first three months.

Eighteen observations at the Obstetric Clinic of Leipzig, made by Fehling, went to confirm the views of Cohnstein regarding an increased temperature indicating the life of the fetus, and that a temperature equal to, or, with greater reason, less than that of the vagina indicated its death. Schlesinger doubted the accuracy of the above observations, and stated that he found the temperature of the non-gravid uterus was likewise higher than that of the vagina.

Cohnstein claimed that such cases were due to the existence of an acute inflammation of the parts, and that in general he believed an elevated uterine temperature indicated pregnancy. On the other hand, that which is important in a diagnostic point of view is, that the temperature of the uterus does not exceed that of the vagina in cases of uterine fibroids, in chronic uterine infarctus, ovarian tumors or increase in size of the abdomen due to accumulation of gas.

It is proved, he says, that a uterine temperature higher than that of the vagina, in the absence of pathological conditions, is a sign of pregnancy, and of pregnancy with a living child.

Several of the cases observed by the writer disproved this assertion. For instance, one case, in whom no pathological condition could be found, and who was not pregnant, had a vaginal temperature of 99.2° , and an intracervical of 99.9° . It could be said with more truth that a vaginal temperature equal to, or more than 99.7° , is a strong presumptive evidence of impregnation, provided there are no pathological conditions of the uterus present, and no increase of heat in the axilla.

A CASE OF RUPTURE OF AN OVARIAN CYST INTO THE
INTESTINES.

BY

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THE subject of the management of cases of ovarian tumors which are under preparation for operation is one of great importance. As patients outside of large cities are often under the care of general practitioners, it is very important that all bold measures and violent treatment should be strongly discouraged, or, at least, should never be resorted to without the knowledge and advice of the future operator. Active purgation is always a dangerous practice when the abdomen is occupied by a cystic tumor. Sir Spencer Wells alludes to the fact that rupture of the cyst may result from purging. In connection with this, I have thought the following case an instructive example of the danger to life resulting from the use of purgatives during the existence of cystic disease of the ovary.

Mrs. S., aged thirty-one, had been twice married and had given birth to three children. She had one child by the first marriage. About two years ago, she noticed for the first time the enlargement of the abdomen which she thought was greater on the left than on the right side, and the swelling was accompanied by abdominal pains. During the two years while the tumor was gradually increasing in size, she gave birth to two children at an interval of eleven months. The last child was born September 21st, 1883, and during the pregnancy the suffering from abdominal pains was very great. The labor was natural, but the tumor had grown to such a size that there was little appreciable decrease in the abdominal enlargement after the confinement.

In October, 1883, Dr. M. Emma Robinson, of Salem, N. J., first consulted me about the case. She had been called in consultation and had made the diagnosis of ovarian tumor. She found that the involution of the uterus had been very slow, the womb at that time, several weeks after the labor, was appreciated as a firm body in front of the tumor, the fundus reaching a point midway between the umbilicus and pubic symphysis. The patient was very weak and was regaining her strength very slowly. Dr. Robinson desired me to operate, but we decided to wait until

the puerperal changes should be completed, and in the mean time to keep the patient under general tonic treatment and to select the most favorable time for the operation. In the early part of December, everything was favorable, but my absence from home made a postponement of the operation necessary. Dr. Robinson frequently saw the patient in consultation, but she had not the regular charge of the case. While awaiting my return, in the latter part of December, there was a most unfortunate occurrence, and it is this with its results which makes the case of such special interest. On the 24th of December, in order to relieve the constipation, the attending physician ordered two compound cathartic pills and an enema. Excessive purgation was the result, with great tenesmus, severe abdominal pains, and vomiting. The diarrhea lasted two days, the stools were large in amount, quite liquid and dark in color. In this condition of collapse, Dr. Robinson was called again to see the patient, the medication having been conducted without her knowledge. The patient rallied slowly under the free use of stimulants, and she was relieved of much of the oppression from which she had suffered previously. The girth, by measurement, had diminished ten inches.

Upon my return home, January 1st, 1884, it was decided to bring the patient to the Woman's Hospital for operation, but she was so weak that the journey was postponed from day to day, and she did not reach the hospital until January 11th. I saw her for the first time on January 12th. Her condition at that time was very discouraging. There was extreme emaciation, the skin was dry and yellow, the pulse was 120 per minute, the temperature 102°, and the respirations 29 in the minute. The abdominal enlargement gave a measurement of thirty-four inches on a line horizontal with the umbilicus. On inspection, there was nothing to question the diagnosis, but on palpation and percussion there was no tumor to be found. The abdomen was resonant throughout. I made a careful examination both over the abdomen and by the vagina. The uterus was found normal in size and occupying its ordinary position. The case seemed one of peritonitis, and if I had not had the previous history, I never should have suspected the existence of a tumor. It is due to Dr. Robinson to state that, at her visit, she had diagnosticated on ovarian cyst and had maintained this diagnosis against opposite opinions of other physicians of Salem. In consideration of this view of the case, I thought it possible that a small tumor might exist and be concealed by a covering of adherent intestines, but etherization was no aid to the diagnosis, there was absolutely no tumor to be found. At first I decided to make an exploratory abdominal incision as a means of further investigation, being influenced by a recent statement made in my presence by an eminent English operator that an abdominal incision per se is without danger; but upon further deliberation I decided against this measure on account of the existing peritoneal inflammation.

The patient lived until the last of March. She had frequent attacks of peritonitis accompanied by diarrhea, and notwithstand-

ing this condition of the bowels, there appeared a few days before death a tumor in the umbilical and hypogastric regions.

The *autopsy* was made upon the day following death. The ordinary abdominal incision opened at once into a mass of fecal matter, pale yellow in color, of an offensive odor, and of the consistence of mush. The mass was encysted, was of the size of a cocoanut, and occupied the middle of the abdomen below the umbilicus. It seemed at first that we had opened the intestine, but upon further examination the covering of the tumor was found to be a sac connected by the left Fallopian tube, and the left broad ligament to the uterus. The right ovary was also the seat of beginning cystic degeneration. The tumor was more closely connected with the cecum than any other part of the intestinal tract. The peritoneal adhesions were numerous, the anterior portion of the cyst was tightly adherent to the abdominal wall, and there were adhesions connecting it with the fundus of the uterus. The intestines were firmly fastened together.

From the history of the case, the course of the disease, and from the autopsy we draw the following deductions. A cyst of the left ovary had formed firm inflammatory adhesions to the intestines, and during the violent action of the cathartic, it had ruptured into the intestines, and through this intestinal opening it had emptied its contents, giving rise to the frequent and large watery stools. The communication thus formed between the tumor and the bowel allowed at first the entrance of gas from the intestine into the sac, and afterwards the passage of fecal matter, and as the sinus was more or less tortuous, the fecal accumulation was very gradual. This would explain the marked resonance of the abdomen in January, and the appearance of a tumor of doughy sensation shortly before death.

A PECULIAR ACEPHALOUS MONSTER.

BY

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New York.

IN November last, I was called to attend Mrs. P., of this city, for the fifth time in labor. Her previous deliveries had been normal. On arriving at the house, I found a child of about seven months' development already born. It was still unattended, but breathing well, and the cord was therefore immediately cut, and

the child handed over to the nurse. An examination of mother was then made, and the placenta was found over and partly in the pubic arch.

Behind and above the placenta, another child was found, presenting by, apparently, a breech.

The placenta was then with some difficulty pushed upward and aside, so as to allow of the descent of number two, and the consummation of labor which took place in about four hours after I reached the bed-side.

The first child, a boy, was well formed, though quite feeble, and rather small for the supposed stage of development.

It was, however, placed immediately in raw cotton and an attempt made to nourish it, but it gradually failed and perished on the fourth day from inanition.

The second one born, also a male, which proved to be an *acephalobrachus* monster, was heavier than the first, and appeared, as it emerged from the canal, *tightly enveloped* in its quite strong membrane, like an enormous kidney.

The sack contained no amniotic fluid and was somewhat adherent from the sebaceous material underneath, making it necessary to tear off the membrane with the fingers.

The placenta of the living child was then quite readily delivered and without laceration; it was large, considerably larger than the average normal placenta, circular, and with a somewhat caulflowered edge.

The cord was unusually short; after cutting it, as is my custom, about an inch and a half to the child, it hardly reached beyond the vulva.

The second cord was of full length and normal thickness. Following this funis upward, the second placenta was found high up, and to the right side of the *fundus uteri*. This one, too, was easily delivered, and without laceration.

It was about the size and shape of a normal human kidney, and appeared healthy with the exception of what felt like two or three calcareous deposits in it. It was not further examined.

The monster was placed in alcohol, and a few days later, at the request of Prof. Lusk, was laid open before the class at Bellevue Hospital and a partial dissection made. It was afterwards completed by Prof. W. E. Welch (to whom I take great pleasure in acknowledging my obligations), with the following result: In cutting through the integument, which presented the usual goose-like infiltrated condition, due to the imperfect circulation at the upper part of the body, osseous tissue was found. It was a small cranium of about the size of a walnut, covered by a skin, with a few stunted hairs on it, and contained what proved to be, on examination by microscope, brain tissue.

The intestinal tract began superiorly in a small sac, and was well developed to within an inch of imperforate anus, where it ended in a small pouch.

Of the organs, one kidney was normal and the other was rudimentary. No liver or spleen or lungs were found. The heart

was developed to about two months, as it had already divided into two cavities, the line of division between the ventricles being quite distinct. The arterial system was well developed and easily traced upward, till, by its bifurcations, the filaments were lost in the integument, and downward to femoral. The accompanying veins were also distinct and quite complete.

The theory of the origin and growth, generally received, of these monsters is well and very clearly set forth by Prof. Lusk, in his work on obstetrics:¹

"It is developed simultaneously with a normal fetus, and is usually born after the latter. Its development occurs after this manner: The balance of circulation in the anastomosing vascular system of twins contained in a single chorion (and therefore of the same sex) becomes disturbed, and the pressure in one system so preponderates over that in the other that the circulation of the latter is reversed, and its heart, lungs, and body atrophy. It now receives its nutritive supplies from the normal fetus."

On the same subject he states further:²

"Anastomosis of greater or less extent exists between the placental vessels of the two embryos.

"The consequences of their communications are of the utmost importance; for, when extensive, the heart's action in one fetus counterbalances that of the other, the stronger blood-current in the placenta pushes back the weaker one, at first impeding the circulation of the less favored fetus, then arresting it, and finally causing it to reverse its direction. The heart atrophies, and an acardia is produced which is simply an appendage to the healthy fetus."

This solution of the problem has been very widely accepted by physiologists and obstetricians, as well as *the theory of the contiguity* of the uterine and placental vascular systems, *upon which it seems to be based.*

There are, however, many careful observers who do not, at this time, fully concur in this view of the fetal blood supply, but follow, wholly or in part, the Hunters, in the belief of a closer relation, in fact that there exists a *continuity*, or intermingling of the uterine and placental systems of vascular supply.

Prof. Turner is one of the strong advocates of this position. After discussing³ his microscopical examinations and the experiments he made by injecting the vessels with a colored solu-

¹ Lusk's Obst., page 519.

² Page 222.

³ Brit. and For. Rev., vol. ii., p. 522.

tion, concludes "that these observations placed the question of the existence of an intra-placental circulation of maternal blood in the category of established facts."

In the discussion before the Royal Society,¹ he concludes his address by stating: "I have carefully examined both views and satisfied myself of the truth of the Hunterian theory."

Dr. Flint, Jr., says:²

"The important point in the determination of the connection of what may be termed the placental maternal sinuses with the vessels of the uterus, can be settled by injection of the uterine vessels, in cases in which the observation can be made while the placenta is still attached to the uterine walls. Prof. Dalton, since 1853, has examined the parts *in situ* in four cases of women who died undelivered at or near the full term of pregnancy, and he adopted the ingenious expedient of filling the uterine vessels with air,³ by which the course of the injection could be distinctly observed. By this process, the venous sinuses of the uterus itself are first filled, next the deeper portions of the placenta, and finally the bubbles of air insinuate themselves everywhere between the fetal tufts, and appear in the most superficial portions of the placenta, immediately underneath the transparent chorion. If the chorion be now divided at any point, by an incision passing merely through its own thickness, the air, which was confined beneath it in the placental sinuses, will escape and rise in bubbles to the surface of the water.

"Such an experiment shows conclusively that the placental sinuses communicate freely with the uterine walls, occupy the entire thickness of the placenta, and are equally extensive with the tufts of the fetal chorion. That the uterine vessels, as they penetrate the placenta, have an exceeding oblique direction, and that their orifices may be easily overlooked is true, but they can be seen by careful inspection.

"We have no doubt of the accuracy of Prof. Dalton's observations, and, in corroboration of this, in 1864, in the presence of Professors White and Elliott, the uterus, with the placenta attached, of a woman who died in the last months of pregnancy was examined," etc. (here he details the experiment and the results), and then concludes: "We must consider the fact of an intra-placental circulation of maternal blood as definitively settled."

Cazeaux speaks of the "exceedingly thin and delicate membrane" intervening between the uterine and placental vessels, but is not willing to admit that there is communication. Braxton Hicks discusses the question at length,⁴ and is confident of

¹ Trans. Roy. Soc., vol. 26-27.

² Flint's Physiol., p. 909.

³ This experiment was performed under water.

⁴ Obstet. Trans., vol. xiv.

the correctness of the contiguous theory. We have, therefore, Cazeaux, John Reid, Playfair, Braxton Hicks, Foster, and many others, sturdily maintaining this doctrine. Professor Welch, too, thinks that the mother's blood does not, but ought to show, on examination, the products of waste and effete material that characterize the placental circulations, if the two currents communicate directly. To this objection, however, it may be urged that unless the validity of the experiments of Turner, Dalton, and Flint can be successfully controverted, we may hope in the future to find a solution to this lesser problem.

If, therefore, the position be accepted that there is no communication direct between the uterine vessels and those of the fetus or placenta, it is obvious that the German hypothesis, quoted above, is the only tenable one, and the only one, thus far, offering a reasonable solution of the difficult problem.

If, however, the Hunterian or continuity theory be established—and we submit that the evidence is strongly conclusive—we have a plausible, direct route from the mother's heart to the monster, and it then appears questionable and unnecessary to go so far out of the way for a solution that is so unsatisfactory, and leaves so many questions unanswered, as the theory that these *Acardii* are dependent on an eddying current, or on the fetal hearts of their fellows, for nourishment and the respiratory processes.

And further, is it not evident that they then possess, *whether there is any connection of placenta or not*, the capacity to sustain a separate existence—the mother's heart being the *vis-a-tergo* and source of supply?

It has been the purpose of this paper to place before the profession, side by side, the opposing theories of the placental vascular supply, as well as the opposing views of the methods of development of this class of monsters, for it must be evident that the settlement of the latter problem is largely dependent on the solution of the former.

In conclusion, I may, I trust, be pardoned for re-stating the claims which the unique features of this specimen seem to sustain: (1) Its developing its cranium and brain to so late a stage [as proved by its covering, with its stunted hairs on it]; (2) Its heart of two months or over [as proved by the division in two cavities]; (3) Its having no sign

of an hepatic development; (4) and finally, its possessing the unique features of developing in its own chorion and developing its own placenta.

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UTERINE INJECTIONS FOR THE PARTURIENT.

BY

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I AM glad to see that resistance is being made in influential quarters to the practice which has lately invaded the lying-in room, of habitually throwing one wash or another up the parturient passage. Plenty of observations, it is true, are reported that would seem to give this treatment the support of experience, but he who has not learned to be somewhat distrustful of such evidence, but securely rests his convictions on so-called facts, is too credulous and easy-going for the laborious and slippery path which our art has to pursue. In medicine, as well as in jurisprudence, there are many situations where presumptions are weightier than affidavits, and I think this is one of them. Neither must the microscope and the test tube be allowed wholly to displace common sense, within whose purview the procedure under consideration peculiarly comes.

Certain strong presumptions against the propriety of this treatment seem to me to meet us at the very threshold, and to deserve, based as they are on universal law and lying within the scope of common knowledge, to be called remonstrances of Nature herself.

1. The first one is, that child-bearing, from its first grumbling pain to the conclusion of its cleansing flux, is a pure function of health, like conception, deglutition, or defecation, and no way a sickness, and that accordingly it requires from us, in the great majority of cases, no sort of help or attendance

whatsoever. In our own, as well as in the lower grades of the animal kingdom, we customarily see it gone through with without a single morbid symptom. Sickness is not an incident of the situation, but an accident; a possibility of the case, to be sure, but not a thing to be presumed and calculated on. It is, therefore, as a measure of prevention only that resort to this treatment has any pertinency. But prophylactic measures are called for and justified only when there is a reasonable ground for apprehension. No man carries about with him a crutch for fear that he may be taken lame on the way. We stiffen up the system with quinine when we visit a malarious region, but not when we travel among the Alleghanies, though even there people sometimes get fever and ague. Now, in the town of twenty-five thousand inhabitants where I practise, there has not been, to my knowledge, a single case that could be in any way construed into puerperal septicemia for many years—at least a half a dozen—and not for more than thirty an epidemic that was entitled to such designation, and I should feel myself settling below the lowest level of the quack, and be ashamed to look a neighboring woman in the face, were I now to tell my lying-in patients that they must have the vulva regularly sealed up after confinement, lest infecting vermin should enter there and breed, and the uterus well syringed out three times a day, lest it should spontaneously rot. Yet this is the practice that we are of late instructed to pursue from some high-titled chairs.

2. In the next place, without resorting to the ancient doctrine of final causes, we may certainly presume somewhat to judge of Nature's needs by her behavior. So deeply has she hatched, and so securely shut in the soil and machinery of propagation in the female, that it would not seem to be her purpose or her plan that we should intrude or meddle there, unless called upon by some abnormal and serious condition of things. No doubt the sick stomach is sometimes benefited by being washed out, but if Nature had intended this to be done every day, she would have made that organ of easier approach. Moreover, the parts of the body which are removed from ready touch are especially hurt or endangered by any handling or exposure. The skin is tougher than the peritoneum.

3. The third presumption is connected with the fact long

ago taught us by the great John Hunter, that the separation of the ovum from its temporary lodging-place in the uterus involves what is equivalent to an extensive wound, healthy and well-disposed and nigh always equal to its own cure, like the gashed bodies of Milton's celestial warriors, but still a wound. Now, consciousness itself teaches us, and the brute animals too, that a wounded part should be shielded from the least touch. Every mother finds out that her little urchin's cut or bruised finger is apt to take on bad behavior if he hurts it afresh, be it ever so slightly. Surgeons, too, now unanimously tell us to meddle with wounds as little as possible, and, after once clearing them from foreign substances and adjusting displaced parts, they practise shutting them up from touch or other offence as quickly, and keeping them thus as long as possible. How peculiarly sensitive even to the slightest shock the freshly wounded uterus is, meddling midwives have also learned, often to their great cost and pain. In the face of this universal testimony and instruction of Nature and art, we are now told to direct a female nurse (for if we do it ourselves, we must shame the very plumber in our bills), whose native incapacity and inexactness no amount of training can much help, several times a day to throw, with a force that can never be accurately measured, and at a temperature that no thermometer can always tell the suitability of, a flood of liquid, medicated, I care not how, upon and into the most sensitive organ of the human body, now strained, wounded, and sore. Of all the many forms of meddlesome midwifery, this appears to me to be the worst. It is taking a great deal of pains to do what, in the vast majority of cases, is wholly uncalled for, and what, thus habitually practised, must be a thousand times oftener harmful than beneficial.

This proceeding, against which common sense so cries aloud, is advocated on two grounds, which may be roughly described as the irrigation and the germicide theories.

Under the former of these heads, it is contended that it is a good plan frequently to wash off and wash out the decaying detritus and purged secretions of the uterus, and of the passage that leads to it, even when they exhibit no morbidity, and that whenever this exists, even in the slightest degree, such treatment is imperatively demanded. They with whom

the former of these conditions is not sufficiently met by the bare observation of Nature's ample and effective provision for what the vulgar call the mother's "cleaning," will hardly be satisfied by any ingenuities of reason. It would be quite in order for such minds next to proceed to superintending, clyster-tube in hand, our daily dejections, or teaching babies how to suck, or they might even begin their supervision of the business of propagation at conception itself, steering the bridal bed.

As to the second situation, our best means of judging of the efficacy of washes thrown into an inflamed or poisoned uterus or vagina, is by recalling what we know of their effects when applied to sores that are open to our view. The most dangerous of the puerperal affections of the womb is generally considered to be, if not identical with hospital gangrene, at least closely allied to it in kind. Can you cure that affection in the stump of a leg by any amount, or frequency, or kind of irrigation, or even favorably modify its behavior? Can you, by water or any medicated wash whatever, turn a dissecting or any other kind of poisoned and infecting wound into a healthy sore? Will cleansing and correcting ablutions arrest or even benefit diphtheria? Do surgeons find it good and curative to undo and wash out a chancre three or four times a day? Is it by such means that they treat a varicose ulcer; or does the leg rot off if, instead of often cleaning away the disordered secretions, they are shut in for many days under a close-fitting diachylon plaster? Pages might be covered with questions of this sort which the same monosyllable would constantly answer. Now, if cleansing and correcting irrigations have been of little or no use, when through the easy approach and by help of the eye they can be applied with the utmost thoroughness and exactitude, how can we claim them to be so beneficial when used in pouchy regions and on corrugated surfaces that are altogether hidden from the sight and almost from the touch, and where consequently their effective application is utterly impossible? The truth is that, with internal as well as external sores, it is only what is already effete and loosened from the living parts that can be washed away; affected and infecting tissues still cling.

Let me not be understood as denying that marked relief

from pain and discomfort, as well as lessened constitutional irritation, will often follow the use of copious washes thrown into the vagina of a parturient when detritus and excretion have accumulated there, hindered from free escape by her recumbent posture; I too have often witnessed their good effects myself. What I deny or boldly question is, that the essential character or substantial behavior of the case, especially if it be grave, can be much influenced by this procedure. The man whose rectum is loaded with feces gets great relief when they are cleared away by an enema; but his main condition, the pathology of the case, the cause of the constipation, is in no way touched.

But it is the recent growth and spread of the doctrine of the germinal and parasitic character of puerperal fevers and inflammations that have chiefly prompted the practice in question, and it is by that doctrine that it is justified. This is, of course, no place to undertake a full examination of this theory; rather is the subject likely to furnish plenty of occupation to a generation or two of doctors, as did long ago the dispute between the humoralists and the solidists, and, later, the contention between the disciples of Cullen and Brown which, starting from Edinburgh, spread through the whole medical world, leaving no monument but a rubbish of books, as very possibly will the present contention itself. Nevertheless it may be worth while very briefly to suggest or recall to attention two or three considerations on the subject, drawn from a wider view than the microscope can cover, confessing to something of that inexactness and inconclusiveness that belong to all presumptions and probabilities, as old as speculation on the subject, and therefore the more apt to be overlooked in the haste and zeal of scientific rivalry.

(1.) In the first place, it is a queer and suspicious fact that it is not for the most part in those diseases, such as syphilis and variola, which of all furnish the most certain evidence of being contagious, infectious and proliferating, that the microscope claims to have identified the elemental spores or germs by which they are conveyed and propagated, but rather in certain ailments not known, or generally considered, to be communicable, or ever yet in fact transplanted from one subject to another, such as malarial fever, phthisis pulmonalis, pneumonia, erysipelas, and so on.

(2.) These supposed germs of disease seem to be found as often as anywhere else in closed and indigenous abscesses, whither they could hardly have arrived through the air.

(3.) This certainly makes it not unlikely that they are the products and companions of morbid processes, and not their spring, fomented by them, and not their ferment.

(4.) In the next place, these microscopic organisms are almost too abounding in situations quite too various to be so altogether venomous as they are reputed. Whatever decaying exuviae or exudations of the body you examine, you are as certain to discover one or more sorts of them as to find tadpoles in a swamp. The doctrine on this subject now striving for acceptance should, in all seriousness, make us afraid to expose a cut or pimple to the air, or even to breathe.

(5.) Why is it that pneumonia, for instance, which is now claimed to be a parasitical disease, so constantly attacks only those who are enfeebled by fatigue, anxiety, over-abstinence or over-indulgence, disordered health, or other depressing influence, passing by the robust and well-cared for? Do small-pox, measles, whooping cough, wait for such favoring conditions?

The truth seems to be that we have as yet no clearer or more certain notion of the origin and laws of propagation of the diseases which from time to time and here and there invade and possess a population than had Sydenham, when he vaguely laid it all to "a certain epidemic constitution of the air." To the same conclusion came that wise man, Noah Webster, when, a hundred years ago, stimulated by a series of epidemics of yellow-fever along the New England coast, he prosecuted the most thorough research on the subject that has ever been undertaken (vide his "History of Epidemic and Pestilential Diseases"); and to the same conclusion came the American Medical Association at its last meeting. Hardly, indeed, it seems to me, will any man who has watched the invasion and career from time to time of those strange epidemical diseases, diphtheria and cerebro-spinal meningitis, for the last thirty years, dare come to a less vague determination. In what this condition of the all surrounding and invading air consists, and how it acts upon us is still, as very likely it forever will remain, a mystery. It may be like an electric storm, or it may be more analogous to a cloud of Colorado beetles.

While it is not to be disputed that in some affections, as notably in *trichina spiralis*, there is a veritable organism which, once introduced into the human system, is propagated there by ovulation or some other form of proliferation, the cases in which this has been proved, or seems most probable, are very few. We are still at liberty, or, rather, enforced to believe, that disease far oftener originates and consists in disturbed action, depraved or palsied function, in short a *modus motus*, than in any substantial *materies morbi*. The pathology of a disease shows what it has done, oftener than what it is.

Healthy life itself is but a mode of motion, "this sensible, warm motion," as Shakespeare describes it, which sickness alters, and death suspends. Is it not most likely that the virus of syphilis or variola operates rather by impressing its own misbehavior on the living tissues and the corpuscles of the blood, as spermatozoa invest the ovum with new faculties, than by any process of breeding? It is a familiar fact that atmospheric changes, even in the direction of the wind, produce marked changes of exaltation or depression in our feelings, particularly in sensitive constitutions and in certain conditions of the system. We can hardly suppose that this is done through the introduction into the body of morbidiferous germs; but I know of no solid reason for supposing the phenomenon is essentially different when a whole population is suddenly attacked by influenza, or decimated by death within twenty-four hours, as has oftener happened in the history of cholera and other pestilences.

The doctrine of the germination of diseases owns the great attraction that it easily enters into our conception, the process constantly presenting itself visibly and tangibly to our eyes in the generation of our fellow-beings, animal and vegetable. But neither, to a close scrutiny, is the other theory destitute of a like support in the analogies of nature. One strained string takes up the vibration of another; electric activity is awakened or reproduced in suitable vessels, not only by contact, but also by mere proximity of place; while every passion of the mind is stirred in sympathy's responses. Is it not conceivable that the malign potency that sometimes lurks so long in the wards of a hospital, ready on opportunity to generate puerperal or scarlet fever, is but a stored-up mode of motion, like electric

energy in Faure's battery; and that the so-called anti-septic procedures that have been found effectual in its suppression operate, not by destroying the germs of disease, but by disturbing the conditions that are necessary to such storage? The gradual decay in the malignancy of certain diseases which has been found to attend their repeated inoculation rather supports than contradicts the view of the manner of their communication here suggested; for do we not often see sensibility diminish under a repetition of shocks or influences?

The pious poet of our childhood, Dr. Isaac Watts, speaks or sings of our life as "a harp of thousand strings." Whatever the justness of the comparison in a theological point of view, it is physiologically sound. And these in numerous chords of conscious or unconscious sensibility are quick and ready, it must be, to respond in vibration to as innumerable influences, though the scrutiny of science is not yet able to measure or describe or even to identify them. The changes and fluctuations in our condition of disease or health are hardly less frequent and various than the fleeting features of the boreal meteor, nor are they less obscure with regard to the laws which, nevertheless, must strictly govern them both.

Moreover—to return to the question immediately before us—be the atmosphere ever so full of germs of disease, how are they going to get inside of the vagina and up into the uterus itself, past the guard of almost hermetical closure by coaptation and mucous packing which Nature has established at the gate? For it is not claimed, I believe, that they have the special instinct and sturdy vigor which impel and empower the spermatozoon. It certainly is not easy to imagine how they will reach that nest, where not the smallest whiff of air enters, unless the finger or the instrument of the midwife operator himself should carry them along. And yet it is lately proposed by certain teachers in this journal, in cases of abortion even, to have us two or three times a day sedulously scrape out and syringe with antiseptic washes the vagina and the womb itself, sealing up the door of entrance thither right afterwards as tight as cobbler's wax could make it, lest by the access there of septic germs the region should be converted into a reservoir of poison.

For my part, I know not whether this fear be more absurd or the practice more pernicious. I have attended more than a hundred women in miscarriages; and, though I never once used an antiseptic injection or any other, nor bothered myself much about what might be left in the uterus, often waiting on Nature for days and days for the expulsion of the placenta, I never lost a patient, nor had a serious case of septicemia. Nay, I did have one, and this is the way it happened. After the fetus was expelled, the placenta still stuck, and being unable to poke it out with my finger, and unwilling to dig it out with the curette, I left it there for three or four days, and till it began to stink, when, by a more painstaking effort, I succeeded in turning it out. The woman had not a single bad symptom, but the doctor had. I chanced to have on the forefinger a little hang-nail, so trifling that it was not thought of. Two or three days afterwards I felt a tingling there, and—to make a long story short—I was laid up for six weeks with septic phlebitis, and was thought to have a narrow escape with my life.

This incident is pregnant of instruction pertinent to our topic. If that woman's uterus, wounded by the recent separation of the placenta, could so long carry so poisonous a mass without the least harm to her health, are not the fears in such regard that harass Dr. Inglis and his fellows somewhat idle? I confess that to me the prevailing disposition to attribute puerperal diseases to the invasion of bacteria, and their kin, seems about on a par with the proneness of old women to lay all the ailments of their children to worms; the extremes of intelligence, as we know often happens in other matters, thus coming together.

However, the doctrine and the practice alike are undoubtedly but a fleeting fashion, destined soon to be discarded and forgotten, as Lister's antiseptic spray has nearly been, and the sewer-gas fury that lately possessed the faculty in our cities.

N. B.—If anything in this article should give offense to some delicate stomach as savoring of ridicule, and therefore unsuited to scientific discussion, as lately happened to some utterances in the Academy of Medicine, I beg to suggest that ridicule is a legitimate weapon of reason, and has been accepted as such from the time of Aristotle. It is the *reductio ad absurdum* of the old logicians.

[EDITOR'S NOTE.—It is almost unnecessary to say that the editor does not agree with the author of this paper in the very extreme view which he sees fit to take. One may not feel that the question of the relation of bacteria to septicemia and puerperal fever is settled, but one does not, on that account, believe in leaving a putrid placenta in the uterus after an abortion. The case the doctor cites triumphantly in support of his position, merely proves the existence of such a thing as luck. He says that "the skin is tougher than the peritoneum." Does he mean to suggest that the skin of his forefinger is more sensitive than the uterine mucous membrane of the woman from whose uterus he removed a stinking placenta? There is a limit even to ridicule.]

A CASE OF UTERINE POLYPUS REMOVED SOON AFTER DELIVERY.

BY

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I WAS recently called to a neighboring town, on a Saturday, to see Mrs. N., and learned the following history. She had been delivered of her third child on Thursday night of the preceding week. The labor was normal in every respect and she got along better during it and afterwards than either time before, and especially with less after-pains. This was the testimony of her mother and of the physician. The latter stated that the placenta was removed from the vagina without the slightest difficulty, no suspicion of anything wrong was excited, and in a few days she was dismissed. On the preceding Thursday evening, the third day before I saw her, and one week after delivery, a military company on parade fired a volley in front of her house which startled her very much. She was immediately taken with severe pains and flooding. The doctor being called, made an examination and discovered something within the uterus which seemed like a piece of retained placenta and attempted its removal, but could only succeed in getting away some pieces, and after several attempts, and the flooding continuing, he sent for me. I found the patient blanched and exceedingly feeble; her clothing and the bed gave clear evidence of the severity of the hemorrhage. Examining the hypogastric region, the fundus of the uterus was found reaching about two-thirds of the way to the umbilicus. Internally the os uteri was very patulous, and within the cavity, after removing some clots, a confused and ragged mass was discovered, somewhat firmer than placental tissue; a piece of it as

large as a hazel-nut, almost detached, was taken away and examined. Further examination showed an attachment, or short pedicle, which began a short distance within the cervix and extended up on the left side of the body farther than the finger could reach. Its diameter was judged to be about that of three fingers at the middle joint. There did not seem to be much room for doubt as to the best course to pursue; the patient was already in a precarious condition, and with the weather at extreme summer heat, her fate seemed certain if the torn and ragged mass was left. But scant time remained to return for instruments, for I had supposed a diagnosis was all that would be required, and as the next day was Sunday, there would be no trains. I succeeded in getting back before dark. The patient was under intense emotional excitement from prospect of an operation, and almost pulseless. A hypodermic injection of morphia and atropia was administered, and she was placed across the bed in the lithotomy position. Introducing three fingers into the uterus and encircling the tumor to draw it down, a piece came away which measured two and three-quarters inches in length by one and seven-eighths wide, one surface of which was smooth and regularly convex, showing the size of the whole tumor to be about that of a medium-sized orange. The uterus was then pulled down, the wire of an ecraseur passed around the pedicle and it was slowly divided. The shreds and broken pieces, of which none were as large as the one mentioned, were then cleared out, the uterus sponged with carbolyzed water and packed with marine lint dipped in the same. Stimulants were freely administered during the operation, and within an hour after she was replaced in bed she was in a fairly comfortable condition. She made an uninterrupted recovery.

Microscopic examination showed the tumor to be a fibro-myoma. That it had undergone softening was evident from the contrast between the part in the uterus and the projecting pedicle as felt after the operation.

The singular points of the case are very apparent. The great rarity of such a tumor causing no trouble during parturition and demanding operation in the puerperal state makes it deserving of record. So rare are such cases that they are not mentioned in many text-books, and although authority is against operative interference at such time, there must be exceptions to all rules, and the treatment in individual cases, as in this, must be subordinated to the condition of the tumor, possibly also of the patient.

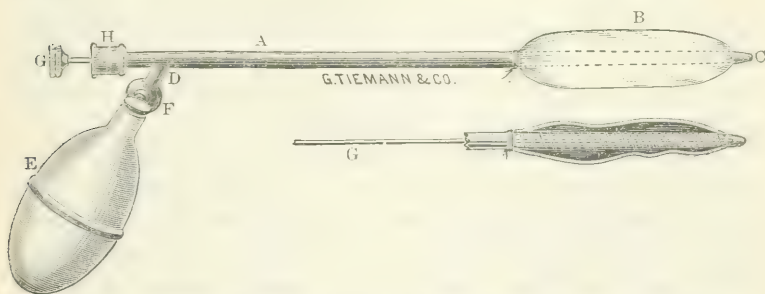
A NEW DILATOR AND CORRECTOR.

BY

C. H. OHR, M.D.,

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IN the practice of gynecology it frequently occurs that we meet with flexions of the corpus on the cervix uteri. We also meet with what, not inappropriately, may be termed a pin-hole os. This stenosed condition of the cervical canal is also commonly found at the point of curvature. A variety of appliances have been contrived for the relief of these conditions, to all of which I have met more or less objection on the part of the patient, and on my part, annoying difficulties and dissatisfaction with results. Reflection and the assistance of the Messrs. Tiemann & Co., of New York, produced the following described instrument, which they have dubbed "Ohr's Dilator and Corrector."



As will be seen in the above cut, the instrument consists of a straight tube (A) one end of which is a spiral, covered by a membranous sac (B) beyond which extends an olive-shaped point (C); this part is designated as the uterine extremity. At the extra-vaginal extremity a short tube (D) is attached, to which, when the object is to dilate, a gum bag (E) is attached, with a stop-cock at (F) to be opened or closed at the option of the operator. The membranous sac is distended by pressure on the gum bag filled with air or water; by this means dilatation of the cervix is to be accomplished. The correcting part con-

sists of the straight steel rod (G) having in the uterine end a small wheel or roller, and at the opposite end a milled head or ring screwed on the steel rod; my preference is for a ring, as by it the operator can more easily detect the course of the wheel. (H) Represents a drum or cylinder packed with hardened gum, through which the rod passes and is for the purpose of preventing the escape of the liquid.

My mode of using the instrument is to soften the sac by introducing it into warm water until it is perfectly pliable, remove the moisture by wiping it dry, smear it well with an unguent of cosmolin or vaselin and belladonna, and after inserting the olive-point into the cervical canal to withdraw the steel rod *pari passu*, as the olive-point is pushed forward into the cervical canal, and when it has reached the uterine cavity to distend the covering sack by gradually forcing *warm* water from the gum bag into the sack, giving the patient an occasional rest. After the distention of the sac, the correcting rod is pushed forward gradually, until it reaches the distal extremity of the spiral, the stop-cock being closed at each step of dilatation and before the rod is pushed forward.

The instrument, at least so far as the spiral, should be thoroughly gold-plated to prevent its oxidation, a result which has occurred to the one I have in use (nickel-plated). This stains the sac and gives it a filthy and unseemly appearance, weakens the spiral, and may cause it to break.

The sack will wear out by use and needs to be renewed. Use and observation have made me aware of certain defects which I have endeavored to correct by instructions and suggestions to Messrs. Tiemann & Co. The sack is composed of different layers of tissue united by a cement to cause them to adhere to each other; too much moistening destroys this bond and weakens the sack, requiring a more frequent renewal of the sack. The renewal is accomplished, after the removal of the former sack, by first slipping the small end of the sack over the olive-point and tying it securely in the recess, and undercut with fine strong silk; the larger end of the sack is then slipped over it to the recess at the vaginal end of the spiral and securely tied into the recess above the end of the spiral.

To illustrate its usefulness and satisfactory results, the following history is appended:

Miss E. G., a very handsome and spirituelle-looking girl of about seventeen years. February, 1883. After she had described her case and given its history, a very miserable and uncomfortable one, more especially so at the periodical time, I told her she was suffering from some uterine trouble, the nature of which I could not determine, even after my catechising, without a personal examination. The idea to her was terribly shocking and not to be submitted to; I told her it was as unpleasant to me as to her, and propounded to her these interrogatories: Have you a beau? Very modestly—Yes. Do you love him? Yes. Are you engaged to him? Yes. Then go and marry him and then come to me. No, I will not until I get well. I cannot know what the trouble is, and will not treat you without knowing. She came to see me four times afterward, and the last time with her mother, whose presence and entreaties, added to my explanations, at length induced her to submit to a digital examination.

There was some vaginitis, but no abnormality about the cervix, but an uncertainty as to the recognition of the *os*. She was told she must get on the table; a small Ferguson speculum was introduced, which revealed a *pin-hole os*. This was on the 8th of May. She was put on treatment for the vaginitis, and on the 17th a No. 2 bougie was with great difficulty introduced, and on the 19th a No. 4; on the 29th, after the menses had ceased for two days, the dilator was introduced; again on the 31st. June 6th a No. 12 bougie was easily introduced, and I told her to go home and get married as soon as possible. She complied with this injunction three weeks afterwards.

July 17th, 1884, I saw standing before me a buxom, fine-looking young matron, with a stout, hearty, three-months-old boy in her arms. When she spoke, I recognized my spirituelle patient of the year before, only increased some thirty pounds in weight.

One more case of a different character. On the 15th of April last, was called to see Mrs. B., mother of five children; had been under treatment four or five years for uterine trouble. She was confined to her bed nearly all the time; pale and almost bloodless from a fifteen weeks' continuous metrorrhagia; after a few days' treatment she had so far been relieved of that condition as to be able to be driven to my office. Examination revealed chronic endometritis with retroversion. The intervention of menstruation and her presumption on slight improvements in her condition retarded the cure of the case, the metrorrhagic condition recurring at intervals till the middle of June. On the 20th, the corrector was introduced, the uterus straightened to its normal position, and she was taught Professor H. F. Campbell's genu-pectoral position and the use of his glass tube. There has been no recurrence of hemorrhage; she has passed her monthly period naturally, and is now, July 28th, absent on a visit to friends in Ohio. She is reckless in regard to care for herself, and it is a matter of surprise to me that there has been no recurrence of the trouble as yet.

TWO INTERESTING CASES OF OVARIOTOMY.

BY

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CASE NO. 1.—*Ovariectomy complicated with an excessive curvature of the spine and deformed pelvis, succeeded by pregnancy and safe delivery to mother and child. Recovery.* Miss J. C., of German birth, thirty-seven years of age, entered the Chicago Hospital for Women and Children August 25th, 1881. She was supposed to have an enlargement of the liver and asthma. This patient was of fair, transparent complexion, dark auburn hair, and dark eyes. She was spare, with long extremities and a curvature or angle in the middle of the dorsal portion of the spine, which shortened the height by from twelve to fourteen inches. The angle of curvature was mostly posterior, but also stood two and a half inches to the left of the mesial line. The apex beat of the heart was found on a line with the left axilla and high up. The sternum was thrown forward, and shelved over an enlarged abdomen.

Her history was, that she was of a consumptive family, and that at four and six years of age respectively she had fallen downstairs. Soon after the latter accident, the beginning of the deformity, and two broken ribs were noticed, but nothing was done except to make her dress a means of concealment of the former. She had labored as a house-maid until within three days of her entrance to the hospital. The abdomen had been increasing in size the last five years.

At this time the pulse was 130 per minute, temp. 99.5° F. The renal secretion was loaded with albumen. The enlargement of the abdomen was believed to be due to an ovarian cyst.

August 27th. This diagnosis was confirmed by aspiration, and the removal of about forty-eight pounds of the characteristic gray ovarian fluid. She was now put upon iron, quinine, and good diet.

September 1st. The cyst was found to be refilling.

November 4th. Respiration had again become labored. Nov. 13th, the cyst was removed. The operation was performed under the carbolized spray. The incision was made in the median line of the abdomen, and from umbilicus to pubes. There were adhesions over the anterior and upper portions of the cyst which were broken up without injury to the viscera. The quantity of fluid removed was nearly as much as that by aspiration. It was a unilocular cyst. The pedicle was ligated with a carbolized silk braid, and dropped back into the abdominal cavity. The left

ovary was examined, found normal, and returned. The abdominal viscera and pelvic cavity were carefully sponged with warm carbolized water, and the incision closed with interrupted silver wire sutures. The Lister dressings were used. Great exhaustion followed, but she rallied as well as patients do ordinarily after ovariectomy.

On the morning of November 14th, she had rested until three o'clock, when she complained of great flatulency, and soon after nausea and vomiting occurred. This was controlled with subnitrate of bismuth and carbolic acid. The morning temperature, taken in the axilla, was 96° F. That of evening was 95° F. During the entire day there was great restlessness accompanied with cold extremities, though these were well surrounded with bottles of hot water.

November 15th. She did not rest until 4 A.M., when she began to take short naps, and continued to do so until noon, when her temperature came up one degree. Pain in the side and great flatulency recurred. There was a constant irritation of the nervous system, for which the various preparations of opium were given in vain. The pulse was small and rapid, the extremities were cold, and there was a constant tossing in the bed, with the arms thrown wildly about, though two, and at times three persons worked to quiet her. At 5 o'clock P.M., the house physician thought her dying, as she had ceased breathing, but she was restored by ammonia and other stimulants. The renal secretion was nearly normal.

November 16th. I was called at 8 o'clock A.M. because "she seemed dying." Again she had become quiet; her pulse was not perceptible, and the extremities were cold. Her eyes had become fixed, and her head rolled back. As I had considered this great irritation due to pressure of the sutures upon the peritoneum, added to that of spinal curvature, I now determined to loosen them by untwisting each one as much as possible, yet leave the wound to heal. After establishing respiration and circulation, I examined the wound and found good union throughout, and thereupon removed every suture, though it was but about sixty-four hours after the operation. The day previous I had removed all of the dressings except the green oil-silk, and applied a large hot poultice. On this day I applied large strips of adhesive plaster, over this a poultice, and outside of all a snug bandage.

November 17th. Delirium and motion were so constant that no thermometer could be used. The tongue was thickly coated with white fur. The pulse was too rapid to be counted. She remained very much in this condition until the 21st, when she occasionally answered questions intelligently. At that time the inguinal glands and leg were found to be swollen, and the renal secretion was scant, but this gradually passed away, and on the 24th, she called for solid food and improved continuously, although up to December 5th she had many delusions.

December 17th. She was discharged, and while waiting for her friends to take her away, sat by an open window, took cold,

and was sent to the surgical ward with facial erysipelas, so that her eyes were closed for five days. To fill her chapter of misfortunes, I will add that she was a few weeks subsequently readmitted for severe congestion of the kidneys, and treated in the medical ward five weeks. About the middle of December, 1882, this patient visited my office, informing me that she was sick and feared pregnancy, as she had been married about four months. I learned by examination that she was over three months advanced in gestation. I now examined the pelvis with a new interest. It was found narrowed at the brim by a flattening of the left ilium, thus shortening the transverse and the oblique diameters.

The conjugata of the outlet was but two and one-half inches, as the os coccygis was sharply flexed forward into the pelvic cavity. The transverse diameter of the outlet was shortened by the left tuberosity of the ischium extending inward from the general flattening or twisting of the os innominatum. The fetal head was small, or supposed to be at the seventh month, as it was quite movable at this date, and she was allowed to go to full term.

May 24th. I was called in the morning, when she was represented as having been in labor all night. The os was not dilated, but the tissues were thinning away. "Pains" were regular, and the os began to dilate about noon. The dilatation, however, was not complete until late in the evening. The pains now had a strong forcing character, and all the soft tissues were, by 3 o'clock A.M. of the 25th, dilated to their full extent, but the unyielding os coccygis prevented any advance of the head.

Several friends present and interested in the case agreed with me in thinking it was now time to use the forceps. In making traction, I tried to avoid the breaking or dislocating of the os coccygis by turning the occiput to right of it, but to my surprise and great fear there was an audible snap as of this bone being broken. My surprise was greater to see the occiput roll out from under the pubic bone as the anterior fontanelle had appeared to be in front. I had been deceived by the overlapping of the sutures which was so great that the posterior fontanelle could not be distinguished from the anterior.

There was a slight laceration of the perineum and considerable hemorrhage. The coccyx was freely movable, but no points of bone pricked through the tissue into vagina or rectum, though there was a transverse irregular ridge, the site of the fracture.

The laceration of the perineum was unavoidably increased somewhat by the introduction of the hand into the uterus to arrest hemorrhage. The patient was placed on her side, the wound kept clean with the carbolized douche, the knees were tied together. The perineum united so much that it was thought unnecessary to perform perineorrhaphy.

The patient was about four weeks in convalescing. There were three days, from the fourth to the seventh, in which she showed marked symptoms of septicemia and in where there were the same motions of the hands and arms and some delirium; but

it passed away and she recovered, so as to sit up between three and four weeks after labor. The child, now over eight months of age, is a strong and healthy girl. The diameters of the head were taken the day after birth, perhaps twelve hours, but I lost them and remember but one, the occipito-frontal, which was five inches. The child was small but well-formed and weighed about six pounds.

Who will tell me the cause of the great irritation of the nervous system after ovariectomy? Was it from the curvature, the operation, the sutures upon the peritoneum, or the inherited tendency to tuberculosis?

What would be the wiser way to do, in such a deformity at time of ovariectomy, to leave a healthy ovary, or remove it? What the wiser course to pursue in such deformity of the pelvis, at time of labor, perform Cesarean section or craniotomy, if it could be determined that the fetus was too large to admit of a safe birth?

CASE NO. 2.—*Ovariectomy with suppurating cyst. Recovery.*—Last June, Dr. Mary Snoddy Whetstone, physician to the Northwestern Hospital for Women and Children, summoned me to Minneapolis to perform ovariectomy, saying that the patient was declining with a septic fever and diarrhea occasioned by absorption of pus from a suppurating ovarian cyst.

Dr. Whetstone had consulted with Drs. Hance, Kimball, and Abbott, of that place, and with Dr. W. H. Byford, Jr., of Chicago, since deceased, and all agreed in the diagnosis of ovarian cyst. The prognosis as made by them was that in the case of the removal of tumor the chances for recovery or death were about even.

The history of the patient as given by Dr. Whetstone was substantially as follows: A. H., twenty-seven years old, born in Sweden, home in Montgomery, Ia. She had always enjoyed good health until the present illness, which began with an attack of malarial fever in September, 1883. Menstruation had been regular till the beginning of the fever, since which time it had entirely ceased. After convalescence from the fever, she noticed an enlargement in the right side of abdomen.

It increased in size until December 25th, 1882. At this time she was tapped, and three gallons of greenish-colored (?) pus were drawn off. This relieved the dyspnea and pressure for the time, but the tumor was soon as large as before. Seven weeks before removal of the cyst, the patient had been tapped a second time, and only four ounces of yellowish pus were drawn off. The purulent discharge had continued to flow from the wound made by the trocar.

I found the young woman a type of the Swede, of medium height, with light complexion, light hair, blue eyes, and good

teeth. She was poorly nourished, with skin somewhat cyanosed, and hectic cheeks. Her face had the peculiar anxious expression of patients affected with this malady. She could sit up but a portion of each day.

The enlargement of the abdomen extended from the pubes to the ensiform cartilage, but was greater on the right side. The abdominal walls were as tense as though not leaking several ounces of pus daily. In the median line, midway between the umbilicus and pubes, was a cicatrix, the site of the first tapping. At the left of this, about three-quarters of an inch, was an open ulcerated fistula, the site of the second tapping, and through which a drachm or two of pus flowed with each movement of the body or turning in bed; but it was noticeable that none could be expressed by pressure upon the abdominal walls. The presence of this fluid and its manner of pouring from the peritoneal cavity told us of another fistula in the cyst, and that its site was remote from that in the abdominal wall. It was also explanatory of the septic source.

June 27th this cyst was removed. My desire was to thoroughly disinfect by using the Lister method, on account of the long-continued absorption of pus and the constant saturation of the surface with the same fluid.

The room, though papered, was washed throughout—ceilings, walls, and floor—with a 2:100 solution of carbolized water. Though this hospital, under its efficient management, seemed typical of cleanliness and order, we made ourselves sure that everything was clean. On account of the diarrhea with which the patient was affected, the laxative for the previous evening was composed of rhubarb and carbonate of soda, five grains each. On the morning of the operation, both patient and nurse were ordered baths and clean linen. About 12 M. the room was closed, and filled with a spray of carbolized water of 4:100 strength. The patient was anesthetized by Dr. H. H. Kimball and Dr. Lizzie Wass. The abdomen was sponged during the time with carbolized water. I then proceeded to operate, assisted by the hospital staff, Dr. Mary Snoddy Whetstone, Dr. Mary G. Hood, Dr. Anna Wass, and Dr. Allen Whetstone.

There were present Drs. C. H. Hunter, W. A. Laton, Amanda Ranslow Hunt, and others. As the cicatrix was in the median line and the ulcerated fistula at the left, I made my incision to the right of the first point, in order to get healthy peritoneum, and from pubes to umbilicus. It passed over an area in the sac, which was made so weak by inflammation that, as I had reached it and broken up adhesions over a sufficient space to use the trocar, while I turned to take up the instrument, it burst, and pure pus welled up and flooded the fresh wound, the patient, the table, and, for a moment, threatened to drench everything in proximity to the patient. I sponged with one hand, and placed a finger of the other over the rupture in the sac; but with ever so light a touch the sac continued to tear. A portion of the fluid was removed by dropping into the cyst one end of the tubing

after the removal of the trocar. But more was enabled to flow out by turning the patient upon the side. I left a portion to distend the sac until after the breaking up of the adhesions. These had formed over the entire surface of the cyst, except a space as large as the palm of the hand posteriorly, and below in the pelvis. Even the adhesions seemed weakened over the anterior surface of the cyst as the result of the general inflammation caused by the constant steeping in pus. Other cysts were found, one so large as to require tapping before it could be removed through the wound. This contained the natural, gray, tenacious fluid.

I did not learn the combined weight of the fluid and cyst, but would estimate it at about fifty or more pounds.

The tumor grew from the right ovary, the pedicle of which was long and narrow. This was ligated with carbolized silk braid and a second ligature of catgut. Two lacerations of serous membrane were found upon the pedicle below the ligature where adhesions had occurred between it and the cyst. These were ligated with carbolized silk, and the stump dropped back into the abdominal cavity.

The left ovary was found healthy, and allowed to remain. The inner orifice of the fistula in the abdominal wall was discovered ulcerated in a circle, the diameter of which was nearly an inch. It was thoroughly sponged, as were the abdominal and pelvic cavities, with very warm, strong carbolized water, the water being several times renewed during the process. The incision was closed with ten silver wire sutures. Lister's dressings were used, also a good quantity of cotton batting and a wide flannel roller. The patient recovered from the shock of the operation as well as usual.

I am indebted to Dr. Whetstone for the full notes of the case from this time on to recovery, and from which I make the following extract:

The day after the operation, June 28th, the temperature reached 99° F., pulse 108, respiration 28. She had nausea all day. Temperature 101° F. at 4 A.M., pulse 112, respiration 30; but the temperature fell again to 98.5° F. It continued much the same until the evening of July 1st, when the temperature rose to 102.1° F., pulse 104, respiration 22. The temperature now continued to rise to 100° F. once daily until July 5th, when it gradually lowered to 98.5° F. morning, and to 100.6° F. evening temperature. It remained the same until July 12th, when it once more rose to 102.5° F., and remained at this point about twelve hours, and then gradually declined to normal. She was allowed to sit up in bed July 15th, and on the 25th went down one flight of stairs to meals. The diet was milk, beef-tea, chicken-tea, coffee, tea, porridge, etc. The medication was principally quinine, iron in the form of muriated tincture, small doses of opium, and subnitrate of bismuth for diarrhea, as it threatened to recur twice or more times.

My experience is so small as an ovariectomist, this being but

my sixth operation, that my preference for the long or short incision may have no influence upon others; but in this case I could not have well cleansed the abdominal and pelvic viscera of the thick yellow pus that had completely flooded them without this length of incision. Had it been a single cyst with a clean peritoneal cavity and few adhesions, I should prefer a short incision.

When I came to the fistula in the abdominal wall, my first impulse was to make another incision at right angles with the long one, and from it to the fistula, then cut out the ulcerated lining, and close up the whole. Though the patient's circulation seemed good, and the spray and high temperature of the room were kept up, and bottles of hot water were placed around the patient, I feared to take the time and keep the abdominal viscera longer exposed, so I sponged the fistula internally and externally once more, and closed the incision already made. The result was that the fistula closed up within a fortnight, showing the treatment to be the correct one.

As the words for gray and green in the Swedish language are so nearly similar, the question arises in my mind as to whether the patient meant to be understood as saying *gray* or *green*. Another question is, If the pus had been green, as understood by Dr. Whetstone, would it not have indicated a worse form of suppuration, and would the secretion of a pure laudable pus have followed as reported?

A RATIONAL VIEW OF PERINEAL SUPPORT.

BY

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THERE are some questions which can be profitably discussed from a theoretical point of view, and the very interesting and instructive article of Dr. McGaughey, in the June number of this JOURNAL, has suggested the following remarks, upon the management of the perineum in the second stage of labor.

That a correct theory on this subject is of great importance is shown by the variance of the deductions of those who have had a large amount of experience to guide them, but whose theories are often so unintelligible as to make it seem doubtful whether their manipulations actually produce the effect they claim for them.

Now it is evident that any kind of support, whether applied to the head of the child or to the perineum of the mother, in order to prevent injury to the latter must do one of two things, viz.: it must prevent either too great or too rapid stretching. The perineum must be distended to a certain degree in order that the head may be born; if manipulation prevents additional distention, it is manifestly of service in preventing rupture; if it cannot do this, or if it is not needed for this purpose, because if left to nature the distention will not pass beyond the minimum which allows the head to escape, then it can only be of service by preventing too rapid distention to the same degree.

The forces of nature which enter into the problem under discussion may be reduced, for convenience and distinctness, to four resultants; two are the resultants of the expulsive forces, one being downward and the other backward; the other two are resultants of the resisting forces, one being upward and the other forward. The upward resisting force yields to the downward expulsive, while the backward expulsive yields to the forward resisting, and our forces are reduced to two, downward expulsive and forward resisting. The relative strength of these two determines the direction of the final resultant, and of the progress of the fetal head.

Now, if we endeavor to protect the perineum from excessive stretching by pressing with the hand upon the head of the child, we shall direct that pressure so as to counteract the downward resultant of the expulsive forces, and aid the forward resultant of resistance. In other words, the force will be applied in such a direction that it may be resolved into two components: an upward, opposed to the downward force of nature, and a forward, aiding the forward force of nature. Let us see what good result can be accomplished by either of these components.

First, the forward force, which aids nature to carry the head

forwards. But the forward movement of the head is absolutely limited by a solid barrier, the pubic arch. If the forward force of the resisting perineum is sufficient to keep the head firmly pressed against this barrier until the occiput escapes from behind it, and while extension is taking place, it is all that is required of it, and the perineum is stretched no more than is necessary. It is plain that no additional amount of forward pressure upon the head can then diminish the degree of perineal distention, because it cannot move the head any farther in that direction. Now for one appeal to experience. Is not the resistance of the distended perineum, in almost if not every instance, more than sufficient to hold the head firmly against the pubic arch, *i. e.*, as far forward as it can possibly go? I have certainly found it so. It follows, then, that this forward pressure needs no manual assistance, and that no attempt to render such assistance by pressing upon the head can relieve the distended perineum. It may increase the pressure of the head upon the urethra, so as to injure it, as occurred in Dr. McG.'s practice, but it can accomplish no good result.¹

The effect of the other component of the external force is simply to oppose the downward expulsive forces, *i. e.*, to delay the progress of labor and postpone the moment when the perineum is put to the severest test. This is often desirable and may save the perineum by giving it time to yield more gradually to the distending process.

My point, then, is that external pressure on the head is utterly powerless to reduce the extent to which the perineum must ultimately be distended, and that it can save it only by retarding a too precipitate labor.

But supposing we do not regard the labor as unduly precipitate, and do not feel justified in retarding it, but still are anxious for the integrity of the perineum; is there no way in which we may apply pressure or support, so as to diminish the danger of such an accident? I think there is; perhaps there are several. At what point is the perineum liable to rupture?

¹ The head, held firmly against the pubic arch by the distended perineum, may be compared to a block of wood bound firmly to the under side of a beam by an elastic strap. You cannot relieve the tension of the strap by pressing upward upon the block. If you happen to be standing on your neighbor's corns, you cannot relieve him by pulling at your boot-straps.

In the median line. This is either its weakest point, or the point subjected to the greatest strain. It is to relieve the strain upon this point, therefore, that our efforts must be directed. The most obvious and rational method of doing this is to place the thumb on one side of the perineum and the fingers on the other, with a napkin between to prevent slipping, and by pressure against the head and towards the median, to drag the sides of the perineum towards the point most likely to give way, and thus relieve the strain upon that point. It may be objected that in this way the accoucheur is able to use but very little force as compared with that which drives along the oncoming head. Very true, but only a very little force is required to prevent rupture. It is the last straw that breaks the camel's back; and it is only necessary to lift this straw in order to save the back. If the perineum can be spared a very small proportion of the strain put upon it, it will be saved; but it cannot be saved by any amount of force applied in such a way as not to diminish any of that strain.

In those cases where malposition is to be corrected, and where it is deemed expedient to forcibly retard the progress of the head, Dr. McGaughey's method would seem to make the physician master of the situation, enabling him to detect quickly any occasion for interference and to act with efficiency, whether the support is to be given to the head, as he describes or to the perineum, according to Playfair's method; but in all ordinary labors not deemed precipitate, the latter seems the more rational procedure.

QUARTERLY REPORT ON OBSTETRICS AND GYNECOLOGY IN FRANCE.

BY

A. AUVARD, M.D.,

Chief of the Histological Laboratory, Clinique d'Accouchements, Paris.

I.—Up to the present, French surgery has expressed no opinion on the subject of vaginal hysterectomy in cases of cancer of the uterus. Lately, however, M. Boeckel, of Strassburg, has read a

paper before the *Société de Chirurgie* of Paris, which has provoked discussion of the subject, and forced many of the members to state their views on this important operation. Boeckel's case is as follows: A patient, aged forty years and the subject of cancer limited to the cervix. Her general health was very satisfactory. Boeckel performed vaginal hysterectomy the 26th of October, 1883. He removed with the uterus an infiltrated gland which lay in one of the cul-de-sacs. On the third day after the operation, the urine began to flow from the vagina, and on careful examination a ureteral fistula was discovered. In consequence, M. Boeckel performed nephrectomy on the side corresponding to the fistula. One month afterwards, the patient was discharged from the hospital cured; but two months afterwards, she returned with recurrence of the disease. M. Boeckel concluded from this single case that vaginal hysterectomy as a justifiable operation should be accepted with great diffidence.

MM. Verneuil, Terrier, and Polaillon pronounced themselves against total extirpation, and in favor of a partial operation whose ultimate results are apt to be as satisfactory. Hysterectomy, whose praises certain operators have sung so loudly, does not in general grant the patient a longer lease of life than would be hers were there no operative interference. For ordinarily, when left to itself, uterine cancer lasts from two and one-half to three years.

Trélat grants the theoretical good of hysterectomy in cases of cancer, but thinks that in practice an impossibility is aimed at. If the disease be in an early stage, can the diagnosis be assured? If the disease is in a more advanced stage, the time has often passed when it can be completely eradicated by the knife; therefore, the best treatment of cancer is that which watches it carefully, limits it where possible, and destroys the ulcerating surfaces. The partial operation does all this. Through this method of treatment, one may not cure, but relief is certainly given.

M. Demons (of Bordeaux) has performed the operation seven times with three deaths, and one of these deaths was due to a mistake of the operator. Of the four patients who recovered from the operation, recurrence took place in one at the end of five months, in another in about nine months; the remaining two patients were in an excellent condition eleven and eighteen months respectively after the operation. M. Demons accepts, therefore, the operation.

MM. Terrillon and Gallard speak highly of the galvano-cautery in cases of epithelioma of the cervix, at an early stage, although they recognize the disadvantages surrounding this method, in es-

pecial the danger of secondary hemorrhage. This last complication is far from being a rarity.

M. Marchand will have nothing to do with the galvano-cautery, although he recognizes the fact that by it the operation is greatly simplified. He would also look with disfavor on Chassaignac's écraseur, and pronounced himself strongly in favor of the knife.

As is readily apparent from the above discussion, which monopolized several meetings and of which I have given only a sketch, there are as many opinions as there are heads; nevertheless I would briefly state the following as being the views held in France on the subject of hysterectomy: 1. The operation is a difficult one, but practice and experience will make it a relatively easy one. 2. Theoretically it is an excellent operation. 3. Practically it can only become a generally justifiable operation when we are in a position to make a diagnosis of cancer in an early stage, and when also we are able to diminish the risks of the operation, at present so numerous.

II.—During the summer course at the *Faculté de Médecine* just completed, Professor Tarnier devoted several lectures to a study of new forceps, and these lectures have been of the greatest interest, seeing that they have emanated from a man who has devoted his whole career to a study of this instrument in order to perfect it, and who has happily attained his aim in the invention of the forceps which bears his name.

The classical forceps, that of Smellie and of Levret, said M. Tarnier, has six capital faults, six essential vices: 1. When applied directly (the concavity of the instrument facing the pubic symphysis), on making traction on the head, which is at the superior strait, or the highest portion of the excavation, the force is not along the axis, and, in consequence, a greater or less amount is uselessly expended, and the internal aspect of the symphysis is compressed to a degree which may do considerable damage to the soft parts. 2. In oblique applications (the concavity of the instrument facing one or the other side of the pelvis), the line of traction is similarly at fault: if the handles of the instrument are in the median line, then the blades, or the curve of the instrument, will come in contact with the pelvis; if, on the other hand, the handles are inclined laterally so that the blades are in the axis of the genital canal, on making traction, the sides of the pelvis are no longer an obstacle, but then the traction is not in the right direction, being oblique to the mid-plane, instead of being, as it should be, at right angles to this plane. 3. With the ordinary forceps, there are lever movements, the more dangerous

the longer the instrument. 4. With the classic forceps, the head is bruised, for with each traction the fetal head is vigorously compressed, and during the intervals the head relaxes. 5. The head once seized, lies immovable within the instrument. It cannot flex; and yet flexion is of great assistance during the passage of the head through the genital canal. 6. Finally, the head being immovable, rotation is prevented.

Now, how remedy these faults? Many and varied instruments have been invented for this purpose. M. Tarnier then described these various forceps—those of Chassagny, Laroyenne, Hermann, Moralès, and eventually gave a description of his own, and they are too familiar to my readers to require comment from me.

This instrument of Tarnier avoids all the disadvantages noted above and realizes to perfection the aim of which many another instrument has fallen short. In short, Tarnier's forceps: 1. When applied directly, traction can be made in the correct axis. 2. When applied obliquely, traction can be made as well in the axis as in the mid-plane. 3. Thanks to the traction handles, there can be no lever movement. 4. The compression screw prevents the bruising due to alternate compression and relaxation. 5. The head may flex and extend at will. 6. Finally, when the forceps are in place, rotation may take place irrespective of traction.

III.—As we have been asked by a number of foreign physicians interested particularly in gynecology and obstetrics for information in regard to the most opportune time and places for studying these branches in Paris, I may perhaps render my associates a service by giving the following details:

In France, obstetrics and gynecology are separate branches. The maternity hospitals in Paris are:

Clinique d'Accouchements, Prof. Pajot.

Maternité de Paris, Prof. Tarnier.

Hôpital Cochin, Dr. Marchand.

Hôpital de la Charité, Dr. Budin.

Hôpital Lariboisière, Dr. Pinard.

Hôpital St. Louis, Dr. Porak.

Hôpital Beaujon, Dr. Ribemont.

Hôpital Tenon, Dr. Maygrier.

During the entire year, both foreign and French students may attend the above hospitals, and one season is as good as another for seeing practical work.

As for gynecology, it will be found scattered throughout all the surgical divisions, and, to mention only those gentlemen who devote themselves particularly to it:

. Hôpital de la Charité, Prof. Trelat.

Hôpital de Lourcine, Dr. Pozzi.

Hôpital Bichat, Dr. Terrier.

Hôpital de Salpêtrière, Dr. Terrillon.

The most favorable time for study in this subject is from November 1st to July 1st. During July, August, September, and October, many of the surgeons are off on their vacations; and some of them about a fortnight at Easter time also. So much for clinical instruction. Theoretical instruction is given officially at the *Ecole de Médecine* and in the private amphitheatres of numerous accoucheurs. The official instruction is given during the winter term by an adjunct-professor three times each week; during the summer term by the Professor of the Theory of Obstetrics (M. Tarnier) and by an adjunct-professor. Private instruction consists in free and in pay courses. In gynecology, most of the courses are free and are given particularly at the *Ecole pratique de la faculté*. In Obstetrics, there are a number of pay courses, lasting for about two months, and in which, in addition to theoretical instruction, lying-in women are examined and operations are performed on the manikin.

This instruction, I repeat, is only given between November and July, and so many gynecologists are disappointed with their stay in Paris, when they come during the months which are given up to vacations. These few remarks may, we hope, prove of use to those who, for the sake of obstetrics and gynecology, come to Paris for instruction.

TRANSACTIONS OF THE AMERICAN GYNECOLOGICAL SOCIETY.

(A B S T R A C T.)

NINTH ANNUAL MEETING.

HELD IN CHICAGO, SEPTEMBER 30TH AND OCTOBER 1ST AND 2D, 1884.

First Day—Morning Session.

THE Society met at the Palmer House, Tuesday, September 30th, and was called to order at 10 A.M. by the President, DR. ALBERT H. SMITH, of Philadelphia.

THE ADDRESS OF WELCOME

was given by DR. W. H. BYFORD, of Chicago.

MOOT POINTS IN REGARD TO INVERSION OF THE UTERUS.

DR. JOHN C. REEVE, of Dayton, O., read a paper on the above subject, in which he directed attention first to the rarity of the occurrence of the accident—once in 140,000 to 150,000 labors. The greatest and most gratifying progress which has been made with regard to this affection is in reference to treatment upon which he did not propose to enter.

The etiology may now be said to be clearly understood. There is now no respectable authority which does not teach that it may occur independent of anything done or omitted to be done by the accoucheur, although it is to be feared that this truth is not yet generally recognized by the profession.

The object of the paper was to present a few points with regard to inversion which do not yet seem to be settled, or upon which opinions are divided.

1. *Can inversion of the uterus occur entirely independently of pregnancy or polypus, or even in the nulliparous organ?*

The conditions necessary to inversion are sudden emptying of the organ after distention of its cavity and thinning of its walls by the gradual development within of some physiological or pathological product. Pregnancy or the growth of a polypus furnish these conditions, and almost universally inversion is the sequence of one of these. But may it not have other origin? The agency of hydrometra and hematometra has been admitted by high authority, but no case has been recorded. Hydatids may also give rise to the conditions necessary to the occurrence of inversion.

The question proposed does not refer to any of these causes. Given a healthy condition of the uterus, and a negative answer may be returned at once. But may not the organ undergo such pathological changes as to permit inversion, or may not this occur independent of the conditions generally stated to be essential? This was first affirmed by Puzos in 1774, and from this point the author of the paper followed the discussion without reference to cases, and closed by saying that there seems to be fair argument in favor of the proposition that uterine inversion may occur independently of pregnancy or polypus, and some cases seem to prove it.

2. *Does inversion of the uterus always begin at the fundus?*

The almost universal view is that it begins at the fundus, and from that point extends downwards. But may it not begin in a reverse order? The doctrine that it may has not been generally accepted. Most have denied its possibility. Thomas claims that it has not been proven. Taylor advocates the doctrine. The doctrine finds strong evidence in its favor from analogy in prolapsus of the rectum. Preparatory to it, however, the walls of the organ have undergone a pathological change, and in this condition the possibility of inversion must be admitted. Taylor gives

three cases: one from Lawrence, another from Ingleby, and the third his own.

Dr. Reeve's conclusion was that there seems to be sufficient evidence to establish the proposition that inversion can begin at the cervix.

3. *Does puerperal inversion of the uterus ever occur except at or immediately after delivery?*

The question is an important one, and upon its answer rests the decision, in many cases, as to the responsibility of the practitioner. No practitioner is excusable for not observing the inversion in long-continued cases, and the inquiry is not with regard to cases in which simple discovery of the inversion has been delayed. The author of the paper then gave a *resumé* of several reported cases, and concluded with the statement that the testimony rendered argument in support of the proposition unnecessary.

4. *May inversion of the uterus take place without sufficient symptoms to attract attention or indicate that anything has gone wrong?*

As a general rule, the symptoms are pronounced. But there are cases in which the general rule does not hold good. Two cases were cited which sustained the proposition.

Dr. Reeve then spoke of the difficulty of reaching a correct diagnosis in cases of inversion, and said that no reliance could be placed on the history of the case, and that the only safe clinical rule is to hold that the vaginal tumor may be an inverted uterus, whatever the history or condition or circumstances of the patient.

DR. JOHN SCOTT, of San Francisco, had had two cases of inversion of the uterus. In one he effected reposition readily; the other was one in which the patient had complete inversion of the uterus for eight months without hemorrhage, and the hemorrhage developed at the time she weaned her child. Dr. Scott saw the case in consultation, and succeeded in effecting reduction in about twenty minutes, but the interesting feature was that he felt distinctly the uterine fibres tear, and when the organ was replaced it remained open completely and admitted four fingers up to the fundus. This condition remained three weeks, and with recurrence of the next menstrual period severe hemorrhage came on and lasted ten days. After it had ceased he found the uterus patulous, and it remained so until the next menstrual period, when hemorrhage occurred again. Dr. Scott then introduced two silver wire sutures and drew the womb together. A violent attack of cellulitis followed, but the woman recovered.

DR. W. H. BYFORD, of Chicago, had seen only nine or ten cases of chronic inversion, and two or three acute cases. One case was especially interesting. In 1870, a young Jewish woman was confined the second time, and her labor was not remarkable in any respect. After labor he examined through the abdominal walls and found the uterus in position. There was no symptom to indicate that any accident had occurred to the patient. Four

or five weeks afterwards he went to California, and before his return the patient suffered from hemorrhage, which led her to call a physician, and he found a completely inverted uterus. The patient after her labor nursed her child and weaned it shortly before the hemorrhage took place; whether or not the weaning had anything to do with the development of the hemorrhage, he was unable to say.

DR. B. B. BROWNE, of Baltimore, cited a case seen in consultation, in which, without any accident during labor, hemorrhage appeared six months afterwards, sufficient to produce syncope, and examination revealed inversion of the uterus. In this case there was laceration of the cervix and the condition Dr. Reeve had spoken of where possibly the inversion begins below instead of above.

Discussion was continued by Drs. A. Dunlap, of Springfield, O.; E. W. Sawyer, of Chicago; H. P. C. Wilson and W. T. Howard, of Baltimore.

DR. H. P. C. WILSON, of Baltimore, then read a paper entitled

FOREIGN BODIES IN THE ABDOMEN AFTER LAPAROTOMY

Frequently discovered during life, occasionally after death, and very often never discovered. He was convinced that the accident occurred much more frequently than is generally supposed. Unfortunately, many of the fatal cases are not published, and besides we are unable to obtain autopsies in many obscure cases which would reveal whether or not a piece of sponge or a pair of forceps had been left and gave rise to the symptoms. He had succeeded in collecting only twenty-one cases, and of these only five had been published. Of the 21 cases, 6 occurred in this country and 15 in Europe. Of the European cases, 5 had been published. In the 6 American cases, sponge was left in the abdomen in 5 and a pair of forceps in the sixth; in four recovery took place by the timely discovery of the presence of the foreign body.

In Dr. Wilson's case, the presence of the sponge was not suspected until it worked its way to the surface five months after the operation, and the patient made a good recovery. He then gave a detailed history of the case, which was one in which he performed ovariectomy in a pregnant woman. Abortion occurred eighteen days after the laparotomy, then came the phlegmon in the abdominal wall, and discharge of the sponge in pieces, which were removed by Dr. Geo. H. Hocking, her attending physician, during a period extending over several weeks. The author of the paper then spoke of the precautions to be taken for avoiding the occurrence of the accident.

DR. T. GAILLARD THOMAS, of New York, thought that more papers of the kind of Dr. Wilson's should be brought before the Society than had been; papers characterized by the frankness of the author and a desire to avoid danger to patients by accidents. In between four and five hundred laparotomies, Dr. Thomas had had the accident of leaving a foreign body in the abdomen occur only

once, and then it was a piece of sponge. The sponge (for only one was used) was used by his most accomplished assistant, Dr. James B. Hunter. The operation was an exploratory incision in a case of enlarged spleen, found to be due to cancer. Ether was used, complete cessation of secretion from the kidneys occurred, and the patient died. At the autopsy, a small piece of sponge was found in the abdominal cavity. It was a very small piece and had doubtless broken off from the sponge which was used. To avoid such accidents, Dr. Thomas never has more than one assistant, one attendant who takes charge of the instruments, and a third who gives the anesthetic. To guard against the leaving of sponges, he has attached to each one a piece of tape about six inches long, as it is hardly possible that both sponge and tape will disappear entirely.

To guard against the leaving of instruments, only one way is certain, and that is to never leave an instrument within the abdominal cavity for any length of time.

DR. A. REEVES JACKSON, of Chicago, knew of three unpublished cases in Chicago in which foreign bodies had been left in the abdominal cavity after laparotomy; sponge in two, forceps in one, and in each case the body was discovered at autopsy. Although he had never had the accident occur in his practice, he made it an inflexible rule to have a written list of all instruments and sponges, etc., and each assistant was instructed not to cut or tear a sponge under any circumstances whatever. At the close of the operation, and before the abdominal wound is united, compare the count with the written list, and he never trusted to either nurse or assistant to make the comparison.

DR. JOHN C. REEVE, of Dayton, O., remarked that the paper proved one thing, and that was that Dr. Wilson used aseptic sponges.

DR. A. DUNLAP, of Springfield, O., had heard of such accidents, but had never been able to understand why they should occur. He had never left any foreign body in any of his cases, unless the patient had recovered without finding it out. He uses but very few instruments and never allows any one to pass them to him; he always takes time to pick them up himself, and always counts them before closing the abdomen. He was now able to see an additional reason for making a large incision through the abdominal wall. He always made one sufficiently long to admit the whole hand which would permit of thorough exploration of the abdominal cavity. He uses large sponges and believes it prudent to tie vessels very soon after they have been seized with the Péan forceps. If small sponges are used on the end of sticks punching and poking about, the operator cannot be certain whether a sponge is left or not. Make the incision free, and he had yet to see a case in which union was not complete within three days after the operation.

DR. W. T. HOWARD, of Baltimore, referred to a case occurring in the Samaritan Hospital, London, where a sponge was left in the abdominal cavity, and

DR. G. J. ENGELMANN, of St. Louis, added a case in which the accident occurred through the officiousness of a distinguished gentleman who had been invited to attend the operation, but not in the capacity of an assistant. At the autopsy, the sponge was perfectly pure and clean, and there was no evidence of inflammation. Each assistant should have only one thing to do, and for that be held responsible.

DR. PAUL F. MUNDÉ, of New York, suggested as precautions against the occurrence of the accident that sponges in the least friable or brittle should be avoided, that sponges should never be torn, but cut, that the number of sponge-holders should always correspond to the number of sponges, and the one should be firmly attached to the other and never be removed during an operation, the nurse being specially instructed to see that each sponge is securely attached, before handing back the holder after washing the sponge.

The discussion was continued by DR. BAKER, of Boston, who alluded to the selection of substances, such as material for sutures, etc., in abdominal surgery, and was closed by DR. WILSON, who could add to his first list seven cases, six having been reported during the discussion, one collected while on his way to the meeting, making a total of twenty-eight cases in which the accident had occurred.

First Day—Afternoon Session.

DR. C. D. PALMER, of Cincinnati, O., read a paper, entitled
ABDOMINAL SECTION—ITS VALUE AND RANGE OF APPLICATION
AS A MEANS OF DIAGNOSIS AND TREATMENT.

There can be no satisfactory therapy without a correct appreciation of the conditions upon which it is based. The art of diagnosis is most important and most difficult, and in no class of cases is the difficulty greater than in certain pelvic and abdominal affections of women. The author of the paper then reviewed the main points in the differential diagnosis of ascites and ovarian cyst, complicated perhaps by cancer of the peritoneum or omentum; also encysted dropsy of the peritoneum, movable kidneys, echinococcus of the kidneys, etc.; ovarian cyst and uterine fibrocyst; extrauterine pregnancy of long duration, etc., etc. In cases belonging to these classes it is almost impossible, even with the greatest care, to be absolutely sure of the diagnosis. It should not be inferred, however, that errors in diagnosis are more frequent in these affections than in diagnosticating diseases occurring in other parts of the body; for diagnosis here has been pushed to a degree of perfection not obtained with regard to any other forms of disease.

Tapping as a diagnostic means had received a great deal of attention; but has not its value been greatly overestimated? It seems that tapping is attended with no inconsiderable danger in all multilocular ovarian cysts. Something, of course, depends upon the method of performing the operation, the precautions ob-

served, etc. But what advantages does it possess? To place the life of a patient in danger for purposes of diagnosis simply, is a fearful responsibility to assume, and therefore tapping should be omitted if diagnosis can be established without it or some other risky procedure. He did not regard it as criminal to resort to it, nor did he accept the extreme view of Tait that it should be entirely discarded, because sometimes information derived thereby is very great, and such as cannot be obtained in any other way except by abdominal section. Dr. Palmer's conclusions concerning tapping were, that it is unnecessary and superfluous for purposes of diagnosis; that it is attended with such risks in its immediate and remote effects as to limit its employment to the minimum—namely—only when absolutely necessary; that it promises but little; that it is capable of doing much harm, that the condition of the fluids is not characteristic; that neither chemically nor microscopically are they positively trustworthy; that the evidence obtained by tapping is simply presumptive, probably strongly corroborative in connection with certain symptoms and signs, and not positively unailing.

With regard to the exploratory incision, accurate and thorough diagnosis can be secured by the smallest opening, and dangers can be recognized before the entire field is surveyed. The author of the paper then gave the statistics of the exploratory incision as found in cases reported by Atlee, C. C. Lee, five cases of his own, and others. All experience shows that the risk of pure abdominal section, except in malignant disease, is less than might be expected. As a general proposition, abdominal section is not dangerous in most cases in which it appears to be justifiable. In many cases of multilocular ovarian cyst it is less dangerous than tapping. As soon as section is followed by very much interference with a tumor or pelvic or abdominal viscera, or if carried to the point of incomplete operation, the dangers rapidly rise and the mortality increases. The fatal issues are very largely restricted to those cases in which the disease would, within a short time, prove fatal without incision. The following were among the conditions mentioned in which exploratory incisions are allowable: (1) some ovarian tumors; tapping is largely unnecessary, and ovariectomy should follow as soon as threatening symptoms manifest themselves; in complicated cases tapping is of doubtful propriety; (2) certain interstitial and extrauterine fibroids producing persistent hemorrhage, growing to large dimensions, threatening life, etc.; abdominal section enables the surgeon to clear up points of diagnosis and to determine whether oöphorectomy or hysterectomy promises the best results; (3) certain cases of acute and chronic peritonitis; (4) intestinal obstruction; (5) some cases of pelvic abscess; (6) extrauterine pregnancy, of which abdominal section has sometimes changed entirely the outlook of laparotomy. After the fourth month, when neither electricity nor puncture can be employed, laparotomy can be practised at least in the tubal and interstitial forms, and as a primary operation in the abdominal form.

Of course, exploratory section is liable to abuse. Its indiscriminate use would be disastrous. Yet abdominal section, with perfect propriety and increased satisfaction, and sometimes diminished risk, may be made to supersede, more often than it has, some other methods of exploration, especially tapping, and it may be utilized in the salvation of a few cases which have heretofore been regarded as hopeless.

DR. G. J. ENGELMANN, of St. Louis, thought that abdominal section had been practised too little in this country *as a means of diagnosis*. Where the exploratory incision is made, a conclusion can be reached with a fair degree of certainty of being correct, and he does not believe that it is so dangerous as tapping, while certainly it is more satisfactory. He would hardly think it was more dangerous to practise it in cases of malignant disease than any other, as he had performed it in three cases, and in all the abdominal wound closed by first intention.

DR. A. DUNLAP, of Springfield, O., had never practised abdominal section as a means of diagnosis.

DR. P. F. MUNDÉ, of New York, thought that sufficient stress had not been heretofore laid upon exploratory incision in cases of doubtful abdominal disease. He regarded the subject of Dr. Palmer's paper a very important one, and thought all would do well to remember, as had been stated by the author of the paper, that the exploratory incision, in the majority of cases, is a rather safe procedure, and that it may reveal, not only conditions worse, but conditions better than were expected to be found. With regard to incision in cases of pelvic abscess, he should first aspirate, and if pus was found, then make a long incision, which he could hardly call an exploratory incision. He had cut arteries in several such cases, but no serious inconvenience followed, and the patients made good recoveries.

DR. H. P. C. WILSON, of Baltimore, said it had always seemed to him that every ovariectomy was an exploratory incision; because he believed it to be very rare for any man to know, before cutting through the abdominal wall, exactly what he would find. He was one of those who believed that exploratory incisions are no more dangerous than tapping, and he had made the exploratory incision in cases in which it would have been impossible to have made a diagnosis in any other way.

DR. R. B. MAURY, of Memphis, Tenn., had made abdominal section in two cases, and found conditions which precluded further operative interference. Both patients recovered from the operation.

DR. A. REEVES JACKSON, of Chicago, asked for the experience of the Fellows in the treatment of pelvic abscess by means of abdominal section.

DR. DUNLAP, of Ohio, related one case, and Dr. Scott, of California, related two cases of this kind, after which the discussion was closed by Dr. PALMER, who remarked that he based his statement concerning exploratory incision in malignant disease

upon his experience in five cases, four of which terminated fatally in from three to thirty days after the operation.

DR. PAUL F. MUNDÉ, of New York, then read a paper on

INTERSTITIAL FIBROIDS OF THE CERVIX AS A CAUSE OF DYSTOCIA, AND THEIR REMOVAL BY VAGINAL ENUCLEATION (WITH SPECIMEN).

Difficult labor from cervical fibroids occurs much less frequently than does difficult labor from fibroids situated in the body of the uterus for the simple reason that the former are much less common, and, if large in the non-pregnant condition, interfere with conception. The cervical growths are usually situated in either the anterior or posterior wall of the cervix, and may, towards the end of pregnancy, interfere with the process of parturition; they may render expulsion of the fetus impossible, even after embryotomy, craniotomy, or version in extreme cases. It was entirely of sessile tumors that Dr. Mundé spoke. If the tumor is so large that it completely fills the pelvic cavity, is so firmly connected with the uterine tissue above, or extends so far up into the body of the uterus, that it cannot be pushed out of the uterine cavity, and if the possible space left between the tumor and pelvic wall is insufficient to permit the extraction of even a mutilated child, only two plans remain for the operator to pursue: either to attempt to remove the mass piece-meal or entire through the vagina, or perform Cesarean section with or without removing the tumor. The results of the latter operation have been so unfavorable as to make the other, if it seems at all possible, by far to be preferred.

The applicability of enucleation of cervical fibroids, aided by means of Thomas' scoop, even of large size, which obstruct delivery, has apparently received but little attention, only a small number of cases being on record. Of course, not every case of cervical fibroid would require or be suitable for enucleation. A small and displaceable tumor could be ignored or pushed out of the way; very large growths extending up into the uterine body above the umbilicus may not be removable by the vagina at all, or may be so only in part; or it may be possible to drag a premature or mutilated child through even a very small opening if part of the fibroid prolapses or it can be reached so that traction can be made.

But that the proper method to terminate delivery in a case where the pelvis is so much obstructed by a cervical fibroid that not even a macerated or premature child can be extracted, is vaginal enucleation, seemed to him to be well demonstrated by a case which came under his observation during last Spring.

The patient was 38 years of age, the mother of seven children. The tumor was a dense mass which filled the pelvic cavity almost to the vaginal orifice. After examination, the diagnosis was made of interstitial fibroid of the anterior wall of the uterus and of the cervix, with pregnancy about six months advanced, and a living fetus.

Two plans of treatment presented themselves. First, to control hemorrhage as much as possible, allow the case to go on to term or until labor began, and then attempt to enucleate the tumor in part or entirely through the vagina, and then remove the child; or if this was not practicable to perform Cesarean section; or perform Porro's operation; or do intraperitoneal enucleation; Second, to wait a week or two for the patient to regain sufficient blood and strength to enable her to stand the operation and then enucleate the tumor through the vagina, without, if possible, interfering with the gestation at all, and thus give the fetus at least a chance to grow to a viable age.

The second course was regarded as decidedly the most preferable, and was adopted. On the morning of the day fixed for the operation, labor pains set in, the membranes burst, and the cord prolapsed. The necessity for immediate removal of the tumor (already decided upon) was thus forced upon Dr. Mundé, and he succeeded in enucleating by the hands and steady traction, aided in one spot only by Thomas' serrated spoon, the tumor which he presented, weighing three pounds, and measuring 8" in length and $20\frac{1}{2}$ " in circumference. The fetus and placenta were then easily extracted. The tumor-cavity was packed with iodoform cotton. No hemorrhage followed, and the patient made an uninterrupted recovery.

The only similar case on record is a recent one by Schroeder, in which S. allowed the patient to go to term, and then before labor began enucleated a tumor weighing about a pound; labor came on next day, and a living child was born. The mother recovered.

Dr. Mundé then gave a *resumé* of the literature of the subject, which included only nine cases with his own, in which the mothers all recovered, and of the children all that were viable were saved.

DR. E. W. JENKS, of Detroit, had had one case of cervical fibroid complicating labor. It was a small tumor, which he removed partially by incision and partially by enucleation, and the labor proceeded. As to the method of effecting delivery in these cases, he would by all means favor enucleation rather than Cesarean section, although he believed in the latter operation when performed before the woman becomes exhausted.

DR. H. P. C. WILSON, of Baltimore, favored enucleation before labor begins.

DR. W. H. BYFORD, of Chicago, said his practice had been, whatever the size or character of the tumor in the cervix, to wait until labor comes on, believing that ablation of the tumor at that time would be as easy as at any other, if not easier, and waiting until this time offered certain advantages. First, in a good many of these cases, large, even interstitial tumors, will be pushed out before the advancing head of the child, and the head will be delivered without injury. If, on the other hand, enucleation be practised, danger of producing a miscarriage is incurred which

will carry with it danger to the mother, and a sacrifice of the child.

DR. DUNLAP, of Springfield, preferred the method by enucleation to Cesarean section.

DR. VAN DE WARKER, of Syracuse, cited a case in which he advised enucleation, but the operation was rejected, Cesarean section was performed, and the woman died. Autopsy proved that enucleation could have been readily performed, and probably the case might have terminated favorably for both the mother and the child.

DR. T. A. REAMY, of Cincinnati, had had one case in which he incised the capsule and enucleated the tumor with his fingers. He should say that in the majority of cases it would be better to defer the operation until labor has begun.

The discussion was closed by Dr. Mundé, who advised that vaginal enucleation be attempted in all cases of labor obstructed by fibroids in the lower segment of the uterus before exhausting the mother or mutilating the child by ineffectual efforts at delivery.

Second Day—Morning Session.

DR. T. GAILLARD THOMAS, of New York, made

A FURTHER REPORT UPON EXTRAUTERINE PREGNANCY,
EMBODYING SIX CASES.

It was supplementary to a report which he made two years ago at the meeting of the Society in Boston. Curious as it may appear, it is within the last ten or fifteen years that the most rapid strides concerning the successful treatment of extrauterine pregnancy have been made, and rules for diagnosis early, not approximate, but to a great degree certain, have been established. Such diagnosis, however, is difficult at a period sufficiently early to prevent rupture of the distended Fallopian tube, and even after the fetal movements and the fetal heart are at our disposal, accurate diagnosis is far from easy.

According to Bandl's statistics, which seem to be well accredited, out of sixty thousand pregnancies, only five were extrauterine. It is evident from this that the practitioner who has within eighteen years met with twenty-seven cases must have seen a large number of them in consultation. In the course of his experience in extrauterine pregnancy, Dr. Thomas had seen every condition, from a phantom tumor to a fecal impaction, confounded with extrauterine pregnancy. After remarking upon the necessity of making a careful study and a complete mastery of the diagnosis in these cases, the author of the paper spoke of the conscientious determination which every practitioner should have in recognizing his duty, and after having recognized it, in performing it unflinchingly. He was not prepared to agree with Depaud that it is ordinarily impossible to diagnosticate extrauterine pregnancy before the third or the fourth month. He then referred to the experiments of Leopold, of Leipzig, which were made upon rabbits,

and consisted in the introduction of fetal rabbits into the abdomen of an animal upon which laparotomy had been performed, and studying the changes which took place subsequently. These observations had a direct bearing upon the destruction of the fetus by means of the galvanic current. In the course of the experiments Leopold had been led to believe that recovery of patients after rupture of the Fallopian tube was much more common than was generally supposed—a view which was corroborated by the experience of the author of the paper.

Twenty years ago, Dr. Stephen Rogers, of New York City, recommended laparotomy with the ligation of bleeding vessels for the control of hemorrhage incident to the rupture of the sac in extrauterine pregnancy, but he had no experience and based his plan of treatment upon theory alone. In January, 1883, Lawson Tait operated under these circumstances, and during that year and the year 1884 his experience was increased by four additional cases: of his five cases four recovered and one died. Dr. Charles K. Briddon followed Tait in October, 1883, in the performance of the same operation, the history of which was embodied in the report of his six cases. After further remarks upon this point Dr. Thomas made the statement that neglect to perform laparotomy under these circumstances was criminal. He then proceeded to the relation of special cases, six in number.

The first he was requested to see by Dr. Ferdinand Beach. The patient was suddenly taken with pain, went home, and remained in bed, and after six weeks Dr. Thomas saw her in consultation. Her sudden attack of pain occurred immediately after being thrown violently upon her feet from a street car. He was not positive concerning the diagnosis, but believed there had been pelvic hematocele, and that the hematocele might have been due to extrauterine pregnancy. The case terminated fatally, and at the post-mortem examination a fetus of three months was found. The case was one of abdominal pregnancy.

The second case was one which he saw in consultation with Dr. Elizabeth Cushier, of New York. Dr. Thomas then gave an extensive and detailed history of the case as furnished him by Dr. Cushier, in which it appeared that the patient had become pregnant after a long period of sterility. She first developed symptoms of pelvic hematocele, and Dr. Cushier recognized rupture of the extrauterine cyst. The usual measures were adopted, and the patient rallied. Dr. Thomas saw the case in consultation, and corroborated the diagnosis. The uterine cavity was explored, and found to be empty. The faradic current was employed, one electrode being inserted within the rectum and the other applied to the abdominal surface. A few hours after the application of the electrical current, the patient's condition was greatly improved, the treatment was continued, the mass subsided, and the patient entirely recovered.

The third was the case seen with Dr. Charles K. Briddon, of New York. Symptoms of pelvic hematocele developed suddenly in the patient, and a diagnosis of extrauterine pregnancy was

made. Dr. Briddon performed laparotomy, the patient rallied, but died of shock forty-seven hours after the operation.

The fourth case was one which he saw with Dr. Cocks, of Harlem. The electrical current was used and the patient began to improve immediately. Complete recovery followed.

The fifth was a case which he saw with Dr. Henry Griswold and Dr. Jewett in Feb., 1884. Labor came on at the end of the ninth month, but there was no dilatation of the os. Dr. Griswold dilated the uterine canal with the patient under an anesthetic, explored the uterus, and found that the organ was entirely empty. The woman's general condition being good, Dr. Thomas advised against surgical interference until something occurred in the peritoneal cavity demanding it.

During March and April the patient did well, but in May she lost her appetite, became emaciated, etc., and by June she had depreciated so much that Dr. Thomas got her to enter his service at the Woman's Hospital, where on the fifth of that month he operated, assisted by Dr. C. S. Ward, Dr. H. D. Nicoll, and the house staff of the hospital. A large male child was extracted, which had begun to undergo decomposition. The placenta was the largest he had ever seen; only a portion of it was removed, and this weighed four pounds. The entire contents of the fetal nidus, liquor amnii and all, were estimated at sixteen pounds. The patient suffered violently from acute septicemia, but in six weeks was discharged entirely cured.

The huge placenta was attached to the ascending, transverse, and descending colon. To tear it off would have been to insure death by hemorrhage; to leave it, absorption and septicemia. Dr. Thomas, therefore, adopted a compromise measure, and passing the cobbler's stitch about an inch and a half from the intestine, he carried the seam all the way around in the shape of a horse-shoe, and then cut away the placenta as near to the seam as he dared, puckered the remaining placental attachment like the mouth of a bag, and sewed it with care into the abdominal wound. As Dr. Pallen expressed it, he "made a marsupial animal of his patient." The abdominal opening was closed, and the pouch was constantly irrigated with carbolized water.

The sixth was a case under the care of Dr. Lambert, of Greenwich, New York, and occurred in a patient whom Dr. Thomas had previously treated for sterility. On examination, Dr. Thomas found the uterus larger than normal, but evidently empty, and also found what he believed to be a small fetus behind the uterus in the abdominal cavity. Dr. S. Beach Jones also saw the patient, coincided with the diagnosis, and Dr. Fordyce Barker was also added to the consultation subsequently. There was a diversity of opinion concerning the exact nature of the case. It was decided that electricity be employed, and soon after its employment there was marked improvement in the symptoms, and the patient ultimately made a good recovery.

DR. P. F. MUNDÉ, of New York, said that Dr. Thomas's remarks on diagnosis and treatment with the electrical current were

especially interesting to him on account of his recent experience in the use of that agent.

Instances of extrauterine pregnancy observed at so early a period as to admit not only of a certain diagnosis, but of successful treatment, were not very common. Dr. Mundé had lately had a case under observation, that of a woman thirty-eight years of age, the mother of one child eight years old, and had had one miscarriage six years previously. She consulted him with reference to the question whether or not she was pregnant. She menstruated last in November, 1883. Her next menstrual period passed without a show, but early in January there was some bloody discharge, although much less than usual. Her appetite was good, but she invariably vomited after eating. She believed that she was pregnant. She complained also of morning sickness, which was not, however, especially marked. A vaginal examination showed a uterus pushing somewhat to the left side, but little if any enlarged, and in the right side of the pelvic cavity an oblong, irregular, deeply fluctuating mass of the size of a goose's egg, which could be slightly moved about, causing the uterus to move with it. Feeling confident that the uterus was empty, Dr. Mundé introduced the sound, and it entered toward the left to the depth of three inches. He diagnosticated tubal pregnancy, and this was confirmed by the appearance of the areola and the oozing of colostrum from the nipples upon pressure; although the breasts were not enlarged, yet they felt rather fuller than usual. Dr. Thomas Addis Emmet saw the patient in consultation, and corroborated the diagnosis made by Dr. Mundé. He also advised the use of the galvanic current. The patient had had some shock, pain in the right side, etc., and after the examination at Dr. Emmet's office she had considerable shock, sufficient to confine her to her bed, and to show moderate signs of collapse. Dr. Mundé feared that rupture of the sac would occur, and in spite of the shock resorted to the electrical current, passing one electrode into the rectum and the other sponge over the abdomen. He gradually increased the current from ten to twenty-four cells, rapidly breaking the current a dozen times or more. The sitting lasted about ten minutes, and the shock was quite painful, and yet not sufficient to make him believe that harm had been done. On the following morning he found the patient in a condition of collapse, which had been preceded by violent pain, and he thought that the sac had ruptured, yet the blanched appearance and the shock did not appear to him to be that from internal hemorrhage. He gave hypodermics of brandy and ammonia at intervals of fifteen minutes during the day, arranged for the performance of laparotomy, although he determined to wait until the patient rallied. Accidentally meeting Dr. Bridgdon, he mentioned the case to him, and received the advice to operate at once. Dr. Mundé, however, did not believe it a wise thing to do, because he was not sure that there was rupture of the sac in his case. The following morning the patient's pulse was just perceptible, but it gradually increased in strength, and

ultimately she made a perfect recovery. To ensure the death of the fetus, he began the faradic current during the second week after the galvanic shock mentioned, using it in the same manner for five days in succession, each time about fifteen minutes. Three months afterward the woman was perfectly well, and the tumor had been reduced about one-third in size. Dr. Mundé, in view of his case, thought it wise to wait a little in cases where sudden collapse seemed to indicate rupture, before resorting to laparotomy. He also thought he should begin with the faradic current, because of the severe shock which he saw in his case after the use of the galvanic current, and he should not use the galvanic current of that strength again except as a sole alternative for laparotomy. He thought where the diagnosis could be made at an early period, to employ any other treatment than the electrical current was risking the patient's life.

DR. C. D. PALMER, of Cincinnati, asked Dr. Thomas at what time in utero-gestation it would be improper to use the galvanic current.

DR. THOMAS said if pregnancy had passed four and one-half or five months, although an absolute line could not be drawn, at least as soon as it could be demonstrated that the fetus is in the abdominal cavity, and not in one of the tubes, the true scientific inference would be to allow the fetus to grow and be delivered at the ninth month alive, and give the woman the chance of laparotomy. If the fetus is not in the abdominal cavity, he should not feel inclined to destroy its life, but allow it to develop and give the woman a chance of laparotomy. Up to that time, he should be willing to allow the fetus to remain in the abdomen, allow it to become encapsulated, as described by Dr. Leopold as occurring in rabbits. When formerly he fixed the line at the end of the fourth or fifth month, he believed that as small a fetus as that would be absorbed, but he should not like to run the risk of asking Nature to take care of a larger one.

The discussion was continued by Dr. H. P. C. Wilson, of Baltimore; Dr. T. A. Reamy, of Cincinnati; W. T. Howard, of Baltimore; and W. H. Baker, of Boston.

DR. R. B. MAURY, of Memphis, then read a paper entitled

A CASE OF TUBAL PREGNANCY WITH RUPTURE OF THE SAC,
AND SOME REMARKS WITH REGARD TO THE PROPRIETY OF
LAPAROTOMY.

The patient was one in whom the symptoms of intraperitoneal hematocele developed suddenly during pregnancy. Efforts were directed to relieve the patient of shock. Reaction occurred, and the patient was comparatively comfortable. On examination, the uterus was not found appreciably enlarged. A tumor was discovered in the right side of the abdomen which could be felt through the abdominal wall. Diagnosis of tubal pregnancy with rupture of the fetal sac was apparently certain. All the usual symptoms of peritonitis developed, and then subsided. Slight pneumonia also developed at the base of one lung, but from this

the patient recovered, and six months after the rupture of the sac there could be felt only a small tumor in the right half of Douglas' pouch.

Dr. Maury made his case the occasion for some comments upon the proposed treatment, by laparotomy, of tubal pregnancy at the time of rupture of the sac. He quoted the views of Playfair, Lusk, Thomas, and others on this point. Commenting upon Tait's cases, he remarked that when studied closely it would be seen that in only one did he operate at the time of rupture of the sac, and in the others several days afterward, and that the patient operated upon at the time of rupture of the sac was the one who died.

In view of the fact that the patient's condition some hours after rupture of the sac is especially one of shock rather than of collapse from hemorrhage, Dr. Maury submitted that even under perfect surgical methods we may never hope, as a rule, to obtain satisfactory results from immediate laparotomy, and at this time it would seem to be more in accordance with sound surgical practice to postpone all consideration of laparotomy until the period of shock has passed away. Our efforts at the time of the rupture should be directed solely to rallying the patient and bringing about reaction. If these efforts be successful, then the question of abdominal section will arise for consideration, and our course in the matter will perhaps be decided: (1) by the age of the fetus; (2) by the amount of extravasated material; and (3) by the character of the resulting inflammation.

Second Day—Afternoon Session.

The Society was called to order at three o'clock by DR. A. REEVES JACKSON, of Chicago.

THE LIMITS OF VAGINAL HYSTERECTOMY FOR CANCER.

DR. PAUL F. MUNDÉ, of New York, read a paper on the above subject, which was substantially a rejoinder to the paper read by Dr. A. Reeves Jackson at the annual meeting of the Society in 1883, and in which he entered his protest against the wholesale condemnation of the operation shown by the Society at its last meeting. Dr. Mundé had never been tempted to perform Freund's operation, and could not but believe that the condemnation which extirpation of the cancerous uterus had received at the hands of the Society was intended chiefly for Freund's operation. So far as the vaginal operation was concerned, he thought that it was due to the many experienced surgeons and conscientious operators who hold different opinions, and to the interests of progressive surgery to enter a respectful protest against the opposition shown to the operation of vaginal hysterectomy for cancer of the uterus.

Dr. Mundé then related the history of two cases of his own, and exhibited the specimens, after which he quoted the three conclusions which Dr. Jackson arrived at last year:

"First. Diagnosis of uterine cancer cannot be made sufficiently early to insure its complete removal by extirpation of the uterus.

"Second. When the diagnosis can be established, there is no reasonable hope for a radical cure, and other methods of treatment far less dangerous than excision of the entire organ are equally effectual in ameliorating suffering, retarding the progress of the disease, and prolonging life.

"Third. Extirpation of the cancerous uterus is a dangerous operation, which only lessens suffering in those whom it kills, and gives no reasonable promise of permanent cure in those who recover. Hence it fails in all the essentials of a beneficial operative procedure, and should not be adopted in modern surgery."

Dr. Mundé directed attention especially to these three propositions, and with reference to the first, claimed that a diagnosis of cancer can be made sufficiently early to warrant operative interference, as was clearly proven by a microscopical examination made by Dr. Heitzmann in his first case. Here the microscope permitted the prediction that the disease would return within two years, and it actually returned in nine months. The microscopical examinations of Ruge and Veit as to the slight changes which take place in erosions, and ulcerations, and doubtful enlargements of the cervix, will enable the expert to diagnosticate at an early stage the tendency to or the presence of a malignant degeneration. Of course, the number of cases will be small.

With reference to the second proposition, that, diagnosis being established, there is no hope for a radical cure, Dr. Mundé maintained that this assertion could be refuted at once by quoting the results obtained by other methods in the clinic of Carl Braun. It is true, Braun's cases were carefully chosen with the special purpose of securing permanent benefit. The results as to mortality obtained in Billroth's and Rose's clinics with operations for cancer of other organs were quoted for comparison with the immediate fatality of vaginal hysterectomy. In Billroth's clinic, the mortality from mammary cancer was 20 per cent; from lingual cancer, 43 per cent; from rectal cancer, 53 per cent. In Rose's clinic, the mortality from mammary cancer was 26.3 per cent; from rectal cancer, 53 per cent; and from lingual cancer, 11 per cent. Of the total number of 256 cases of vaginal hysterectomy for cancer of the uterus which he had been able to collect, the percentage of mortality was 24.6 per cent. Dr. Mundé thought that, with these figures, Dr. Jackson's third statement was disproved, and all he could grant him was a qualified assent to his first assertion that, *for the present* at least, it is exceedingly difficult to diagnosticate uterine cancer sufficiently early to secure its complete removal by any method of operation as long as women are not taught to seek advice when suspicious symptoms *first show themselves*.

With reference to the operation, there are two main questions: (1) Can vaginal hysterectomy for uterine cancer be performed without so great a loss of life as to render it unjustifiable; and (2) Can we hope to secure by it complete immunity from a return

of the disease, or at least so much relief from suffering for a few years as to compensate the patient for the risks she incurs of losing her life by the operation.

To the first question, Dr. Mundé gave an unconditional affirmative answer. The second he could not answer in the affirmative with equal positiveness.

Dr. Mundé then enumerated the most improved methods of performing the various steps of the operation. He then defined as clearly as possible the precise conditions in which alone the operation seems justifiable: (1) Limitation of the cancerous degeneration to the uterus and absolute freedom from disease of the parametrium; (2) Cancer of the cervix extending up the cervical canal to a height the precise limit of which is doubtful, and thereby rendering the possibility of complete removal of the disease by high supravaginal amputation and cautery extremely questionable; (3) Cancer or sarcoma of the body of the uterus; (4) Perfect mobility of the uterus. This is absolutely indispensable; (5) A capacious vagina permitting the ready exposure of the cervix and vaginal vault throughout; (6) A sufficiently vigorous condition of the general system to permit the patient to stand the shock of the operation.

When the above conditions are carefully considered, it will be apparent that the number of cases fit for complete vaginal hysterectomy is comparatively limited, and that if these rules are complied with, there is little danger that the number of victims from this "unjustifiable" if "brilliant" spectacular operation will be large.

All he desired in the paper was to protest against the wholesale condemnation of a perfectly rational surgical measure, on the more or less theoretical ground that it is too dangerous and not sufficiently beneficial, when the facts, as shown by statistics, prove the contrary, and to claim for vaginal hysterectomy for cancer a certain justification and application, however limited that application may for the present be.

DR. A. REEVES JACKSON, of Chicago, said the paper which he had the fortune, good or, as now seems, bad perhaps, to have written for the Society, had been attacked with such a strong array of artillery that he scarcely felt prepared to make a defence at all points. There are, however, principles which underlie all therapeutic measures, surgical as well as medical, and the way in which a surgical procedure is to be properly judged, is to submit it to the same principles which govern not only that particular operation, but all other. Operations for cancer, whether of the uterus or any other organ, cannot escape this rule.

Dr. Jackson claimed that a surgical procedure, to be justifiable, should conform to the ends and aims of all medical and surgical measures, and should alleviate suffering and prolong life. When any operative procedure does neither of these, he claimed that it is unjustifiable, without regard to special success. Statistics had been adduced to support a certain proposition. He was quite

willing to submit any procedure to that kind of test if he wished to either sustain or overthrow it.

Does extirpation of the uterus for cancer by any method lessen human suffering? Does it save human life? Dr. Jackson maintained that it does neither. All know that, when cancer returns, it brings with it precisely the same symptoms it manifested at first. Does it save life? Here we can be guided by statistics only. In the paper which he read last year, he presented the result of a careful estimate, based upon all the cases which he could collect, and it was an unmistakable showing that two hundred years of human life would have been saved had the operation never been performed.

With regard to the dangerous character of the operation, in two of the three cases in the city of Chicago, death occurred within two hours after the operation, and in the third the patient lived about three years. There was no reason to believe that these were altogether exceptional cases.

With regard to the microscope, of course it can aid us in diagnosis in some cases, and it may trace the disease to the submucous tissue, but can it tell us just how far it has extended? The microscope may show when cancer is absolutely present, but it cannot tell how far it has extended, because the disease does not always manifest itself in continuous tissues, and foci for its reproduction may be formed at a remote point, beyond the reach of the microscope.

Dr. Jackson quite agreed with Dr. Mundé in one statement, namely, that "this operation has a very limited area," and he firmly believed that that area should become more and more contracted; certainly it should not be performed when operations which do not involve very greatly the life of the patient have almost, if not the same, ability to remove the disease.

Speaking of successful treatment, what does that mean? When we extirpate the uterus for cancer, we know that patients can live without a uterus, and we remove the organ for the purpose of curing the disease, but it is not a successful operation when a cancerous uterus is removed, because the disease is not removed, and if it shows itself within two months or two years, the disease has simply continued and is not a recurrence.

Dr. Jackson, in closing, maintained that the operation does not come up to the requirements of a beneficial one, that it neither lessens human suffering, nor saves life, and that these facts are sustained even by the improved statistics given by the author of the paper.

DR. VAN DE WARKER, of Syracuse, did not understand that the debate at Philadelphia, last year, was an almost official expression against total uterine extirpation for cancer, although that opinion had gone abroad and returned to us. The lesson to be learned, however, is to be exceedingly cautious in expressing any opinion concerning a new operation proposed in pelvic surgery.

Dr. Van de Warker thought it was not a question of statistics at all; that is, how many die and how many recover after vaginal

hysterectomy. The question is: Is there any better way to reach the same results as have been claimed for this operation? He believed that there are other methods of treating the disease which are safer and equally competent with the knife. His own results in this work had convinced him that the cases should be divided into two classes: 1st, that in which the disease occurs in the menstruating woman; and 2d, that in which it occurs in women who have reached the menopause. There seems to be a potency in the disease, when it occurs in the first class, that insures its return, and which does not show itself to the same degree in the second class of cases.

Although he believed equally good results can be obtained by other methods than the knife, he sanctioned the use of the knife until a fair trial of the operation had been made without prejudice. He was willing to perform the operation, and it was against the prejudice against the use of the knife that he wished to protest, rather than to base opposition to it upon theoretical ground.

DR. G. J. ENGELMANN, of St. Louis, thought Dr. Mundé had not as yet advocated the operation absolutely, and he fully indorsed all that he had said in his paper. Certainly it was unjust to say that this operative procedure is one which does not alleviate suffering, because the patients who had survived had been relieved of all suffering and, so far as he knew, had a fair chance for life; time, however, must settle that point. He regarded the operation, although not well established as is ovariectomy, as in a condition to entitle it to be tested thoroughly.

DR. C. D. PALMER, of Cincinnati, O., said that, use whatever method we might, it cannot be determined whether or not the cancerous disease has extended to the parametrium. Of course, this can be determined in many cases, but the question is: Can we tell that it *is not* involved? He thought it was impossible in at least one-half of the cases. Dr. Palmer then spoke of the analogy between cancer of the uterus and cancer of the breast, with reference to the invasion of the neighboring lymphatics, and of the impossibility of determining whether or not the pelvic glands are involved. He thought the operation was not applicable to the usual form of uterine cancer beginning in the cervix.

DR. W. H. BAKER, of Boston, fully and completely supported Dr. Mundé in the position taken; for, as he stated in his own remarks last year, he believed that the cases in which total extirpation of the uterus was warranted were limited to those where the disease was confined to the body of the uterus; or else to those cases where the disease affected the body and cervix, but did not extend to the cellular tissue around the uterus. At the same time, he regarded the operation as much more serious than some other methods of treatment which had given better results. He quite agreed with Dr. Mundé that the cases in which the radical operation is called for are comparatively few, because not seen sufficiently early.

With regard to the cases (six in number) which he reported last year and previously reported in the AMER. JOUR. OF OB-

STETRICS as still living after the lapse of *five* years, in one, the disease had reappeared within the last year; the other five patients are apparently well. He regarded total extirpation as the wisest operation when the disease has extended beyond the cervix and involved the body of the uterus.

DR. JOHN SCOTT, of San Francisco, thought that the restrictions which Dr. Mundé had placed on the operation should make us very cautious before reaching a decided conclusion as to whether or not it is justifiable. In his own city, the results of the operation had not been as favorable as had been obtained by others.

DR. T. A. REAMY, of Cincinnati, O., thought there was no propriety in calling up the early history of ovariectomy as had been done in every discussion on this subject to which he had listened. For who does not know that, when a woman survives the operation of ovariectomy she is *cured*, and the comparison does not throw any light whatever upon the question. It is not the question whether or not the uterus can be removed and have the patient survive, but it is one which has not been answered by statistics, and cannot be in the present state of the operation; namely—does it mitigate suffering or prolong life? He then referred to a case in which the patient lived four years without having any operation whatever performed. Such cases should be placed by the side of those in which the uterus has been removed, if correct comparison is to be made and correct conclusions reached.

There is no doubt that the mortality of the operation will be reduced as the technique is better understood; but the statistics given by Dr. Mundé do not show that the results of the operation are any better than when it was first revived.

Dr. Reamy's position is that the operation can now be performed more safely than in former times; that as the uterus can be removed more safely the mortality will diminish; but that statistics do not prove, nor does the paper, and the proof is lacking, that the operation prolongs life or cures the disease, or from a clinical standpoint, that it is justifiable. Hence from a purely scientific aspect he regarded it as an unsound surgical procedure, at the same time disclaiming any disposition not to test it by clinical experience.

DR. FENGER, of Chicago, believed that there is a place for the operation, however small it might be.

DR. MUNDÉ, in closing the discussion, said that he was pleased to see that all the speakers substantially agreed with him, except Dr. Jackson. Concerning statistics he could only ask Dr. Jackson to compare the two sets given and see which made the better showing. So far as the aggregate saving of life was concerned, the same estimate which Dr. Jackson had made could be made with reference to any operation which had a recognized position in surgery, and Dr. Mundé did not regard it as a very good way of getting at the truth as to successful results. What is a successful operation? Success may be of two kinds. First an operation is successful that cures permanently; and second, it is successful

if it accomplishes the end desired, and in this instance that end is for the present removal of the diseased organ without killing the patient. The ultimate result is still an open question.

The special point which he wished to make in his paper was to prevent the spread of the impression that the operation should never be performed, and to restrict its performance to a limited number of carefully selected cases. The operation is still in its infancy and should be thoroughly tested before being unconditionally approved or condemned.

Third Day—Morning Session.

The Society was called to order by the President.

ANNUAL ADDRESS OF THE PRESIDENT.

DR. ALBERT H. SMITH then proceeded to the delivery of his address which consisted largely of a paper on "The present Aspect of the Puerperal Diseases." Reference was made first to the work being done and the influence exerted upon the profession by gynecology. The speaker next alluded to the contributions which gynecology has made to medicine and surgery. Several pages were then devoted to the growing and present importance of gynecology, and the greater field of surgery which has opened by it, especially in abdominal cases. Probably the time was not far distant when gynecologists would have entire control of abdominal surgery, not only in women, but among men.

After speaking of the great success and brilliant career of this specialty, the President made the serious inquiry whether we were making the progress we should when our labors were compared with those of our brethren in the old world. Are we gaining ground in original work? Is the standard of our literature what it should be? Does it show earnest profound study, and does our clinical practical work influence the old world, as we are instructed by their work on histology and pathology?

An attempt at making a *resumé* of the progress which has been made in this special department during the last year not being practicable, Dr. Smith took for the text of his address the discussion on puerperal fever, initiated at the New York Academy of Medicine by Dr. Thomas and Dr. Barker. He gave first a brief outline of the views presented by Dr. Thomas and Dr. Barker, or the views of two distinct schools, the absorptionists and the essentialists. Then followed mention of the difficulty in coming to any positive opinion or method of practice in the face of such opposing theories. To do this, we must first decide what puerperal fever is, and Dr. Smith defined it essentially as a febrile condition occurring in women in from two to four days after the termination of pregnancy, either at full term or by abortion, characterized by typhous symptoms, high temperature and rapid pulse, metastatic abscesses, cellulitis and gangrene, formation of thrombi, and the blood in a condition of hyperinosis. He then spoke of the influence of traumatism, and alluded to the condition of the genital tract after parturition, but if no unhealthy influence is brought to bear, every case goes on well not-

withstanding. If we except the extreme view, the conscientious obstetrician might ask the question, whether the poor creature might not be better off in the hands of a midwife with whom Dr. Thomas says the disease is almost unknown.

Dr. Smith then spoke of the old doctrines which have long since been exploded, with reference to the essential nature of the affection. Turning to the two prominent theories of to-day, (1) that of the essentialists, and (2) that of the infectionists or absorptionists, he remarked that he had to regretfully acknowledge that, after a careful study of Dr. Barker's work, he was unable to recognize why, in the many clinical cases on which investigation was based, any one of them should be set aside as belonging to any given class. While Dr. Barker rejects the absorption theory, he gives us no reason why he accepts the view of the essentialists. Allusion was then made to some further modifications of the essential theory set forth by Hicks and Kinkead.

With regard to the absorptionists or infectionists, there were two views: (1) that represented by Semmelweiss, who maintains that the disease is due to absorption of decomposing matter; and (2) that by Pasteur, who says the disease is produced by micro-organisms, or as Paul Bar expresses it, "no microbe, no puerperal fever."

After giving an outline of the course of the disease as it develops in accord with each theory, Dr. Smith spoke of autogenetic causes, such as the breaking down of coagula in the uterus, etc. He devoted considerable space to the consideration of the French doctrine advocated particularly by Paul Bar, who claims that we cannot accept septic poisoning without the presence of microbes. He would not say that the disease was not due to microbes, but would say that so far there has not been proven the slightest causative relation between the two; the micro-organisms almost invariably present and associated with the progress of septic fever exist as an epiphenomenon (Billroth). What shall we do then? Hold on to the good old way; remove everything which can taint the atmosphere, exercise rigid purification of all that pertains to the puerperal woman.

Dr. Smith was unable to understand why there was such opposition to intrauterine injections immediately after labor. It had been his custom to use them as a matter of routine with water at 115° F. and bichloride solution 1 to 1,000, and had seen only the most pleasant result follow.

On motion by DR. REAMY, the thanks of the Society were given to the President for his practical and interesting address.

DR. E. W. SAWYER, of Chicago, then read a paper entitled
SOME REMARKS ON THE OCCIPITO-POSTERIOR POSITION IN VERTEX LABORS; WITH AN ANALYSIS OF THIRTY-FOUR CASES.

First with reference to the frequency of the position, which he had found to be one-fifth of all vertex presentations. He had been unable to find any satisfactory reason set forth for the comparative frequency with which the occiput is directed posteriorly, and ventured the following explanation. In primiparæ the

unyielding abdominal walls conform less readily to the back and scapular region of the fetus than to the abdominal surface, and the back is made to occupy the hollow at the side of the woman's vertebral column. On the other hand, in the multiparous woman, the elasticity of the abdominal tissues compels the occiput to occupy the hollow of the pelvis, and the occiput is made to present to the right instead of the left by the presence of the rectum.

It is almost universally taught that the occiput, when it presents posteriorly, rotates forward, but according to his observation, when the occiput engages in the excavation of the pelvis in the posterior position, it remains in that position and delivery is completed without change of position, if the process is not interfered with. Rotation forward occurs only rarely. How long shall the accoucheur wait for it to take place? Dr. Sawyer then cited cases which illustrated the danger of waiting, and said that in no instance had he had occasion to regret interference because made too early. In other words, he had no expectation that rotation would occur before both mother and child would demand succor. He had been governed by the general rule not to allow the presentation to remain longer than two hours at any one point in the parturient canal, after the waters have drained away, and it is generally conceded that these cases require interference earlier than all others. The position accurately made out, one or two courses can be pursued: (1) drag the head through the canal with forceps; (2) attempt to rotate the head into the anterior position. The importance of changing the position is at once apparent on account of the danger to the perineum. Dr. Sawyer had failed to accomplish rotation of the head with the tips of the fingers, so attractively described in some text-books. But with the hand in the vagina, aided perhaps by external manipulation, the change in position can be accomplished easily.

Change in position effected, one of two courses may be adopted: (1) to allow labor to progress unaided; or (2) to deliver with forceps. Dr. Sawyer preferred the latter and had adopted it in his cases. In occipito-posterior position of the vertex, one of the most important functions of the forceps is to hold the head in a state of flexion, and to this end no special form of long instrument is demanded, but in that combined movement he had experienced great advantage in the use of the forceps with a continued curve which he presented to the profession in 1878. The point which he wished to submit especially for discussion was included in the proposal to introduce the hand into the vagina for purposes of diagnosis, after ordinary methods have failed, and, with the hand in the vagina, to make rotation.

DR. W. L. RICHARDSON, of Boston, had never failed to see the occiput rotate forward when the child was in the normal position with an occipito-posterior presentation. The cases which he had seen where it did not so rotate have been those in which there has been some obstruction, and generally in the middle pelvic strait.

Generally in this class of cases, when seen in consultation, the

attendant has been using forceps and has made a bad matter worse. Dr. Richardson had been pleased with the results of applying the forceps in the reverse manner, with the convexity forward, and when the extension is remedied, the labor progresses favorably without further interference.

The paper was discussed by Drs. T. A. Reamy, of Cincinnati; Joseph Taber Johnson, of Washington; Wm. T. Howard, of Baltimore; G. J. Engelmann, of St. Louis; the President; and the discussion was closed by Dr. Sawyer.

Third Day—Afternoon Session.

The Society was called to order by the President, who introduced the newly elected President, Dr. William T. Howard, of Baltimore. After a few appropriate remarks by Dr. Howard, the Society, on motion by Dr. Reamy, extended its thanks to Dr. Smith for the faithful and impartial discharge of his duties as President.

DR. GEORGE J. ENGELMANN, of St. Louis, then read a paper entitled

A RARE AND FATAL FORM OF SEPSIS WITHOUT SYMPTOMS.

In the cases referred to, fever and pain are generally wanting. The symptoms which do appear are greatly at variance with those ordinarily accepted as characteristic of septic poisoning. There may be no depression of vital forces; no profuse sweats; no intestinal or bronchial catarrh; and the disease may or may not begin with a chill. It is the notable absence of symptoms and the fatal result which characterize this form of septic infection. It is rare that all symptoms are absent in cases of this class, but they may be. The author took issue with Dr. Barker, of New York, who had said that the symptoms of septicemia vary according to the amount of poison absorbed, and consequently the intensity of the disease; and he also took exception to the statement made by the same author that "elevation of temperature is constant and measures to some extent the intensity of the poison."

Dr. Engelmann had seen the condition to which he referred in connection with puerperal disease, uterine fibroids, pyemia and septicemia, diphtheria and enlarged prostate. Illustrative cases were reported. The paper was discussed by Drs. W. L. Richardson, of Boston; T. A. Reamy, of Cincinnati; Joseph Taber Johnson, of Washington, each of whom had seen similar cases, and the discussion was closed by Dr. Engelmann.

The following papers were read by titles: The Hygiene of Pregnancy, by Dr. Samuel C. Busey, of Washington; Rapid Dilatation of the Cervical Canal, by Dr. William Goodell, of Philadelphia; The Physiognomy of the Vulva as a Sequence of Anal Disease, and the Cause or Sustaining Cause of Uterine Disease, by Dr. Isaac E. Taylor, of New York; The Early History of the Treatment of Vesico-vaginal Fistule in the United States, with Statistics of the Several Modes of Operating, by Dr. Nathan Bozeman, of New York; Contributions to the Topographical and Sectional Anatomy of the Female Pelvis, by Dr.

David Berry Hart, of Edinburgh; Fibro-myomata and Fibro-cystic Myomata of the Uterus, with Cases and Specimens, by Dr. R. Stansbury Sutton, of Pittsburgh; On the Ring of Bandl, by Dr. William T. Lusk, of New York.

The following officers were elected for the ensuing year:

President, Dr. William T. Howard, of Baltimore.

Vice-Presidents, Dr. William L. Richardson, of Boston, and Dr. Paul F. Mundé, of New York.

Secretary, Dr. Frank P. Foster, of New York.

Treasurer, Dr. Matthew D. Mann, of Buffalo.

Council, Drs. Joseph Taber Johnson, of Washington; A. Reeves Jackson, of Chicago; H. P. C. Wilson, of Baltimore; and E. Van de Warker, of Syracuse.

The next Annual Meeting will be held in Washington, D. C., beginning on the third Tuesday in September, 1885.

NOTE.

It may be interesting to those members of the profession in this country who devote particular attention to Obstetrics and Gynecology to learn that the American Gynecological Society, with a view to extending its influence and inviting accessions to its ranks, has decided to increase the limit of its membership to one hundred (instead of sixty, as heretofore), and to do away with the rule requiring candidates to be recommended by the Council before being eligible for election. In future, candidates for admission must present a paper on some subject in obstetrics or gynecology, together with an application, indorsed by two Fellows of the Society, to the Secretary (at present, Dr. Frank P. Foster, 32 E. 31st st., New York), at least one month before the annual meeting, in order that their candidacy may be communicated to all the Fellows before the meeting; the election will then take place at the annual business meeting of the Society. The presentation of a paper is required, not so much as a test for the candidate's fitness, which is supposed to be sufficiently vouched for by his previous record, and by the testimony of the indorsers of his application, than as a guarantee of his willingness to contribute to the literary work of the Society. If such a paper were not required, candidates might be elected and attend the meetings now and then, without ever having contributed a written or spoken word to the Transactions. Such drones would be eminently out of place in an active working body like the American Gynecological Society.

The formality of a paper as an evidence of the fitness of a candidate would, therefore, naturally become a test, if the paper happened to be a careless or worthless production. It is precisely through its members and the excellence of their professional and literary work, that the Society, while desirous of extending its usefulness, proposes to preserve the high standard which it has hitherto aimed at. It is to be hoped that candidates will remember this hint, and insure the success of their application by a paper worthy of the Transactions of the Society. EDITOR.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF NEW YORK.

Meeting, February 19th, 1884.

DERMOID CYST OF THE OVARY.

DR. J. B. HUNTER related the following case, and presented the specimens: The patient was a widow, twenty-eight years of age, who had come to him some weeks before, stating that she had suffered for eight years from severe dysmenorrhea and metrorrhagia, which had grown worse from year to year. Her periods continued for fourteen or fifteen days. All the usual remedies had been tried in vain. Upon examination, Dr. Hunter found one ovary enlarged and lying behind the uterus. The other ovary could not be felt. A week ago he performed laparotomy, making an incision more than two inches long. The right ovary was found to be enlarged, and was removed; the fimbriated extremity of the Fallopian tube was pretty firmly attached to it. The left ovary was then removed. It was enlarged, and, on cutting into it, about one-half of its substance was found to consist of a dermoid cyst, containing hair and pultaceous matter. The tubes were also removed with the ovaries. The abdominal wound was closed in the manner which Dr. Hunter had adopted for some time: a silver-wire suture was passed through the abdominal wall and the peritoneum; the edges of the peritoneal surfaces were then carefully coaptated with catgut sutures, and the remainder of the wound was closed with silk sutures. The wound had healed by first intention, and the patient had had no bad symptoms. Dr. Hunter had obtained better results from this method of closing the abdominal wound than from any other.

DR. C. S. WARD remarked that the specimen was the smallest dermoid cyst of the ovary which he had ever seen. With reference to Tait's operation, he knew of no other surgical operation of equal magnitude which was so uniformly successful, so far as recovery from the operation was concerned.

DR. CLEMENT CLEVELAND inquired whether there was anything in the anatomical appearances of the structures of the pelvis which would account for the condition of the ovaries and the symptoms.

DR. HUNTER replied that the uterus was retroverted and one ovary lay behind it. It had been thought that the woman was suffering from cellulitis, but he found no evidence thereof. He thought that, where there was sufficient disease of the ovaries to interfere with their function, it was common to find aggravated pain. With reference to other methods of closing the abdominal wound in cases of laparotomy, he said that a number of patients were known to return with ventral hernia, or with a weakness of the abdominal walls, which was a source of discomfort to them ever afterward.

DR. WARD said it was not uncommon for hernia to occur after laparotomy for the removal of a large tumor, but he had never known it to take place after Tait's operation. This might be accounted for, in part, by the fact that Tait's operation had been comparatively recently introduced. He thought, however, that the real explanation was that the tonicity of the abdominal wall still remained, and was in no wise compromised by the operation. He had not had an opportunity to witness Dr. Hunter's method of closing the abdominal wound; it might be an improvement upon other methods. He had, however, had occasion to open the abdominal cavity several times after laparotomy for large tumors, and he had found firm union by adhesion of the peritoneal surfaces within forty-eight hours after the operation. He should think the same thing would occur after Tait's operation.

DR. HUNTER remarked that he had not seen any case of hernia following Tait's operation, but he had seen very weak abdominal walls, a condition which would favor hernia. He had seen imperfect closure of the peritoneum favoring abscess in the abdominal wall.

DR. WARD had seen abscess, but it had been extra-peritoneal, and it usually opened or was incised in the line of the sutures; he had never known a mural abscess burst into the abdominal cavity. The tendency in the peritoneal wound was to union by adhesion, and if mural abscess occurred, it followed the tracks of the sutures in seeking an outlet.

DR. HUNTER recalled a case in which a secondary operation was necessary, on account of an abscess which had broken into the peritoneal cavity. Abscess was more likely to develop in the cellular tissue than in the cavity.

DR. POLK said he lost a patient after performing Tait's operation from just the condition of which Dr. Ward had spoken. The patient was an unusually stout woman, and the abdominal wall was very thick. Of course, there had been no recent distention of the abdominal cavity, although she had had a child several years before. When he got through the operation, which was easily done, and started to bring the walls together, he discovered that, when he tightened the sutures, they cut directly through the peritoneum, and after putting in all the sutures save the last one, in running his finger underneath so as to see what the condition was, he discovered that each suture had cut through the peritoneum, and that the fascia was in direct contact with the intestines. He was so firmly impressed with the importance of bringing the peritoneum together that, in that case, he unwisely undid the sutures, and attempted to get the peritoneum together, but the intra-abdominal pressure was so great that it was an utter impossibility to accomplish it. He had said "unwisely undid the sutures," because he believed that the handling of the cut surface, in consequence of his efforts to bring the peritoneum together, was the direct cause of the formation of an abscess, which abscess began in the upper portion of the incision, and finally emptied into the peritoneal cavity, causing general peritonitis and death. So that the opinion he held was this: that all experience went to show that it was of the greatest importance to get the peritoneum together, and in ordinary cases of tumor, where the peritoneum was strong and well stretched out, it could be accomplished without difficulty; but in certain cases, where the condition he alluded to was present, it was not very wise to insist on hav-

ing everything just as we would like. Of course, the abscess might have formed in any event, but he was sure he had aided its formation very much by the handling that the cut surface was subjected to.

DR. POLK then showed two specimens, from different patients, and related the cases as follows:

DERMOID CYST OF THE OVARY.

The patient gave a history of pelvic discomfort, and upon examination, a tumor of about the size of an orange was found in the region of the right broad ligament, and nothing upon the left side, although all the pain was upon the left side. The uterus was displaced somewhat on account of the presence of this tumor upon the right side. The patient was not in very good condition for operation, because she had some bronchitis, which had been going on a good while. She had a good deal of cough with it—more than the amount of bronchitis seemed to account for. It was thought there might be a certain reflex element in the production of the cough. At any rate, the pain was so constant and her distress was such that an operation was considered advisable, and it was done last Saturday. The tumor which you see there was found on the right side, and proved to be a dermoid cyst. It was free, and consequently was removed without difficulty. The tube was taken out along with it. The tube on that side, however, was entirely free from any disease, and the fimbriæ were distended about as well as can be seen in any specimen. . . . When I searched the other side of the pelvis, I found that the tube was dilated, and that the outer end of it, the fimbriated end, had grasped the posterior surface of the ovary, and was firmly adherent—so firmly that the outer end of the tube was closed. That was interesting, as showing that it was the cause of the pain the patient suffered upon the left side. The tube on the right, as you see here, was free from disease. That on the left explained the difficulty. The ovary was cirrhotic, as we express it, so that I removed it. I used the ether apparatus of Clover in the operation, which, as you know, involves the re-inhalation of the ether over and over again, together with the expired air; so that you use a minimum amount of ether, but its use is always accompanied by more cyanosis than you see with either ether or chloroform administered in the ordinary manner. The result in this case was about as good as in all others in which I have used the instrument. There was no vomiting, and there was very little shock after the operation. The patient was quite blue, however, but the presence of a good deal of mucus in the bronchial tubes probably accounted for that in great measure. Everything went well until Sunday afternoon, about twenty-four hours after the operation, when the temperature suddenly rose to 104° F., and remained at or above that point until yesterday (Monday) afternoon, when it dropped to normal. The sudden rise of temperature I thought

was connected in some way with the wound, but as there was total absence of any local sign or symptom of inflammation there, and the temperature dropped so suddenly, I came to the conclusion it was in all probability due to some morbid condition of the lung, because there were such intense dyspnea and cyanosis. It was thought she would die: the temperature rose to 106° F., the breathing became very superficial, and she was as blue as we sometimes see children who are born cyanotic. However, her temperature dropped under the influence of the rubber coil, quinine, and mainly as a result of the subsidence of the inflammatory process, or whatever it may have been. To-day her condition is as good as it could possibly be—the temperature being normal, the pulse a little over 100, and the patient taking nourishment very well. The cyanosis in this case was no doubt intensified by the opium, although the latter was not given in unusual amount (a fluid drachm of Magendie's solution in the course of sixteen hours). The patient died on the fifteenth day after the operation, the cause of death being double catarrhal pneumonia, having its origin in the chronic bronchitis, and its immediate cause being the action of the ether and the opium.

MYOMOTOMY.

The other specimen is a uterus taken from a patient thirteen days ago. This patient had the ordinary history of fibroma of the uterus, with excessive menstrual discharge, which weakened her a good deal. I determined, therefore, to perform Hegar's operation upon her, and remove the ovaries and tubes, but I found that she had some albumin in her urine. Before that it had not shown itself, and this made me postpone the operation ten days. Finally, when it disappeared, her condition being good, I opened the abdominal cavity. I planned for the operation finally performed, but it was a plan that I did not intend to follow out unless forced into it. My original design was simply to take out these ovaries and the tubes, and possibly ligate the ovarian artery if I could get at it easily. I endeavored, before opening the abdominal cavity, to ascertain the condition of the ovaries, but it was an impossibility. I opened the abdominal cavity, and getting my fingers down, I found the ovary on the left side with no adhesions, but it was very difficult to get it up to the opening. The usual manœuvre of lifting the uterus by way of the vagina was resorted to, which brought it up—so well up that I got at it without difficulty. I found the ovary in close apposition with the pampiniform plexus, which upon that side was very much enlarged. In ligating the ovary, I attempted to use Mr. Tait's Staffordshire knot, but it slipped after I had cut away the ovary, and the stump, which contained not only the ovarian vessels, but a portion of the pampiniform plexus, was of such size that it was impossible to catch it. I made successive futile efforts to ligate it, but the more I attempted the worse I made it, for every time I tried to put the

ligature into tissue, I simply imbedded it in the pampiniform plexus, and the vessels opened and blood spurted. Under these circumstances there was nothing left for me to do but to remove the uterus. I therefore enlarged my incision, passed a rubber tube around the cervix, and cut away the body of the organ. I did not use the clamp, but simply left the rubber tube in position, using that as a ligature. I found that, owing to the thickness of the abdominal wall and the presence of a fibroid tumor on the anterior uterine wall (so low that it really encroached upon the internal os), I had difficulty in getting a good stump of sufficient length to be easily brought out of the wound. Still, by introducing pins and lifting it up, and by carefully turning in the peritoneum and stitching it to the stump below the ligature, I managed to get it in proper extra-abdominal position.

The patient has done very well up to to-day. The peritoneum has united firmly, but an abscess developed along the line of the sutures and prevented the proper union of the wall. By finding the pus and washing it out carefully, however, and putting on adhesive plaster, I have managed to get it together very well. The patient to-night is in good condition, but the slough over the stump (this is the thirteenth day) is still firmly adherent. I take that to be in part due to the fact that I was obliged to include a fibroid tumor in the ligature. The points of interest in the case to me are: the difficulties that I experienced in getting a proper stump, owing to the presence of the fibroid tumor, the thickness of the abdominal wall, the shortness of the pedicle, and then the facts connected with the bleeding from the stump of the ovary, owing to the shortness of the attachment of the organ to the posterior portion of the broad ligament in the proximity of the pampiniform plexus. But for this last fact, the patient would simply have been rid of her ovaries and tubes. But then, hemorrhage occurring, and it being impossible to arrest it, it became absolutely necessary to remove the uterus.

[*Note, June 13th.*—After a sharp attack of peritonitis, the patient made a good recovery.]

DR. W. T. LUSK inquired as to the exact way in which Dr. Polk applied the elastic ligature.

DR. POLK replied that he lifted the uterus out of the abdominal cavity, as there were no adhesions, just as in the Müller modification of Porro's operation, and put the tubing around it while it was thus raised, putting it around double. At first he put the tubing around single, but it broke.

He wished to correct a statement made in the original report: the parietal peritoneum was not in contact with the entire circumference of the stump below the ligature; the two structures were separated by the ligature at the upper side for a space about as large as one's finger-nail. This had induced him to put in a drainage-tube above the stump. The resulting sinus had not closed, and had caused him some anxiety, as, from its position, it had become a receptacle for the oozing from the sloughing stump.

DR. LUSK said that, when in London last summer, he saw Dr. Bantock operate in a case in which the tumor was about the size of that presented by Dr. Polk, and he was very much pleased with the action of the clamp and wire with which he drew out the uterus. The wire was sufficiently strong to guard against any possibility of breaking, and was so stiff that it could not be handled by the fingers, but required a special pair of forceps, which Dr. Bantock had devised for seizing it, turning it round the stump, and attaching it to the clamp.

DR. HUNTER had operated in a case similar to Dr. Polk's last summer, and in that instance he employed a piece of rubber tubing about as large as that on a Davidson's syringe, wrapping it twice around the stump, thus controlling the hemorrhage perfectly. He then put on the clamp, and cut off the entire mass. He reported the case at the time, but had not yet reported the sequela. A little sloughing took place around the stump, and the neck of the uterus came out entire. A fistula led from the abdominal wound down into the vagina, and required to be washed out for about two weeks. The sloughing was probably to be accounted for by a small portion of the vagina having been included in the clamp. The fistula had gradually contracted and closed, and the patient had done well. Examination by the vagina at present showed no remains of the cervix.

DR. POLK.—I wished to put a clamp on underneath this tube, but I really did not have the room. I was afraid to make greater traction, through fear of lacerating the tissue. So I had to depend upon what the pins would do for me, and they held it up very well.

DR. HUNTER remarked that, in his case, it was extremely difficult to raise the uterus high enough, and he was afraid the clamp would cut into the abdominal wall and cause the patient pain, but she did not complain of it in the least.

DR. WARD said that Hegar's operation, as compared with Tait's, was the more difficult and serious of the two. With reference to removal of the uterus, he thought fashion had guided the profession largely in the use of the elastic ligature. It seemed to him a better method was by the use of the wire *écraseur*, whereby the mass could be constricted, and by applying the ligature in its track. He believed in the intraperitoneal method of treating the stump, the same as in ovariectomy. The cases which he had observed did best if the stump was returned. Last summer he witnessed an operation by Dr. W. T. Bull, in which he constricted the uterus by the wire *écraseur*, and then ligated the stump with a strong silk ligature. There was very little bleeding during the entire operation. After the operation, Dr. Bull applied iodoform to the stump thoroughly, and dropped it, and the patient recovered without a bad symptom. It was true that, in some cases, the wire *écraseur* would cut into the uterine tissue if it was very friable; but in other cases it would work very well indeed. He believed that, by this method, sufficient constriction could be made with the wire to make ligation safe enough to allow of the pedicle being dropped.

RUPTURE OF THE UTERUS; LAPAROTOMY.

DR. E. L. PARTRIDGE related a case of rupture of the gravid uterus—less on account of any unusual features which it presented than because of the rarity of the accident. The patient was a

poor woman, under the care of a midwife who seemed to be an intelligent and honest person. She stated that the patient had been in labor since four o'clock in the morning, and that she had been with her since five o'clock. She had made examinations from time to time, and had distinctly felt the head presenting. The cervix was dilated to perhaps two inches and a half. The patient, while standing, not having had any specially severe labor-pains, suddenly exclaimed that something had given way, and immediately lay down upon the bed. The midwife again made an examination, and could not feel the head, as she had previously. She sent for Dr. W. E. Forrest, but he being out, she sent for another doctor, who remained by the patient two hours without appreciating the condition. This was about 11 A.M. Dr. Forrest then came and found rupture of the uterus, and sent for Dr. Partridge, who arrived perhaps an hour later. The pulse was about 120, not excitable, but fairly strong. There had been comparatively little external hæmorrhage, but more than one would expect in ordinary labor. Palpation showed that the uterus was pretty well contracted and situated toward the patient's left side. The fetus was evidently on the right side. An extremity could be distinguished through the abdominal wall, and the finger was passed upward through the cervix, through a rent in the uterus, toward the right side of the abdomen, where a foot could be distinguished, the head being in close proximity, but on a higher level. It seemed, therefore, that the head had been presenting, and that when the rupture occurred the head pressed through the rent and left the feet presenting. It was decided to open the abdomen and remove the child, although some of the physicians present favored extraction through the vagina. The operation was performed at 4 P.M. by Dr. Forrest. The incision in the abdominal walls extended from the umbilicus down to the symphysis. The bladder was found spread out like an apron, and, when lifted, extended up nearly as high as the navel. A small amount of urine had been withdrawn with the catheter just before the operation. The peritoneum presented an appearance as if there were gangrenous intestine beneath, but, after a moment, the cause of this appearance was recognized to be liquid blood, situated beneath that membrane. The peritoneum was opened, and most of the free blood in its cavity escaped before the child was removed. The child was easily extracted. The placenta was entirely within the abdominal cavity. The child was large, and, of course, was dead. The uterus was well contracted. The rent began on the left side, near the cervix, and extended to the median line, thence upward to within a third of the distance to the fundus anteriorly. The child lay on the right side. The peritoneum was stripped up from the edge of the rent in the uterus, so that it could be lifted from the wound to the distance of an inch in one place. The peritoneum had yielded in front of the head before it became lacerated.

rated, and then laceration took place not exactly in a line with the rent in the uterus. The rent in the uterus was closed with silk, five or six stitches being employed. The abdomen was very readily cleansed of the small amount of blood which was present. No liquor amnii was present. The abdominal wound was then closed. The pulse was now 115, whereas it had been 120. It seemed, therefore, that the immediate result of removal of the child from the abdominal cavity was an improvement in the condition of the pulse. Dr. Partridge did not see the patient after this time, but he was told that she lived for four days, and then died of peritonitis. It was recognized when the abdomen was opened that it would have been utterly impossible to extract the child through the vagina, and Dr. Partridge thought it might be said in a general way that, when rupture of the uterus had existed for some time, one could be pretty sure that the uterus would be found so contracted that the child could not be drawn through the rent. Anyway, it would be a question, if the child had escaped completely into the abdominal cavity, whether, under any circumstances, it would be proper to seize it and draw it through the uterine rent, for the procedure would certainly result in making the rent considerably larger. For that reason, then, if for no other, he thought it would be unadvisable to withdraw the child through the vagina. The pelvis was normal in development; the patient had borne children before. There was no apparent reason for the rupture. The uterine tissue seemed to be quite firm, and the stitches held perfectly well. There was no oozing from the uterine wound after it was closed. No autopsy was made.

DR. LUSK inquired if Dr. Partridge thought there would have been any advantage in removing the entire uterus.

DR. PARTRIDGE said he had often thought of the propriety of such a procedure, on general principles, but the question had not been raised in this case.

DR. LUSK said that, inasmuch as the laceration was principally in the lower segment of the uterus, the organ never contracted down upon it well, and he had often wondered if it would not be a safer procedure to take out the entire uterus. In the two cases which he had witnessed, the patients at first were in excellent condition for operation. The uterus was not removed, however, and they finally died after an operation similar to the one which Dr. Partridge had performed.

DR. WARD thought Dr. Partridge voiced the sentiment of the profession of to-day when he said it was unwise to attempt to extract the child through the vagina, especially after the occurrence of such a rupture as he had described. He believed that, in the majority of cases which had been reported, the child had escaped almost entirely through the rent, and under such circumstances he thought laparotomy was the only proper resource, and the suggestion made by Dr. Lusk, with reference to the removal of the uterus, was one which deserved serious consideration. He should certainly act upon that suggestion should he again be called to see such a case as he witnessed with Dr. Partridge

some years ago, in which the uterine walls were so thin and friable that it was with great difficulty that the sutures could be made to hold.

DR. W. R. GILLETTE thought the question raised by Dr. Lusk was one which might be discussed theoretically all night, and we should know as little at the end of that time as now regarding its possible practical value. It seemed to him, however, an unnecessary risk to submit the patient to, without promising corresponding advantages. It was just as possible to thoroughly cleanse the abdominal cavity and the uterine cavity after sewing up the ruptured uterus as after laparotomy for any other purpose, and it seemed to him that removal of the uterus under such circumstances would simply add other dangers to those which already existed.

DR. LUSK referred to the danger of gaping of the uterine wound, and added that the longer the stitched surface was, the greater this danger would be.

DR. POLK said that so few cases had been reported giving the results of suture in Cesarean section that we could hardly base conclusions upon them. In the case reported by Dr. Garrigues, however, complete union had taken place, as shown by the autopsy a short time after the operation.

DR. GILLETTE said that several years ago he was requested, by an old and quite well-known practitioner, to take charge of a woman in labor, in a rear tenement-house. The doctor had been in charge all night, and was desirous of relinquishing the case, saying that he was about to leave town and could not continue with it. Dr. Gillette consented to take the case, but insisted upon the doctor's remaining to examine the patient with him and give him the benefit of his knowledge of what had occurred through the night. While this preliminary conversation was going on, Dr. Gillette remarked that there were no ejaculations indicating pain upon the part of the patient. His suspicions and fears were considerably aroused, and were not allayed when, upon stepping into the lying-in chamber, he saw the patient looking like wax, with a pinched, worn, anxious expression, very feeble, and quite content from the fact that the pains had suddenly and entirely ceased some two hours previous.

Upon making an examination, he found evidences from palpation of a ruptured uterus, with the escape of the fetus into the abdominal cavity. The ordinary vaginal digital examination revealed no presenting part, but, upon introducing the hand, the worst fears were realized, and the case was thoroughly diagnosed as a typical one of rupture of the parturient uterus. It was, as memory told him, almost similar to the case related by Dr. Partridge. In those days little was known or thought of laparotomy, and Dr. Gillette proceeded at once to seize the foot of the child and deliver by version and extraction. It was accomplished easily. The placenta was removed by easy extraction. To shorten the story, the patient recovered without an alarming symptom, notwithstanding every disadvantage of squalor, filth, and inattention in the way of nursing was present. The case was not related for the purpose of advocating the method of treatment pursued in preference to laparotomy and the more modern and antiseptic surgical methods of treating such cases. On the contrary, Dr. Gillette thought the latter procedure was the proper one in the light of the advanced state of obstetrical practice of the present

day. Indeed, it would seem almost impossible, with our present views, for a woman to recover under the conditions which must have existed in the case related. There was no doubt of the complete rupture of the uterus, as the hand was passed completely into the abdominal cavity; and how recovery could take place with the abdominal cavity full of blood, amniotic fluid, vernix caseosa, and what not, was one of those mysteries which must forever confront the theories of antisepsis and drainage.

DR. LUSK said that cases of rupture of the uterus had been reported in which the peritoneum was simply lifted up, but left intact.

DR. GILLETTE supposed the peritoneum must have been ruptured in his case; at any rate, the child had escaped entirely out of the uterine cavity.

DR. H. T. HANKS had once been called to a case of rupture of the uterus, under the care of a midwife, and on his arrival had found the patient in a collapsed condition. He was surprised at the ease with which he could pass the hand through the uterine rent, seize the feet, and extract the child through the vagina. He remembered a case in the practice of Dr. Barton, of Orange, Mass., in which the child had also entirely escaped into the abdominal cavity with the exception of the feet. In this case also the doctor was able to pass the hand through the rent and extract the child. That patient recovered. Dr. Hanks thought that, where the rent was sufficiently extensive and the uterus had not so contracted but that the hand could be passed through the rent, extraction through the vagina should be preferred to laparotomy.

DR. PARTRIDGE said, further, with regard to removal of the uterus, that he did not know that there would be any special disadvantage in doing it, and in this case it was feasible, but, on the other hand, he could not say that the procedure would offer any special advantages unless the condition were such as in the case referred to by Dr. Ward, in which the uterine tissue was so degenerated and softened that sutures would not hold; or in case the uterine rent was so lacerated and irregular that perfect coaptation could not be obtained, and it might be expected that subsequent leaking would occur through it into the peritoneal cavity; or in cases of pelvic deformity in which subsequent pregnancy would be undesirable. Women who had sustained rupture of the uterus had lived to bear children.

The point which DR. LUSK wished to make was that, if it was dangerous to sew up the wound in Cesarean section, because there would be a tendency afterward to sloughing and gaping of the wound, allowing of the escape of the uterine contents into the peritoneal cavity, these dangers would be even still greater in a case of rupture of the uterus; and the question was, whether it would not be better to remove the entire organ, especially as the patient was usually in good condition for an operation.

DR. POLK inferred from the discussion which had taken place that it was the voice of the Society that, if the uterine rent were in a position to give favorable results from suture, the proper procedure would be to sew it up; but that, if the rent were in such a location, and the uterus in such a condition, as not to favor union, then probably removal of the uterus would be the better method to adopt. Each case, therefore, would have to be treated on its individual merits.

TRANSACTIONS OF THE OBSTETRICAL AND GYNECOLOGICAL SOCIETY OF WASHINGTON, D. C.

Stated Meeting, May 16th, 1884.

DR. S. C. BUSEY, *President, in the Chair.*

DR. H. D. FRY read a paper on

THE DIAGNOSIS OF PREGNANCY IN ITS EARLY STAGE, WITH SPECIAL
REFERENCE TO TEMPERATURE OF THE GENITAL CANAL.¹

DR. SMITH, in opening the discussion, said we were too prone to rely altogether upon the signs of pregnancy as laid down in the text-books. The question was, had we any valid reason for making a positive diagnosis of early pregnancy, by relying upon a rise of vaginal temperature. He had examined four women during this afternoon between 2 and 6 P.M. for their axillary and vaginal temperature, which gave results differing from those of Dr. Fry, and being anomalous if Dr. Fry's views were correct. The figures were as follows:

Case 1.	Vaginal temp.,	100.8°.	Axillary temp.,	99°.
Case 2.	" "	100.4°.	" "	99.4°.
Case 3.	" "	100.8°.	" "	99.8°.
Case 4.	" "	100.5°.	" "	99.5°.

None of these women were either sick or pregnant, and all but one were examined before dinner, so that the question of the effect of digestion could not arise. In Dr. Fry's cases of pregnancy, the average vaginal temperature was only 99.7°, while in the non-pregnant it was 99.1°.

The other symptom relied upon and added to that of rise of temperature was backache, but Dr. Smith could not recall a case where backache was a prominent symptom of early pregnancy. He had met with slight backache, but not amounting to more than any other ache of which women would complain, and if either of these aches was pathognomonic of pregnancy he had failed to discover it. Rise of temperature as a symptom of pregnancy, was mentioned by some authorities. Thus Meadows said that the vagina was the hottest accessible part of the pregnant woman. Dr. Smith did not believe there was any symptom which, taken by itself, was characteristic of pregnancy. A man's individual experience would more surely enable him to determine the existence of pregnancy than any rule laid down in the text-books. The knowledge gained by frequent examinations of women, enables a physician to reach correct conclusions almost intuitively. It was only by taking the symptoms as a whole, and not by selecting a single one, that correct conclusions were to be reached. All know how fallacious a symptom suppression of the menses is, even when associated with nausea. Dr. Smith then gave the symptoms which usually go along with pregnancy, and laid special stress upon the existence of an enlarged uterus, as determined by conjoined manipulation, when associated with other symptoms. A short time ago,

¹ See original articles in this number.

he was requested to examine a young woman who had been indiscreet, in order to determine whether she was pregnant or not. He was not permitted to see her face, which was covered with a sheet, nor to ask any questions except through her female friend who was present. A digital examination and the conjoined method showed that the uterus was small. Her breasts were found to be normal, showing no areola except such as is usual in virgins. He therefore gave the opinion that the woman was not pregnant, but that the suppression of the menses was due to the fact that she was frightened at the possible results of her indiscreet doings. By a peculiar coincidence, some weeks ago, he was called upon, in one afternoon, by four women who wanted to know whether they were pregnant or not. Two of these were married, one a widow, and one a "grass-widow." In examining one of them, he found blood at the end of his finger, and predicted that she would be unwell before next morning; and in the other cases he was able to state that no pregnancy existed. The greatest difficulty he found in cases of retroversion; he was reasonably positive when the uterus was in a normal position or when it was anteverted, but in retroversion he could not utilize conjoined manipulation for the purpose desired, without using more force than was justifiable in such examinations. As to the symptom of *flat belly* insisted on by some, he had no faith in it. Unfortunately we cannot always believe what women tell us, and it is a good practice to adopt Gooch's aphorism: "Never rely upon the evidence of their tongues, but on that of their bellies."

DR. J. T. JOHNSON.—Dr. Fry had not succeeded in pointing out a positive sign of early pregnancy, any better than he (Dr. J.) had in the paper referred to by Dr. Fry. The importance of the subject demanded that we ought to be able to give a positive opinion, but in fact we were no better able to do so now than others were a hundred years ago. He thought that too much faith had been placed in rise of temperature of the genital organs, for this had failed in too many cases. Rise of temperature only showed that there was congestion, without giving the cause of the latter. Dr. Smith relied mainly upon the enlarged uterus, when accompanied by other signs and symptoms, but the trouble was that we were not asked for an opinion when all the palpable signs were present. Dr. Smith's patients, as well as Dr. Fry's, had had children, and a woman of this kind would not be likely to consult a physician when all the usual symptoms were present; they asked advice when the usual signs were absent. In his paper, referred to by Dr. Fry, Dr. Johnson mentioned a case in which the patient desired to visit Europe on a pleasure trip, provided she was not pregnant. She had missed her menses once, and presented a dark areola, and other signs; she was, therefore, advised that if her menses failed to appear at the expected time, she had better not leave her home. Her menses did not appear, and her friends left without her, shortly after which the menses came on. In the discussion on his paper, all agreed that if an enlarged uterus could be made out by palpation, and if the neck was soft and larger than normal, we might rely on these as signs of pregnancy, provided the woman had been exposed to the risks of becoming pregnant. As to taking the intra-cervical temperature, by placing the bulb in the canal, he held the practice to be risky, a number of cases of abortion having followed thereon. Dr. Smith had, in four cases, pronounced that the parties were not pregnant, but had not stated on

what single sign he reached his conclusions. (Dr. Smith said he relied upon the conjoined manipulation.) Dr. Johnson admitted that enlargement of the uterus, proved by manipulation, together with other signs, seemed to help us out of doubt in most cases.

DR. MCARDLE inquired of Dr. Smith, how long the ladies examined by him had missed their sickness. (Dr. Smith: Two months.) And would the doctor be willing, upon the strength of his examination, to introduce a sound into the uterus. (Dr. Smith: No.) Then how could he give a perfect diagnosis of absence of pregnancy?

DR. SMITH said he took care to leave a loophole, in case of a possible mistake. He would state that, to the best of his knowledge, a woman was not pregnant, and yet leave room for doubt so as to protect himself. There was another point, viz.—the differentiation of membranous dysmenorrhea and early pregnancy. Young married women sometimes cast off menstrual membranes and call it abortion. In a recent case, a young lady who believed herself pregnant after jumping a fence and other violent exercise, had membranous dysmenorrhea, shedding a perfect cast of the uterine lining. In these cases, as also in retroversion, the diagnosis was difficult.

DR. HAGNER suggested whether we might not get the *placental bruit* or the *fetal heart-sounds*, by placing a stethoscope on the neck of the uterus. He also threw out the idea that, with the present strides made in the application of electricity, a delicate galvanometer might show the difference between the passage of a current through water and solid walls. If there was no pregnancy there would, of course, be no amniotic fluid, and the slips of platinum would not separate, while they would separate if the current passed through fluid.

DR. ADAMS said that the remarks made applied to cases where there was no difficulty in securing an examination, but not to those where it would be disastrous to a young practitioner to ask for a vaginal examination. He mentioned the case of a young lady who consulted him for suppression of the menses of several months' duration, which she attributed to cold, contracted at the time of an attack of measles. There was no abdominal enlargement; she had a distressed look and some cough. He declined to give an opinion, but thought she was pregnant. A month after, a thorough examination revealed softenings in the apex of the right lung. She has had two attacks of epistaxis, but the menses have not returned, and there is no enlargement of the abdomen. If he had been hasty in his conclusions, he would have condemned the girl unjustly. These are the cases where difficulty of diagnosis arises.

DR. BUSEY could corroborate the value of rise of cervical temperature as a diagnostic mark of early pregnancy. His experiments, some years ago, had led him to rely on the difference between the cervical and axillary temperatures, the rise in the former being from 1° to 1.5° in pregnant cases. He had applied this in a number of cases seen in consultation, and the results verified his opinion. In one case, the physician, the wife, and the husband all denied the presence of pregnancy, but the result settled it. In another case he saved the woman from being operated upon by determining pregnancy through the thermometer. At the same time he pointed out that this rise of cervical temperature might be due to other causes, which should first be excluded. He always took the temperature in the cervix and axilla both,

and had twice in his life proved that a woman was not pregnant when she was supposed to be so. In one of these cases the attending physician, the husband, and the nurse declared that the woman had gone to ten months. The thermometer showed no rise of temperature and the woman was not pregnant at all. In the second case the lady stated that she was in her seventh month; there was no rise of temperature and no pregnancy. He did not rely on vaginal, but exclusively on cervical rise of temperature, although his recollection of the difference between vaginal and axillary temperature corresponded with Dr. Fry's figures. While sure of its diagnostic value, he did not assert that it was pathognomonic. We could not rely on one symptom alone in pregnancy. (Dr. Fenwick asked as to the value of the dark areola in cases of first pregnancy.) Dr. Busey had lately determined a diagnosis by a dark areola and the frequency of micturition; but we should group a number of symptoms together, and this, as a rule, placed the case out of doubt. At the same time, grave errors had been committed; thus, Kimball, of Lowell, admitted that he had opened a pregnant woman under a mistaken diagnosis.

DR. JOHNSON.—Dr. Fry had claimed that the rise of temperature in the vagina of $.7^{\circ}$ indicated pregnancy. How would he account for the discrepancy between his and Dr. Busey's figures of 1° to 1.5° ?

DR. FRY.—His average rise was $.8^{\circ}$.

DR. KLEINSCHMIDT, referring to the value of the dark areola, said that in very light complexions we could place no reliance upon the areola's changes. He had saved the reputation of a young girl who had had a baby, and who was suspected by her family, by showing her mother the unchanged areola, which absolutely gave no sign that pregnancy had ever existed.

DR. FRY, in closing, said his paper had presented nothing positive, and he had from the start apologized for the negative character of his results. He had simply held that a vaginal temperature $.7^{\circ}$ above that of the axilla was presumptive of pregnancy, if there was no fever or other local cause. Cohnstein, in his first paper, advised to place the bulb of the thermometer in the uterus between the membranes and the walls, holding that no bad effects resulted. In a later paper, however, he said that the practice was dangerous. As to backache as a symptom, he had not spoken of it as prominent, but it was present in four of his ten cases; it was one of the signs of uterine congestion, as was the purple discoloration of the vagina, and neither was of great value in a diagnostic sense; nor were headache and nausea. Dr. Smith had claimed that the fingers, in skilful hands, were quite enough, but a sufficient number of errors had been committed in that way to caution us. Thus, in a woman who had borne children and whose uterus was in a condition of hyperplasia, bimanual examination would afford no help. As to Dr. Hagner's reference to the use of electricity, he had suggested some delicate instrument, thinking of Langley's actinic balance, which was so delicate that it would determine the temperature of the moon's rays. He thought the symptoms in Dr. Adams' case suggested phthisis and not pregnancy.

INTERNATIONAL MEDICAL CONGRESS.

HELD AT COPENHAGEN, AUGUST, 1884.

SECTION FOR OBSTETRICS AND GYNECOLOGY.

Reported by LEOPOLD MEYER, M.D., Copenhagen. (Translated from the *Centralblatt für Gynäkologie*.)

August 11th, Morning Session.

President, STADFELDT (Copenhagen); later, HEGAR (Freiburg).

P. MUELLER (Berne) presented a paper on:

THE CESAREAN SECTION AND ITS MODIFICATIONS.

The chief dangers to be feared in Cesarean section are hemorrhage and sepsis. The old Cesarean section shows a mortality of over eighty per cent. The advances made by abdominal surgery could not fail to extend to the Cesarean section, two different paths having been followed:} either a more radical procedure was carried out and the entire uterus extirpated, or else a more conservative spirit prevailed and the dangers were sought to be obviated by careful treatment of the uterine wound. The results alone must decide whether the radical or the conservative method is preferable. However, the mortality percentage is known only of the radical modification, amounting to a little more than fifty per cent. On the other hand, there are no available statistical figures regarding the conservative method which thus far has been too rarely tested in comparison with the former. For this reason it cannot yet be foreseen to which method the prize will finally be awarded. If the conservative method is to gain recognition, it will have to fulfil another indication, that is, it must protect the bearer of the retained uterus against rupture of the latter organ in a subsequent pregnancy, as well as against the dangers of a repeated Cesarean section.

EUSTACHE (Lille) would prefer the Cesarean section to the Porro operation. The mortality, which by the way is very difficult to determine, is nearly alike for both operations, about fifty-five per cent. However, the investigations of his assistant, Dr. Dancourt, have shown that of 30 women on whom the Porro operation had been performed, and whose subsequent history he had been enabled to follow, nine presented a very bad state of health after the operation. Inasmuch as the Cesarean section is less mutilating, and as two-thirds of the women remain well after this operation, the latter is to be preferred.

SAENGER (Leipzig) spoke in defence of his modification of the classic section. The "sero-serous" (peritoneal) suture is the main point; if the edges of the uterine wound are well coated, the subperitoneal resection of the muscularis may be dispensed with. Thus far, four cases had been operated on according to his method, viz., Leopold, three cases, three recoveries; Beumer, one case, which died of an old pyelonephrosis. As regards the statistics, all the Cesarean operations which enter into this consideration (*i. e.*, those per-

formed since 1876) must be divided into several categories. We then find 13 cases performed according to his method or which at least approximate the principle of a rational suture of the uterine wound; of these 13, there were 9 recoveries, or 69.2%. There remain 11 cases of Cesarean section with but one recovery. Säger by no means rejects the Porro operation altogether, but looks upon it as an exceptional resort and has formulated for it the following five indications: 1. Stenoses and atresiae of the cervix and of the vagina; 2. tumors of the pelvis occluding the obstetric passages; 3. pregnancy in the rudimentary horn of a uterus bicornis (S. himself has successfully operated in such a case); 4. osteomalacia, as it appears that the extirpation of the uterus herein has a favorable influence (Fochier, Fehling); 5. repeated Cesarean section. To this Martin has added a sixth indication: when grave dangers to the mother are to be expected from the puerperium after the ordinary section.

LEOPOLD (Dresden) reported on his three cases operated on according to Säger's method: all of the three mothers and children still enjoy the best of health. The course in the first and third case was uncomplicated throughout: in the second case, there ensued from the second to the eighth day a profuse hemorrhage beneath one of the broad ligaments which was gradually absorbed. In future cases, the speaker thought he would be able to do without the resection and protect the wound sufficiently from the uterine cavity by sutures, after the peritoneum had been folded in. Exceptional cases, in which extirpation of the uterus is indicated, will, however, continue to occur.

PAPILLON (Lille) corrected an error in the statistics given by Eustache.

LAZAREWITSCH (Charkow) rejects the resection of the muscularis, as thereby the wound is likely to gape in the direction of the uterine cavity.

LEOPOLD MEYER (Copenhagen) expressed surprise that laparotomy, or Thomas' operation in American parlance, had not been mentioned at all. It is universally represented as a not very difficult operation and has given good results in the United States.

BREISKY (Prague) finds that the whole statistical basis is too weak, the numbers too small, every additional case may give rise to essential variations. For the present, the speaker thinks the superiority of the Porro operation, as regards prognosis, is established. In substantiation of this view, B. reported on five cases operated on by himself according to this method; all the mothers had been saved. When in a case of contracted pelvis it is proposed to operate in the second labor according to Porro, this shows a great concession to this method. B. would only in exceptional cases perform the old Cesarean section, but in that event would gladly accept Säger's propositions.

P. MUELLER, in conclusion, repeated that he did not think the entire question was ripe for discussion. Still he again pointed out the doubtful fate of those women on whom the Cesarean operation had been performed according to the conservative method. This speaks strongly in favor of the Porro method. Moreover, the speaker emphasized the facility with which the Porro operation could be performed, in comparison with the rather complicated method of Säger. M. again called attention to the fact that the Porro operation comprises quite a number of procedures which *a priori* cause an unfavorable prognosis.

Howitz (Copenhagen) read a paper on:

DIAGNOSTIC DIFFICULTIES DUE TO HYPERTROPHY OF THE
CERVIX UTERI.

When the supravaginal portion of the cervix is elongated, the uterine diaphragm (Aran) is displaced upward, the internal os is often high above the pelvic inlet, and the corpus uteri is completely in the abdominal cavity. If then the body of the uterus be enlarged (by a tumor and especially by pregnancy), great diagnostic difficulties may be presented. The tumor felt in the abdomen appears much larger than would be expected from the estimated period of gestation, because the whole corpus uteri can be palpated through the abdominal parietes, while ordinarily a large part of it is in the pelvic cavity. The tumor is characterized by abnormally great mobility, the latter in fact is so great that the tumor is easily taken for an ovarian cyst with long pedicle. On the other hand, the fetal heart-sounds can be heard at an unusually early period, owing to the elevated position of the womb. The tumor always feels fluctuating, almost undulating, without hydramnios having ever been found by the author. The following may be the explanation hereof. The hypertrophic cervix is firm, rigid; does not become soft and doughy as in pregnancy generally, and does not take part in the formation of the ovisac. Thus the cavity of the body is dilated more than usually. That its walls are in abnormal tension is indicated, moreover, by the intermitting, labor-like pains felt by all of these patients. On exploration the cervix feels firm and hard; above is felt, especially per rectum, a rounded body which is easily mistaken for the fundus. From this point a short pedicle leads to the supposed tumor. That under these circumstances mistakes are readily made is not to be wondered at, and indeed there are cases enough where physicians of limited experience have sounded the uterus and thus caused abortion, or even performed laparotomy under the impression of having to deal with an ovarian tumor. But when bearing in mind the possibility of such a supravaginal hypertrophy occurring, the mistake will be avoided. As instances of this combination of cervical hypertrophy with uterine enlargement, the author reported in detail three cases of pregnancy and one case of fibro-myoma of the uterus complicated with supravaginal hypertrophy of the cervix; in all these four cases the diagnosis could be made only with great difficulty.

MARTIN (Berlin) would prefer to call the condition elongation rather than hypertrophy. The speaker had seen several such cases and likewise found great diagnostic difficulties, to which the always obscure history materially contributed its share. Irregular hemorrhages, profuse secretion, pains, and general debility prevent the patients, and even the physicians, from suspecting pregnancy. Superadded to this is the great mobility of the corpus, and the fact that the upper end of the elongated cervix lies far back in the pelvis and hence is reached with difficulty.

PRIESTLEY (London) had recently seen such a case in which the diagnosis was exceedingly difficult, rendered all the more so by the enormous development of adipose in the abdominal walls. The first definite sign here were the intermittent uterine contractions—specially emphasized by Braxton Hicks—which could be provoked by touch. The labor was very difficult, mainly by rigidity of the cervix, and had to be terminated with the forceps. The question is, whether in such cases the cervix had not better be amputated, particularly before the onset of pregnancy.

HOWITZ had seen altogether twelve cases which will be published elsewhere. He could not coincide with Martin that these patients generally feel very ill. In reference to Priestley's remarks, he stated that in all the cases under his observation the women had been delivered without difficulty.

Afternoon Session.

President, PIPPINGSKÖLD (*Helsingfors*).

KOEBERLÉ (Strassburg) submitted a paper on:

THE TREATMENT OF FIBRO-MYOMAS OF THE UTERUS BY LAPAROTOMY.

The indications for laparotomy depend upon: 1. Excessive and protracted hemorrhages, and a rapid and continuous growth of the tumor; 2. The more or less advanced age of the patient: the farther removed the patient is from the menopause the earlier the operation will be indicated; 3. The site of the tumor: those located in the lower segment of the uterus or in the broad ligaments always offer an unfavorable prognosis for operative interference; 4. Special conditions, when the tumor makes life a burden to the patient. The operation is *contraindicated* when extensive vascular adhesions exist between the tumor and the anterior abdominal wall; when the tumor is wedged in the lesser pelvis; when there is ascites which tends to increase and reproduce itself rapidly; when there are present incurable diseases or such as may jeopardize the progress of recovery. Regarding the *method of operating*, the incision should always be made in the median line. When the incision extends beyond the navel, the latter is excised. In pedunculated tumors, the pedicle is simply ligated (in two or more portions) and dropped. Comparatively small, limited sessile tumors can be enucleated and the wound then united linearly with dropped sutures. This method, which K. has employed since 1878, gives no absolute protection against hemorrhage and should be resorted to only in exceptional cases. The large tumors can be removed only with partial or total extirpation of the uterus. These cases are divisible into two classes; one in which the tumor extends no farther than the internal os and is not adherent to the ligaments, the other in which it reaches into the cervix or has developed subperitoneally. The former class of tumors can be operated on in various ways; usually K. isolates the uterus on both sides

by a series of dropped sutures, applies two metallic ligatures to the cervix, tightens them with *serre-nœuds*, removes the tumor, and fastens the stump in the abdominal wound by a transfixing needle. In this manner the author has operated on about fifty cases. The mortality is about the same as in the extraperitoneal ovariectomy operation, five to ten per cent. It would be better if the pedicle could be dropped; but K. fears that thereby the hemostasis could not be certainly controlled. When the tumor is situated in the broad ligaments, the operation is almost invariably fatal. Here oöphorectomy is preferable, but is often difficult, sometimes useless, for instance, where occlusion of the intestine indicates the operation. Schroeder's method, seductive though it appears, is too difficult and too dangerous, and admissible in exceptional cases only. The author makes the toilet of the peritoneum with napkins, never uses sponges. He closes the abdominal wound in the manner several times described by him (deep sutures inclosing only the fibrous parts, superficial harelip sutures). The peritoneum is never stitched, being altogether superfluous and a waste of time. The wound is covered with charpie impregnated with iodoform. The wound surfaces, as well as the dropped ligated portions, are all powdered with iodoform, but the least possible quantity. Otherwise the author employs no antiseptic measures. The dressing is changed three or four times a day.

APOSTOLI (Paris) read a paper on:

THE ELECTRIC TREATMENT OF FIBRO-MYOMAS OF THE UTERUS.

While the former electric treatment of fibro-myomas was characterized by weak intensity of the currents and by the, frequently vaginal, application of the active pole, the method of this author is distinguished by the constant current, which is the only one employed, being very intense, reaching even as high as one hundred milliampères, and also in that the active pole is always applied within the uterus. If the platinum sound cannot be introduced, a false passage is made by cauterization. The positive pole finds application when hemorrhages and leucorrhea are the most important symptoms; the negative pole when there is internal dysmenorrhea or when pelvic cellulitis has become superadded to the formation of tumors. The tumors shrink, but do not disappear altogether, and the patients recover completely, as shown by more than one hundred observations made by the author.

WIEDOW (Freiburg) spoke on:

OÖPHORECTOMY FOR UTERINE FIBROMATA.

He had collected the entire number of oöphorectomies thus far performed for uterine fibroma, partly from reports in special journals, partly from private information, and thereby he is in the position to give the permanent result after the operation and the future fate of the patients. His main object was to show the

influence on the arrest of the hemorrhages and on the atrophy of the tumors. The results were about as follows: Total number of operations collected, 149, of these patients 17 (11%) died of the results of the operation. For the purposes of this investigation, however, we can utilize only those cases which were under observation for at least one year after the operation. There remain 49 cases, with the following results: 36 times *menopause and diminution of the tumor*; 3 times *menopause* (no report on the tumor); 1 times *diminution of the tumor* (no report on the hemorrhage; 8 times *diminution of the tumor with slight regular or irregular hemorrhages*; 1 times *slight hemorrhage at quarterly intervals* (no report on the tumor). These results may be called excellent. The author has also endeavored to ascertain whether and in what manner the result of the operation has been modified by the position and the size of the tumor. As to the *position* of the tumor, two cases of *cervical myoma* are reported in detail; in both, the hemorrhages and other troubles disappeared after spaying, and in one case the tumor atrophied. As regards the size of the tumor, in 12 cases in which the tumor reached to and beyond the umbilicus, the results were as follows: in 10, the menopause and diminution of the tumor ensued; in 1, irregular slight hemorrhages, tumor slightly diminished; in 1, menopause after several months, regular unimportant hemorrhages with diminution of the tumor. Accordingly, the size of the tumor is no longer to be looked upon as a contraindication of oöphorectomy. Altogether, it appears to the author that, according to the results here recorded, oöphorectomy must occupy the first rank; myotomy (with a mortality of 33%) the second position. The speaker would restrict myotomy to, 1st, fibro-cystic tumors; 2d, pedunculated subperitoneal tumors in which the disposal of the pedicle is easy. In conclusion, the speaker expressed himself as opposed to Tait's view, that the extirpation of the tube is the main thing. The present case-reports suffice to show the error of this view; again and again the tubes have been left behind, and still the menopause and atrophy of the tumor resulted.

DISCUSSION ON THE LAST THREE PAPERS.

OLSHAUSEN (Halle) spoke mainly against the contraindications formulated by Koeberlé. Wedging of the tumor in the lesser pelvis is a contraindication as little as ascites; both these conditions rather augment the indications. Vascular parietal adhesions cannot be diagnosticated; at all events they do not contraindicate myotomy. The main difficulties in myotomies arise: 1st, in the subperitoneal development of the tumors; 2d, in cervical myomas. In the subperitoneal development, if the base of the tumor cannot be reached, ligatures *en masse* must first be placed at all available points, then the peritoneum is to be incised, and the tumor enucleated, which can be done not rarely without profuse hemorrhage. At times we must be content with partial operations, which are permissible if the ovaries are removed. In the case of cervical myomas, we may be forced to perform total

extirpation which, however, is exceedingly difficult from above. Is it permissible to leave the lowest part of the tumor behind? The intraperitoneal disposal of the pedicle will be that of the future. The elastic ligatures must be preserved in one-per-cent (not one per mille) aqueous solution of corrosive sublimate, and can then be dropped without harmful results. Drainage is to be discarded.

SPENCER WELLS (London) fully agrees with Olshausen that neither ascites nor vascular anterior adhesions form a contraindication. The other difficulties pointed out by K. and O. are nowadays of much less importance since we have adopted the strictest antiseptic precautions in the operation; among these precautions, cleansing of the vagina in particular, and its packing with iodiform gauze must not be forgotten. Silk is the best material for ligatures; only for the disposal of the pedicle the elastic ligatures are preferable. By compression with Wells' large clamps, the ligatures *en masse* can often be entirely dispensed with. Thanks to the elastic ligatures, the intraperitoneal method will very probably supersede the extraperitoneal disposal of the pedicle. Drainage has not been employed by the speaker since 1878. Why K. does not use the peritoneal sutures is difficult to understand. The speaker begged to inquire whether K. had ever witnessed any bad result from them.

HEGAR (Freiburg) emphasized the fact that the disposal of the pedicle must vary with the anatomical peculiarities. The intraperitoneal method might perhaps be preferable if Schroeder's procedure could offer sufficient guarantee against hemorrhage, which is not the case. Nor have the elastic ligatures proved reliable in the disposal of the pedicle; too often they give rise to abscesses. Thus far the extraperitoneal method has given the best results; that the after-treatment is troublesome cannot have any weight. Special mention should be made of the "wild operations" to which no method is applicable. These are cases of subperitoneal development and of cervical myomas. In subperitoneal tumors, the operation may be very easy, but is usually very dangerous. In the case of cervical myomas, removal by laparotomy gives very bad results; for these the speaker recommends oöphorectomy, which is generally not difficult, as the ovaries are lifted up by the tumor. Altogether, in regard to the question whether oöphorectomy or myomotomy is preferable, H. occupies the same standpoint as Wiedow.

KNOWSLEY THORNTON (London) finds, like Sp. W. and O., that ascites as a rule indicates rather than contraindicates the operation; a very rapid re-accumulation of the fluid still would show malignancy. The speaker knows only the following contraindications: malignancy; concomitant diseases such as nephritis, etc.; enormous size of the tumor, where the patient forms merely an appendix to the tumor. In very many cases of fibro-myomas no operation at all is indicated. Oöphorectomy is to be recommended especially in tumors the size of a child's head; in bulky tumors this operation is generally difficult and its use questionable. Enucleation from the vagina is to be rejected unless it has commenced spontaneously. The extraperitoneal treatment of the pedicle with Koeberlé's *serre-nœuds* has thus far given the best results. The speaker never employs drainage, but he approves of the peritoneal sutures. Sponges are to be preferred to napkins.

August 12th, Morning Session.

President, PRIESTLEY (London).

CONTINUATION OF THE DISCUSSION ON THE TREATMENT OF FIBROMYOMAS BY LAPAROTOMY.

MARTIN (Berlin).—The tumor itself is to be removed, not only in order to disperse the immediate symptoms, but so as to avoid subsequent disintegration or degeneration. Contraindications are: 1st, the general condition; 2d, the occurrence of malignant alteration. Adhesions we must endeavor to overcome, and one attains more and more skill herein. The operation must terminate with the complete closure of the abdominal wound and the peritoneal investiture of the stump or of the wound surfaces. In order to guard against sepsis, drainage should be established toward the vagina.

HOWITZ (Kopenhagen) expresses himself very decidedly against the too frequent performance of laparotomy for uterine tumors. It is but rarely indicated, and the supravaginal amputation of the uterus is very grave, the mortality being at least thirty-five per cent. (The speaker has lost 5 out of 13 cases.) The hemorrhage is usually due to disease of the endometrium and can often be overcome by scraping. Should this or some other intrauterine treatment remain ineffectual, oöphorectomy should be tried, but supravaginal amputation only in those cases where the operation is indicated by the rapid growth of the tumor, and when ergot and electricity have failed.

KALTENBACH (Giessen) spoke of the dangers lurking in the development of extensive thrombosis of the veins of the pelvis and thigh. The extraperitoneal method gives the greatest security against hemorrhage and sepsis. K. has tried Schroeder's method recently in four cases, but in two of them had to change to the extraperitoneal mode on account of insufficient control of hemorrhage. The elastic ligatures are liable to give rise to necrotic disintegration of the included portions.

SÆNGER (Leipzig) expressed himself in favor of the intraperitoneal method. In four cases he had used a combination of Schroeder's and Olshausen's methods. The elastic ligatures should not be drawn too tight. All the four cases recovered, three without any untoward accidents. The speaker called attention to still another danger in fibro-myomata, that of cardiac dilatation which in a fifth case caused death twelve hours after the operation.

KOEBERLÉ, in conclusion, stated that the contraindications given (ascites, vascular anterior adhesions, etc.) are not to be looked upon as absolute. He, too, does not use drainage.

MIKULICZ (Krakau) read a paper on

ANTISEPSIS DURING LAPAROTOMY.

The principles of the antiseptic procedure during laparotomies are the same as in all other operations, but the technical execution deviates materially in some points. From the standpoint of antisepsis, the most important qualities of the peritoneum are: 1st, The large *superficial extent*; 2d, the *great power of absorption and transudation*; 3d, the capacity of furnishing a plastic exudation

and rapidly to form firm adhesions. Herefrom may be deduced for antisepsis in laparotomies: A. That the peritoneum, *per se*, is far more sensitive to all septic infectious materials than every other tissue. Besides, *irrigation* is not applicable here because, on account of the rapid absorption, the danger of intoxication is very great; furthermore, that *drainage* is ineffectual. For these reasons the main object of antisepsis during laparotomy lies in absolutely keeping septic matters from the peritoneum. Therefore, antisepsis during laparotomies is mainly prophylactic. Drainage of the peritoneum is quite superfluous in the greatest number of operations performed aseptically. B. The second chief object of antisepsis during laparotomies is, from the beginning to guard against the formation of "dead spaces," i.e., to prevent the accumulation of secretion in the peritoneal cavity, so as to counteract spontaneous infection. Pre-eminent among these is a careful toilet, then the avoidance of secreting wound surfaces in the peritoneal cavity by various means, such as ligature *en masse*, actual cautery of pedicle and adhesions, the stitching together of raw surfaces, etc. Moreover, we must avoid everything that would incite the peritoneum to increased secretion, e.g., irritation by the stronger antiseptics. Of the least importance for the course of a laparotomy is the actual dressing of the wound; a narrow strip of some antiseptic dressing material suffices completely for the abdominal wound. Of antiseptics, carbolic acid thus far seems to be the best for laparotomy.

KÜSTNER (Jena), basing on the experience had at B. S. Schultze's clinic, warmly recommends the sticks of solidified bromine, made by the Schering'sche Fabrik, for the disinfection of rooms. The sulphurous acid often employed for this purpose is altogether worthless.

OLSHAUSEN (Halle) would not, like Mikulicz, stitch the sac into the abdominal wound in incomplete extirpation of ovarian tumors. This procedure nearly always provokes protracted, exhausting suppuration. The remnants of cysts can be quietly left behind; with careful antisepsis this can be done without any danger, even if a complete toilet is impossible.

(To be continued.)

REVIEWS.

PRACTICAL MANUAL OF DISEASES OF WOMEN AND UTERINE THERAPEUTICS. FOR STUDENTS AND PRACTITIONERS. By H. MACNAUGHTON JONES, M.D., etc., Examiner in Obstetrics, Royal University of Ireland, etc., etc. London: Baillière, Tindall & Cox. 1884.

In the preface to this manual, Dr. Macnaughton Jones pays his respects to specialism and the specialists. He fears that in so doing he may appear unorthodox. We would assure him that, on the contrary, he appears simply old-fashioned, or even as Rip Van Winkle

must have seemed when, on awakening from his lengthy sleep, he was surprised to find things changed around him. Specialism has long since ceased to be an evil in medicine, if it ever were one, and specialists are simply a necessary result of the rapid pace at which additions to medical knowledge have been made during the past decade. We fail to see the "many disastrous consequences which have followed that modern parcelling-out of the body into segments, and the handing over of a small piece of it to this or that specialist"! We think, on the contrary, that the medical art is to-day better practised and human ills more completely relieved than was the case when the human mind was per force obliged to master each and every detail of a naturally vast and complicated science. And, therefore, we cannot view with alarm the fact that the student looks on "the treatment of women's affections as a specialty." He is perfectly justified in so doing and simply thus shows himself abreast of the times. If, before attempting to practise on a species, he be only well informed in the genus, it matters little if Dr. Jones would rank him amidst the "mushroom-like brood of specialists" which he daily sees sprouting into existence. And, if after "some experience of special work," which Dr. Jones confesses to have had, the average gynecologist is unable to agree with Dr. Jones in the express opinion that ophthalmology and otology should be the only specialties, this may possibly arise from the fact that his experience has been wider and deeper than, from a careful study of this manual, we are inclined to believe has been that of its author. In fact, this book is in many respects just the kind we should expect a general practitioner to write on a special subject. It is in no sense original; it nowhere gives evidence of any special aptitude towards the treatment of the diseases of women; it is more a compilation from the writings of men universally known as specialists than anything else.

A detailed review of this manual appears unnecessary. It deals more particularly with what may be termed minor gynecology. The major operations are simply mentioned, the reader being referred to more elaborate works for their description. Dr. Jones has evidently studied to advantage the writings of Thomas, Goodell, Hart and Barbour, and others, and therefore the description of diseased conditions and advice as to treatment are in general good. In some respects, however, both the general practitioner and the student, for whom the book is intended, will find him a faulty and unsafe guide. Let them beware, for instance, of copying Sims' position, "drawn from life," as it appears on Fig. 188 (page 32). We venture the assertion that, with the woman lying as represented, it is, if not impossible to introduce the finger into the vagina, practically impossible to pass the speculum; and, as for Fig. 158, page 232, where the nurse is holding the speculum, and the operator is on the point of making an application, it rather exemplifies in a marked manner the way in which the nurse should not, or rather cannot, with effect hold the instrument. The woodcuts, as a rule, are atrocious. The author's directions (p. 176 and 177) for the introduction of a retroversion pessary are lacking in one very essential point, and the manner he recommends is certainly not the simplest. He omits (in this place) to insist on reposition of the uterus before the introduction of the pessary—a point which, if neglected, will nullify any action for good of the instrument; and, for our part, we cannot consider the dorsal position as favorable as the semi-prone for placing the pes-

sary in good position and with the infliction of the least pain. The general views on the subject of pessaries are very sound. A protest is entered against their indiscriminate use, and yet their value, when really indicated and properly adjusted, is recognized. The author proceeds, however, to figure, even though he cannot indorse, a variety of forms the theoretical and practical utility of which is very questionable. Particularly is this the case when speaking of the treatment of prolapsus. Why describe those balls and rings; why give space to Zwanck's pessary, for instance? Dr. Jones obviously recognizes the disadvantage, common to them all, of giving temporary relief at the expense of an eventually worse condition, and so it would have been far better had he been independent enough to omit them from his book altogether. We would also criticise the author's praise of the sound as the best and safest repositor after the finger. We think that teaching better which tells the novice never to replace the uterus with the sound, but if the fingers fail, to use a repositor. The latter instrument cannot, from its construction, injure or perforate the fundus; the former may, unless used knowingly and with the greatest possible care. Finally, we would simply note the fact that on page 164 Thomas' intrauterine stem is credited to Peaslee, and we would earnestly request any reader about to attempt trachelorrhaphy not to do so until he has informed himself in regard to the details of the operation more fully than they are herein set forth; for thus alone will he be spared trouble during its performance, and insure for his patient an uncomplicated convalescence and a good result.

EGBERT H. GRANDIN.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF LONDON. Volume XXV. For the year 1883. London: Longmans, Green & Co. 1884.

We are glad to note that the contents of this volume are largely obstetrical. Those members of the Society whose leaning is rather towards matters gynecological, have, contrary to the tendency of times, kept more in the background and allowed the sister science ampler latitude. This is as it should be; for, in the eagerness with which, nowadays, the diseases of women are studied, there is danger lest obstetrics receive not its due share of attention. And yet, how large a number of gynecic disorders are due to neglect or ignorance of often the simplest obstetric axioms!

The most elaborate paper in this volume is DR. CHAMPNEYS' contribution entitled "The Pressure of the Femora and its Influence on the Shape of the Pelvis." In this paper the author lays more stress on muscular action as a cause of the shape of the pelvis than do most authorities. This gentleman also contributes a long paper devoted to the "Obstetrics of the Kyphotic Pelvis," consisting of a careful study of this pelvis founded, in particular, on a personal case.

DR. BRAXTON HICKS contributes a paper on "The Behavior of the Uterus in Puerperal Eclampsia," as observed in two cases—a subject which has not been carefully studied. He finds that during a convulsion the uterus becomes firmly contracted and remains so for from ten to fifteen minutes and then relaxes, the obvious corollary as to treatment being speedy delivery in order to save the fetus.

DR. G. ERNEST HERMAN reports a very interesting case of "Acute Gangrene of the Vulva," where no cause could be discovered.

MR. LAWSON TAIT's paper on "Uterine Myoma" is practical and contains a digest of his experience with removal of the uterine appendages as a means of stopping the growth of these tumors. He has operated after this fashion fifty-four times since 1878, with a mortality of 5.5%, and he knows that in thirty-eight of his cases an absolute cure has been effected. He, therefore, naturally favors this operation rather than hysterectomy; and he concludes "we may take it, therefore, that, like every other operation, removal of the uterine appendages for myoma will meet with a certain proportion of failures, but it is no more to be condemned on this account than is excision of the knee because many such cases have ultimately to submit to amputation."

DR. J. HENRY BENNET's paper on "The Os Uteri Internum, Its Anatomy, Physiology, and Pathology," is simply a repetition of his views as published in 1849. These views, he thinks, are being lost sight of, and yet are very important, and so he emphasizes them anew. It is not our intention here to give these views in abstract. They are well known to the profession on this side of the water, as are also many of Dr. Bennet's *ultra* statements. It is pertinent to ask, however, will the scales never drop from this distinguished gentleman's eyes? When will he cease to kick against facts? When will he spare us such statements as the following on page 230, where, in speaking of waves of opinion in gynecology, he refers to the *unjustifiable* practice in America of sewing up the lacerated cervix uteri "for insignificant lesion easily cured by the simplest local treatment." Dr. Bennet is perfectly right in protesting against extremes of practice in any one direction, but he fails to see, what must be evident to all others, that he himself is going to extremes and, in this instance, very far.

The last elaborate paper is by DR. ROBERT BARNES and consists of a study of "The Mechanism of Labor," more especially with reference to Naegele's obliquity and the influence of the lumbosacral curve. It is a paper which must be read in order to be appreciated, and we can make no attempt to give the gist of it here.

Scattered throughout the volume the reader will find descriptions of interesting specimens as well as short papers which our space will not allow us to mention. Sufficient the statement that the volume will well repay perusal.

EGBERT H. GRANDIN.

ITEM.

THIS number has been delayed to admit the Proceedings of the American Gynecological Society at Chicago, on September 30th, and October 1st and 2d.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

ALCOHOLISM IN CHILDHOOD AND YOUTH.

BY

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MANY of the consequences of the daily increasing intemperance of all classes and of both sexes are as well recognized in modern medical practice as others are on temperance platforms, or in the proceedings of our criminal tribunals. Largely, however, as it has been discussed from all these different points of view, this many-sided topic still unfortunately remains unexhausted; and hence I venture to submit a few remarks on some recent developments of alcoholism which have lately come into frequent observation in the children's hospital. The evils, moral and physical, resulting from the abuse of alcohol were never so prevalent as at the present time, and the latter have become almost as traceable in the diseases of youth as in those of adult existence. Amongst the results of the killing pace at which the race of life is now so generally run from its start to its finish, one of the most serious is the fact that the period of childhood has, in many instances, become so abridged by the necessity of entering on the struggle for existence before the moral, mental, or physical powers are sufficiently developed that a premature breakdown in any of these is no longer exceptional. This is exemplified in one of its phases by the painful exhibitions of juvenile alcoholism now daily witnessed, more especially amongst those neglected little street arabs who are forced out into the thoroughfares of every great city,

there to eke out a living as best they may, and the pathological consequences of whose inherited or acquired alcoholic proclivities are brought under almost continual observation in our hospitals.

The history of some cases of this kind, one of which I have elsewhere referred to, will probably best illustrate the subject of the present communication.

J. L., aged eight, was admitted into St. Joseph's Children's Hospital February 8th, suffering from delirium tremens. His mother was an habitual inebriate, and during gestation had been repeatedly drunk. His father, a sober man occupying a respectable position, as soon as possible sent this child to be reared in the country, away from her example. Returning home when nearly six years of age, the propensity for alcohol soon, however, manifested itself; for then discovering a bottle of whiskey secreted by his mother, he helped himself and became stupidly drunk. From this time, despite his father's efforts to prevent it, he tipped almost continually for nearly two years. A few days before being brought to the hospital, he abstracted a bottle of port wine from their lodging-house keeper, and this he finished all to a glass or so at one sitting. After this last bout he fell into a drunken coma in which he nearly died. On being aroused therefrom by active treatment, he was found delirious, and after a short time was brought to the hospital.

His condition was then that of well-marked delirium *e potu* with great vital prostration. On being spoken to, however gently, he shook like an aspen leaf from head to foot, and tried to hide himself under the bed-cloths. At the same time he understood perfectly whatever was said to him, and manifested an absurd over-anxiety to comply minutely with every direction. He glanced furtively around the ward, complained of imaginary annoyances, and exhibited an abject terror of the other children in the ward, clinging nervously to the sister in charge, to prevent her leaving him alone with them for a moment. His eyes were glistening and staring, pupils dilated, skin cool and clammy, tongue moist and coated, abdomen hard, liver tumefied, temperature sub-normal, pulse 140, weak and somewhat irregular; general aspect emaciated and cachectic. He was ordered beef-tea with a little capsicum, in small quantities and at short intervals, milk and soda water to relieve thirst, and a mixture with tincture of digitalis, bromide of potassium, and aromatic spirits of ammonia, in suitable doses. On the following day he was slightly better, having had some sleep during the night. For the next few days he continued to improve, all the symptoms slowly subsiding, and at the end of a week the delirium and tremor had nearly disappeared, although he still continued extremely excitable, restless, and debilitated.

On the 15th, a crop of boils showed themselves first on the face

and neck, and thence extended gradually over the whole body. These furunculi did not entirely disappear for nearly a month. During this time he remained as weak in mental as in bodily health. At last, however, under the influence of various tonics such as Fellows' syrup of the hypophosphites which we prescribe largely in the Children's Hospital in small doses, Parrish's syrup, and all the various other remedies of the same class which the ingenuity of my colleagues could suggest, and which were successively administered, together with a very generous dietary, daily baths, and above all, free exposure to the fresh air and sun-light in the grounds of the institution, he began to recover again. And it was then observed that *pari passu* with the regainment of his physical strength his mental condition also obviously improved so that by the middle of April he was discharged convalescent from the hospital. I may add that he was shortly afterwards brought before one of our Divisional Magistrates, by whom he was sent to a Reformatory School where he is now going on satisfactorily, and with every prospect of turning out well.

CASE II.—In an adjoining bed to the patient just referred to, we had at the same time in the hospital another juvenile victim of alcoholism. M. N., a newsboy, aged eight, was admitted December 29th, suffering from alcoholic blood-poisoning, marasmus, and eczema. Before admission this boy had been in the habit of going out to the public-house for his drunken mother's bi or ter daily supplies of whiskey, and on each occasion was rewarded with a sip of the raw spirit until its taste and use had become second nature to the unfortunate child, who then invested his own little earnings in the same way. I need not enter into any detailed account of this case and shall only add that after nearly three months' treatment he left the hospital in perfect physical health and apparently quite free from drink craving.

CASE III.—A more pitiable instance of the consequences of alcoholism is at the time I am now writing still in another of my beds in the same institution, in the case of a very attractive little girl, five and a half years old, who was brought in suffering from acute traumatic meningitis, the result of a savage blow on the head inflicted some time previously by her drunken mother. This was eventually one of the few recoveries in such an advanced stage of that most fatal of all the diseases of childhood, acute meningitis, that we have met with in the course of a long and sad experience. During her ravings in the course of the disease, and even during her convalescence, this poor child babbled repeatedly and craved earnestly in her own words for "a drink of porter from the gallon." We subsequently ascertained that from the time she could totter under its weight she was sent out to the public house every day by her mother for a can of porter, out of which she was accustomed to help herself to a little on each occasion.

CASE IV.—We have now another child in the hospital suffering from injuries consequent on her parents' alcoholism. In this case the little victim is a girl about three and a half years of age, whose

right leg has been fractured by a furious kick from her intoxicated mother.

CASE V.—In the same ward is also a wretched infant similarly injured by a kick over the hip-joint from the drunken brute who claims to be its father.

Cases such as those just referred to, which are but a few of the many instances of the same kind which have come under my care in the Children's Hospital, within the past few years, are, I think, proof that alcoholism demands greater consideration than it has received as a prolific cause of disease and death in childhood.

In the majority of cases of juvenile alcoholism that have thus come under my notice, this tendency appeared to have been inherited, and was most marked in those children whose mothers were confirmed inebriates. Some reference is therefore necessary in this connection to intemperance in women as bearing in many ways on the diseases treated in hospitals for children where its effects are strikingly evinced by the moral and physical deterioration of the offspring of the drunken, and by their special tendencies to strumous, tubercular, and other constitutional taints.

In many of these instances of alcoholism in women, the toxicological effects of which are transmitted to their children in the manner just alluded to, the craving for stimulants is traceable to some of the disorders which come before us in gynecological practice. Oftentimes it results from the too general habit of administering alcoholic stimulants for the relief of dysmenorrhea. In such cases, "this unkind nepenthe" is frequently employed in gradually increasing doses to produce the same effect, until eventually, and often unconsciously, the victims of dysmenorrheal alcoholism may become an habitual and incurable drunkard, and possibly the future mother of children cursed by the inheritance of the same awful proclivity.

In this matter our profession is directly concerned, and can do much by its influence to lessen this source of hereditary alcoholism. Even yet, however, we have hardly sufficiently emancipated ourselves from the influence of those eminent authorities who, some twenty years ago, in leading the reaction against the antiphlogistic treatment then in vogue, went to a very undue extreme in the opposite direction. Under any cir-

cumstances the use of alcoholic liquors should be resorted to, as other narcotic stimulants are, only when therapeutically required, and in doses regulated by the special exigencies of each case. Had we, therefore, to deal with the hale and happy inhabitants of Dr. Richardson's ideal city of Hygiea, we might with great advantage adopt his views as to the total prohibition of all intoxicants. Unfortunately, however, we live on earth and not in Utopia, and in our profession have to consider the condition of those enfeebled by disease or age, and too often those who have been prematurely broken down, mentally as well as physically, in sustaining the battle of life with sinking heart as well as failing thought. Hence, with Solomon, would I still prescribe "Wine to him who is ready to perish and to him that hath grief of heart," though not to the healthy and never to the young.

In considering the causes of the increasing tendency to alcoholism now noticeable amongst the young, it must be borne in view that, as already mentioned, this proclivity is, in the majority of cases, inherited, and results from the prevailing intemperance of women as well as of men in all classes. For although persons of good position no longer drink as much wine after dinner as formerly, still even amongst those of the highest ranks, as well as in the middle and lower classes of society, a vile habit of tipping, or "nipping," ardent spirits or liquors in small quantities throughout the day has become generally prevalent; and this is productive of far more injurious consequences than followed the post-prandial excesses of our forefathers. Nor can it be wondered at that the children of such a generation should even in their earliest years manifest the proclivities and diseases of alcoholism.

The evils thus arising from intemperance in young and old should at least suffice to warn us against allowing children to become habituated to the use or taste of any intoxicant. The custom of giving wine to the children of the better classes or stronger stimulants even to infants among the poor, as a *bonne bouche*, is so universal in its practice and so calamitous in its consequences that it is full time that the voice of the profession should make itself heard in no uncertain tone in reprobation of this source of incalculable ills. Many years ago, in a work which has since passed through several editions and has been

republished in America as well as in England, I discussed this subject, and showed that "under no circumstances should stimulants be given to children, except when required therapeutically, and then only in the form and definite doses of other medicines." Since then lengthened experience in hospital and private practice at home and abroad has amply confirmed my opinion that it is physiologically wrong and morally unjustifiable to allow a healthy child to taste alcohol in any form.

I have no wish here to enter on the wide question on which so much can be said on both sides, as to whether the total prohibition of the sale and use of all intoxicants is or is not expedient. But though I am somewhat dubious as to the possibility of stamping out the prevailing intemperance, some of the deplorable effects of which have been referred to, by mere legislative measures, I am certain that something might at least be done in this way to lessen the growing evils of alcoholism amongst the young. With this view the existing licensing laws should be more rigidly enforced and amended so as to effectually prevent the sale of all alcoholic liquors, not merely to children, but also to those within the period of adolescence.

NOTE ON HYPODERMIC INJECTION OF MORPHIA IN CONVULSIONS OF CHILDREN.

BY

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A RECENT article in the AMERICAN JOURNAL OF OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN (April, 1884) on "Morphine by Subcutaneous Injection in Infantile Convulsions," by Dr. Clark, of Oswego, N. Y., recalls to my mind an interesting case occurring in my practice two years ago.

One Sunday afternoon in April, 1882, there came to my office a negro man with a little boy two years of age. He had walked in three miles from the country bringing the child in his arms. The little patient had been sick for two days; had a high fever

when he left home; convulsions came on while on the road, and had now lasted about an hour. I took him into my office, administered by the mouth valerian, applied cold cloths to his head, sinapisms to the back of neck and the extremities. The convulsions were unchecked, and soon became almost continuous, there being very little intermission between them. In a short time the child was totally unable to swallow, and appeared to be rapidly sinking into a dying condition. Under these circumstances, I felt justified in risking morphine by hypodermic injection as a last resort. Accordingly, I injected about one-fifteenth of a grain of morph. sulph. into his arm. In ten minutes he was quiet, and in twenty minutes was sleeping nicely. I kept him at my office awhile longer, and as he seemed to be doing well, directed him to be taken to a house near by where he could be kept until morning, giving such directions as were necessary for the treatment of the case, which was malarial fever. He had no return of the convulsions and made a good recovery. I have not used this treatment in any other case, though sometimes strongly tempted to do so when hot foot-baths, cold applications to the head, sinapisms properly applied, anti-spasmodics internally, and chloroform by inhalation seemed about to fail. I have determined, however, in the future to resort again to this treatment in severe cases.

This may be bold treatment, but these three cases are sufficient to embolden us to try the remedy in others, and see if it will prove to be as far superior to the present accepted treatments of convulsions in children, as the hypodermic use of morphia in large doses in puerperal eclampsia is believed by many to surpass the ordinary treatment by venesection, chloral, etc.

TRANSLATIONS.

THE INHERITANCE AND TRANSMISSION OF SYPHILIS.

BY
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of Vienna.

Translated from *Jahrbch. f. Kindhlkde.*, xxi. B., 1 u. 2 H.
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(Continued from p. 1006.)

HUTCHINSON's ideas are a little less sharply restricted. He considers all three kinds of retro-infection possible, and after describing the first two, he adds: "In a third group, comprising at least half of all cases, the disease is characterized by no outward symptom and no disturbance of the general health. The absence

of all manifestations during the first years does not exclude the possibility of tertiary symptoms in a more or less distant future, but these generally do not follow, and a woman infected in this way generally remains for her lifetime free from every syphilitic manifestation. Thus, *this syphilis* may escape us and, *in spite of its existence*, baffle all our search for it, even when it has taken deep enough hold of the mother's organism to prevent a further new infection."

Zeissel seems to hold a similar opinion, though he does not formulate it so exactly. He says: "We hold the opinion that in most cases in which women bear a syphilitic fetus in utero they become infected from it. This infection either manifests itself in distinct symptoms of syphilis, or the disease remains *latent*." The case mentioned above is offered as proof of this opinion, and, in addition, the circumstance is stated that only two cases are known of subsequent infection of such a mother from her syphilitic child, and finally Caspary's negative attempt at inoculation is mentioned, which, as Zeissel says, proves that in such a mother inoculation with syphilitic poison is *sometimes* impossible. From these premises, the surprising conclusion—not altogether consistent with the statement that the mothers are infected from the fetus only in the *majority* of cases—is drawn that a healthy woman who bears a syphilitic fetus in utero, *almost without exception*, becomes syphilitic. In another place, he uses a still stronger expression: "I never saw a woman who had borne a syphilitic child who did not have at least latent syphilis."

We have noticed already the case of J. Neumann. A woman bore two syphilitic children, the second of which infected the grandmother from sores on its mouth. The mother nursed this child for eight months, and yet the most careful examination, extending through a half-year's observation at the clinic, failed to discover any signs of syphilis. This failure is the more complete because Neumann is a firm believer in the presence of syphilis in such a case. In order to make this latent syphilis show itself, he used the cauterisatio provocatoria of Tarnowsky, this author having stated that in syphilitics an artificial inflammation of the skin will give rise to a syphilitic infiltration. The result of this was entirely negative. There was no induration, and the health of the woman was not affected. Neumann then tried inoculation. The woman was inoculated eight different times, each time at two points, with pus from chancres or ulcerated papules, and every time with an absolutely negative result, while an inoculation with the secretion of a soft chancre produced a characteristic sore and adenitis. From these facts, Neumann draws the conclusion that, even when we find the mother of a syphilitic child free from syphilitic symptoms, we are not justified in concluding that she is healthy.

All these four authors, therefore, starting from the premise (which, as we shall see, is almost indisputable) that women who have borne syphilitic children are much less receptive of the

syphilitic poison than others who have not been placed in so peculiar a connexion with a syphilitic organism, have drawn the, in our opinion, too far-reaching conclusion that such women are "at least latently syphilitic," or "not healthy," that is, not free from syphilis.

Now we must first define exactly what is meant by latent syphilis, and when we are justified in declaring an individual latently syphilitic, we know that syphilis manifests itself in a primary lesion and in isolated, separated outbreaks of various symptoms, and that longer or shorter periods occur, both before the appearance of the initial sclerosis and between the outbreaks of the other symptoms, in which clinical examination discovers no distinct symptoms of the disease. We know then decidedly that in these intervals the disease or the disease-making agent is present in the body in some form or other, but the disease is undiscoverable by our means of examination—it is latent.

Now when can we declare that an individual at a given moment has latent syphilis? This will only be possible in rare cases, for when an individual has previously had decided symptoms of the disease, and at a later date is free from every specific manifestation, we cannot then tell whether another outbreak of the disease will follow or not. If not, we have to do, not with a latent, but with a cured syphilis. We, therefore, have much more frequent opportunity of saying that an individual *was* at a given time latently syphilitic, namely, when after that time he again showed manifestations of the disease, than we do of declaring an individual at a certain moment latently syphilitic. The latter is possible only under two conditions, namely, when we can either show that *the individual is in condition to transmit his syphilis by inheritance to his descendants, or when we succeed by inoculation with his blood or his secretions in making another healthy individual syphilitic*. Logically speaking, therefore, we are only then justified in declaring an individual syphilitic when he either offers indisputable manifestations of syphilis or when we can bring strict proof that the individual is capable of transmitting the syphilitic poison to a second individual in any way, that is, through inoculation or through inheritance.

It may be here objected that we have not stated all the proofs of the syphilitic infection of an individual, because we have passed over in silence another characteristic which also belongs to latent syphilis, namely, immunity against a new infection. But this negative characteristic *alone*, separated from all the above-characterized positive marks of the specific disease, can never justify us in making the diagnosis of syphilis in any special case. For an individual of whom we know nothing, except that the attempt to inoculate him with syphilis has failed, is possibly syphilitic, but possibly also not. It is, of course, right to say that syphilitic individuals possess immunity against syphilitic inoculation, but it would be an unjustifiable syllogism to invert the sentence, and to say that every one who possesses an immunity against

syphilitic poison is syphilitic. There are without doubt men who, without being syphilitic, are either temporarily, or, perhaps, continuously proof against syphilitic infection. The small number of scientific inoculations made up to the present time give us no correct idea of the frequency or rarity of such immunity, and we also have no decided knowledge as to whether infection is not frequently enough thus escaped during sexual intercourse, in spite of the extremely favorable circumstances. But of greater weight in this direction is the quite frequent observation, which I myself have often made, that healthy nurses are not infected by syphilitic children in spite of long-continued suckling, and even when specific ulcers are present upon the lips of the children. Günzburg has published a mass of material on this point from the Moscow Foundling Asylum. He reports that 31 nurses suckled altogether 120 hereditarily syphilitic children (39 of whom had lesions of the mouth) during 395½ months without any of them becoming infected. Günzburg has even drawn the conclusion from these observations that hereditary syphilis is not infectious. This declaration is, however, absolutely wrong, as we shall soon see; but another conclusion may be drawn from these observations, a conclusion of importance in the question now interesting us, namely, that at least the temporary immunity against syphilitic infection must be much more frequent than we are accustomed to suppose, and that we are therefore in no way justified in declaring an individual syphilitic on the single ground that he at a certain time proved unreceptive of syphilitic infection. Were, perhaps, those thirty-one nurses in the Moscow Foundling Asylum all latently syphilitic because they escaped infection from syphilitic infants? The rule, "whoever possesses immunity toward syphilitic infection is syphilitic," appears to us still more arbitrary when we examine the results of inoculations of animals with syphilitic poison. J. Neumann has reported very recently that his whole, very numerous, experiments in inoculating the syphilitic poison upon various animals have been complete failures. Here we have a large number of highly-developed organisms, possessing the capabilities of developing the most various seeds of infection in their organisms to the same characteristic manifestations of disease as we observe in man, and yet who showed absolutely no result from the inoculation of the syphilitic poison in the form in which it leads in man to a classified syphilitic disease. No induration developed at the point of inoculation, and no other symptoms followed. These organisms, therefore, responded to the syphilitic poison as do syphilitic men. Now what would any one say if we should declare that this ass, this dog, etc., is syphilitic because he possesses immunity against syphilitic infection? It would be, of course, nonsense. We can say nothing more than that the organism of this animal, its tissues, its blood, and its fluids, from some reason unknown to us, are incapable of developing the syphilitic poison, of increasing it, and reproducing it, as the organism of most men is able to do.

In animals, this incapability of reproducing the syphilitic poison is inherited—congenital—but we know that this peculiar condition of the tissues and fluids which renders an organism incapable of developing an infection may also be acquired. There are, for example, three ways in which immunity against variola may be acquired: first, by having passed through an attack of the disease; second, by the accidental or scientific inoculation of another, probably related, but certainly not identical disease product; and third, during the fetal period, by the mother's passing through an attack of variola or vaccinia.

At present, we know with certainty of only one way of acquiring immunity against syphilis, namely, by an already present infection. But this does not preclude the possibility that such an incapability of reproducing the syphilitic virus may be acquired in another way also, and we will now show that those women who, without being themselves infected, have carried and borne syphilitic children, possess in most cases immunity against the syphilitic poison without being themselves syphilitic, that is, without offering any trace or token which we must with the greatest logical strictness require for the establishment of the diagnosis of the syphilitic infection of an individual. The acceptance of such an immunity is justified by the observation that such women are not infected by their own syphilitic children either *intra* or *post partum*, in spite of the very frequent opportunities offered. These opportunities are:

(1) The process of birth, when the children come into the world with pemphigus bullæ or other eruptions. There are always abrasions or injuries of the outer and inner genitals during delivery, and the possibility of this form of infection must be admitted. But there is no case known in which infection in this method has been shown.

(2) A still more frequent opportunity is offered when mothers, entirely without symptoms of syphilis, suckle their syphilitic children. While, as we shall soon see, strange nurses are frequently infected from hereditarily syphilitic children, yet up to a recent date there has been no case known in which a mother was infected from her child. This fact has given rise to the theory that hereditary syphilitic products are not infections. It was especially Ricord and his supporters, who, since they denied the contagiousness of secondary symptoms and believed infection to be possible only from the primary lesion, were forced theoretically to deny also the contagiousness of hereditary syphilis, and they claimed that in the cases of supposed infection of healthy by hereditarily syphilitic children, there was a confusion with acquired syphilis. But after a large mass of observations had proved that this theory was false, and after the contagiousness of hereditary syphilis had become generally acknowledged, Günzberg (1872) again advanced the old theory, basing it on the fact that many healthy nurses had not been infected by hereditarily syphilitic children. But since these negative observations are opposed

by very many positive and indisputable ones, and even positive results from inoculation, the former prove nothing more than that the receptivity for syphilitic poison varies in healthy individuals. I have myself made positive observations in three cases, in which there could not be the slightest doubt as to the hereditary origin of the syphilis of the children, and yet healthy nurses were infected on the nipples and afterward had secondary symptoms. Moreover I have found in the literature a large number of cases, the details of which allow of no other conclusion than this. On account of the importance of the subject, I will here give a list of the authors who have reported cases in point from their own observation [list of twenty-nine authors, with their publications, omitted from translation, found on page 95 of *Jahrbch.*]. Many of these authors report two, three, or more such cases from their own observation. There is in addition quite a large number of well-proved cases of infection of healthy persons, from children with hereditary syphilis, in other ways than by suckling. Not least important of these is a positive result reported by Rinecker, obtained by inoculating a healthy man from the efflorescences of a hereditarily syphilitic child only forty-nine days old. Such a case alone is sufficient to put an end to the discussion of the contagiousness of the products of hereditary syphilis.

Now since the possibility of such infection is proved, and since it is an established fact that healthy nurses are frequently infected about the nipples from such children, it is certainly a significant fact that the mothers, whose exposure to this form of infection is incomparably more frequent than that of the nurses, should yet have been so rarely affected by it that, until very recently, not a single reliable case was to be found in the literature. Hereditary syphilis is rare in the classes by whom wet nurses are employed, compared to its great prevalence among the poorer classes, among whom the mothers frequently nurse their children for long periods. In the great majority of cases of hereditarily syphilitic children coming to my clinic, they were nursed by the mothers, yet I have never seen an infection of the latter, though the children frequently had lesions of lips or mouth.

(3) There are also other opportunities of infection of mothers post partum from their syphilitic children. Think only of the manifold conditions of family life, the care of the children, their washing, the artificial methods of nourishment, and particularly the fondling and kissing. Syphilitic infection of other persons from such children has in fact often occurred in these ways. I have twice seen older sisters (born before the father acquired his syphilis) infected by a younger, syphilitic child. In one case an adopted child in a family was infected in this way. In the first two the point of infection was the mouth, in the other the primary lesion could not be found, as is frequently the case in the acquired syphilis of children. In the literature I find five cases of infection of grandmothers, usually upon the mouth [references on p. 97 of *Jahrbch.*], one of infection of a nurse on the

finger, one on the arm, and many others. The mother is exposed to all these chances of infection incomparably more frequently than the other members of the family or strangers, since they are for months continually busied with the child, and cannot refrain from kisses and caresses even when the child has sores on the mouth. Yet no such case of infection of a mother is reported. It therefore appears *to me to be positively proved that these mothers possess at least a greatly directly decreased susceptibility to syphilitic infection.*

On the other hand, the number of observations of actually occurring post-partum infection of such mothers is exceedingly small and even these few are not indisputable. In my previous article I cited the cases of Cazenave (1847), Brizzio Cocchi (1858), and Müller (1861), and showed that no one of these reports was reliable enough to allow of any far-reaching conclusions. But since then there have been some cases reported, which, on account of their importance, I will sketch in short.

Guibout case.—A woman who was herself healthy bore a syphilitic child. While she was nursing it, four chancres appeared on the nipples, one of which was *indurated*, and recognized as such by Fournier. (Unfortunately there is no report of the date of this infection or of consequent general symptoms.)

Ranke's case.—A syphilitic man begot two syphilitic children. The mother remained healthy. The second child, two weeks after birth, had a macular eruption and ulcers in the mouth. During the suckling there appeared, in the location of a small fissure on one of the mother's nipples, an exquisite hard chancre, which was followed by a sharp eruption of roseola.

In the discussion which followed the report of this case, Busch (of Bonn) reported an analogous case of his own observation, but did not give the exact details.

Scarenzio's case.—A man, before marriage, had a genital ulcer and an indolent bubo, but no recognized general symptoms. A child was born which soon after birth showed symptoms of syphilis, and was nursed by the mother. In the seventh month, ulcerations appeared in the child's mouth, and the mother had an indurated ulcer near the nipple, swelling of the axillary glands, and later, papules, ulcers in throat, etc. The next child had a pustular eruption, and the woman had frequent relapses or fresh outbreaks.

Lueth's case.—(Published by Zeissel.) The man was infected and treated before marriage. The wife was healthy and bore a child, who, five months after birth, had fissures at the angles of the mouth, psoriasis palmaris and plantaris, a maculo-papular syphilide, and multiple glandular swellings. Four months later, the mother (who nursed the child), complained of soreness of the nipples, and a syphilitic induration developed in the areola. Afterward the glands of her neck swelled, and psoriasis palmaris and plantaris appeared, and papules in the mouth and throat.

In this connection the cases of Ranke, Scarenzio, and Lueth are worthy of careful consideration, for they are detailed in such a way that we can find no assailable point in them, and we may admit that they prove the *possibility of the infection of a mother from her own syphilitic child.*

They are of no great practical value, but of great weight theoretically. The cases are by far too exceptional to lead to

Casparry's conclusion, that "hereafter a syphilitic child may only be nursed by its mother when she is without any doubt syphilitic." Casparry here admits, not wholly in agreement with his already cited opinion, that only those mothers of syphilitic children are undoubtedly syphilitic who show distinct symptoms of the disease. But letting this point go, I cannot agree with this caustic rule, for I consider it an excessive caution, on the ground of such a notably extremely small chance of infection of the mother to expose a child to the risks of artificial nourishment, or a wet-nurse to the danger of syphilitic infection. Moreover, such a woman is constantly exposed to the risk of infection from her syphilitic husband. It is also always in our power to reduce the danger of infection from the child to a minimum by energetic mercurial treatment of the latter.

But the theoretical weight of these observations is much greater. In the first place, they show that the immunity which a woman acquires by bearing a syphilitic child is not absolute, but is analogous to immunity acquired in respect to other infectious diseases, which always shows exceptions, whether it is acquired by having passed through the disease itself (two or more attacks of scarlatina, variola, etc.), or artificially, by a preventive inoculation (vaccination). But the peculiar significance of these cases lies in the fact that they prove, even to the most obstinate disbeliever, the direct transmission of syphilis from the father to the child, without the intervention of disease of the mother. For if such mothers are infected with syphilis from their children post partum, it is certain that up to this time they were free from syphilis.

(To be continued.)

ABSTRACT.

1. Weigert: The Paths of Extension of the Tubercle Poison After its Entrance into the System (*Jahrb. f. Kindhld.*, XXI. B., 1 u. 2 H.).—In studying this question, we may a priori say that there are four possible paths in which Koch's bacillus, as well as any other poison, may travel through the body, having once gained entrance. These are: 1st, simple mechanical transmission, as by coughing, aspiration, or swallowing; 2d, extension per contiguitatem; 3d, by the lymph-vessels; 4th, by the blood-vessels. The numerous variations in the form of the tubercular process in the body will depend on the predominance of one or the combination of two or more of these methods of extension.

The transmission of the poison by mechanical ways is easily understood and has long been known. By coughing, the sputa containing the bacilli may be carried to the larynx, may be swallowed and carried to the intestines, or may be moved to other portions of the lungs. The movements of heart, lungs, and especially the intestines, are also important factors

in this method, bacilli from the outside of the intestine, opposite a tubercular spot, falling into different parts of the abdominal cavity, especially into Douglas' cul-de-sac.

The second path of extension is perhaps the most important—the extension *per contiguitatem*. This leads to an increase in size of already existing deposits. From this method arise the “solitary tubercles” of spleen, kidney, brain, prostate, etc., and caseous inflammations in the lungs, the uterus, or the lymphatic glands. In adults, this method of extension is usually very slow. In children, it is more rapid, and larger deposits are formed. Certain tissues offer more resistance to the poison than others, thus elastic membranes, the larger vessels, capsules of glands. Yet these all may finally yield. Of importance in this connection are the tough capsular tissues which form about a slowly advancing deposit of tubercle, and completely circumscribe it and prevent its further spread. This always points to a chronic process and is rarely seen in children, where the tissues do not have time to so organize. The poison may extend *per contiguitatem* to other organs, into blood-vessels, etc., thus opening up other paths of travel of great importance.

The third path of extension is by the lymphatic system. Here we notice differences between children and adults. In the latter, for instance, we frequently find the lymphatic vessels themselves greatly affected when starting from tubercular ulceration of the intestines, while the mesenteric glands are but slightly affected. In children, on the other hand, these glands are often swollen, and even caseous, while the lymph-vessels leading to them are not at all affected. This may be due to the fact that the poison is more easily absorbed by the child's membrane at the entrance part of the lymph-vessels and, therefore, does not remain long enough there to develop: or to the fact that the transmission through the vessels is easier, and the poison thus reaches the glands without any delay on the way. Anatomico-physiological conditions confirmatory of this theory should be sought. In the lungs of children, the extension of the poison is often *per contiguitatem* from bronchial or pulmonary glands to the lung tissue, so that we do not find pulmonary tuberculosis in children affecting the apices so regularly or so exclusively as in adults. When glands are tuberculous, the poison has generally been carried to them by the afferent vessels from the region of their origin; but this is not always the case. For example, we may find a group of glands, such as the axillary, retroperitoneal, or cervical which lie in the neighborhood of affected bronchial glands, but whose afferent vessels arise from perfectly healthy regions. From the affection of the bronchial glands, through the close capillary network of lymph-vessels, some of the afferent vessels of the other glands may be obliterated, and the course of the current changed, so that whole masses may become affected. The lymph glands seem to act toward the tubercle bacillus as toward other finely-divided material, as a sort of sieve, and the poison does not readily extend into the efferent vessels. Hence the slowness of development of this form of the disease. In some cases, the poison enters the blood through lymph-vessels which pass through no glands, though the frequency of this is lessened by the very general obliteration of these vessels by the surrounding process, especially in the peritoneum.

The fourth path is by the blood-vessels. Besides reaching these indirectly, through the lymph-vessels, the poison may gain entrance directly,

by disease of the walls of the vessels. In regard to "general tuberculosis," many errors, from olden time, are still retained, even in minds which fully accept the new theories. Not every case of local tubercle, nor every case of "pulmonary consumption," becomes generalized. The fever of phthisis has nothing to do with a general infection, as may easily be proven by autopsies. The *danger* in these cases is much more from the other three paths of extension. The *necessity* of blood infection from the slow, scrofulous gland affection is still less. It is also a mistake to suppose that the generalization of a tubercular affection depends on the severity of the process. In children, this has some force, but in this case there are two factors—the severity of the local action, and the lesser power of resistance in the walls of the blood-vessels.

It is necessary, in order that the poison should pass into the circulation, that it should enter a vessel which is still permeable. The poison always (with exception of methods already mentioned and in the portal system) enters through the walls of the vessels from an outside deposit per *contiguitatem*. Now, in man this process is so slow that small vessels are obliterated by the inflammatory non-tubercular tissue before the poison reaches them, while the walls of larger vessels offer more resistance, so that there is more time given for them to be obliterated in the same way. Such obliterated vessels may often be seen by the naked eye. *Sometimes* the poison reaches the vessel before it is obliterated. This is more apt to occur in small animals and in children than in adults.

When once the poison is in the blood, secondary deposits may be formed in the walls of the vessels, probably from the *vasa vasorum*. This is especially the case in the worst form of tubercular blood-poisoning—the acute general miliary tuberculosis, where a large number of organs are affected at the same time or in rapid succession. For this, it is necessary that a large amount of tubercular poison should enter the blood at once, and as this—as we have explained—rarely happens, this is a rare form of the disease. More often we find isolated deposits in certain organs, as the spleen or kidneys. Here only a small quantity of the poison has entered the blood. The *choice* of organs affected is inexplicable. There is another form of general infection in which many organs are affected, but the deposits are not in the form of miliary tubercles of like size and development, but of masses varying in size, age, and development, showing that there has been a succession of eruptions of the poison into the circulation, affecting different organs at different times, and the deposits then extending. This is more of a chronic general tuberculosis. The portal system has a peculiar relation to the circulation, as regards the absorption of tubercle poison. It is especially disposed to the entrance of the bacilli. We find hepatic tubercle nearly as often (ninety per cent) as we find intestinal tubercular ulcerations. The author even believes that the portal absorbents may take up the tubercular poison from the chyme itself, without the intestines being diseased. Another peculiarity is that the poison, when taken into the portal system, remains sequestered in the liver, unless, in very rare cases, deposits formed here enter per *continguitatem* into the hepatic veins and so again into the general circulation. The liver is to the portal system the same preservative sieve which the glands are to the lymph-vessels. It exhibits the same action in regard to cancer.

J. F., JR.

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ORIGINAL COMMUNICATIONS.

TWO UNUSUAL CASES OF LABOR.

BY

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A GREAT as well as wise English physician lately said that "what we need in medicine is less opinions and more facts." Acting upon this maxim, the present paper is restricted, as closely as may be, to narrating the two following cases.

CASE I.—*Acute pulmonary edema at labor, and death, after delivery, from heart-clot.*

The patient, an anxious-looking, worn-out lady of thirty-five, Vpara, was first seen by me at delivery. Of her previous history I learned this, that her preceding labors had been difficult and partly instrumental. The dystocia was probably due to uterine inertia rather than to any organic obstruction. She was said to have once had "rheumatic fever," but this lacks confirmation. The present gestation was more than usually tedious, the appetite capricious, and generally the condition by no means a "pleasing punishment." There had been no urinary analysis, though the extremities had been for some time edematous. But for the last two or three weeks, her most annoying trouble was a tedious hacking cough at times even dangerous in severity. Spurious labor began two or three days before March 7th—the actual time of delivery—for which opiates had been prescribed. True labor

began on the afternoon of the 6th, and through the night progressed but little. She had been given morphia gr. $\frac{1}{4}$ hypodermically at 8 P.M., and chloral hydrate gr. xx. in two doses, through the night. Her attendant, a gentleman of large and valuable experience, was compelled to leave town the next morning, and I was called in his absence, Saturday at 9 A.M.

Like so many of our cases of labor, my patient and I were utter strangers up to this time, and the history given above was learned partly before delivery, partly after death. I found, upon inquiry, that during the night pains were infrequent and feeble, but were becoming more severe, the pulse 96, temperature not taken. There was nothing unusual in her condition except a dry spasmodic cough, which was apparently checked in the paroxysm by sitting upright. The cervix was dilated about an inch, thin and tense, cephalic presentation. The membranes had spontaneously ruptured in the early morning. I anticipated no trouble other than a powerless labor, and therefore did not make an extended examination of the thoracic viscera. Dilatation, being tardy, was hastened by the finger. But as the labor progressed, the paroxysms of coughing increased in severity and frequency, and after 1 A.M. it was decided to hasten delivery in order, if possible, to check the suffocation and cyanosis. The effectiveness of the contractions seemed to be destroyed by the coughing and the subsequent exhaustion. At noon the head had engaged, but was progressing little if at all; the pains feeble, and cough incessant. ℥xxx. of ergot were given anticipatory to forceps. She had usually been etherized when delivered, and demanded it now, and in perhaps ten minutes after taking the ergot, she was allowed to inhale a small quantity of ether. One blade of Simpson's forceps had been applied, but before the other could be inserted, powerful contractions were aroused, and a feeble boy was readily expelled, weight about seven pounds. The placenta was easily squeezed out, and firm contraction maintained by compression. Against my expectation, emptying the uterus and relieving abdominal pressure did not stop the cough. It began again and continued until the woman fell back from the upright position, which she instinctively maintained, as was supposed moribund. Her condition was then most appalling—face pale, covered with cold sweat, lips, hands, and feet livid, eyes glassy and dazed, pulse very irregular, about 160, and respiration altered to convulsive gasps for air. In this state, there began to pour from the mouth and nose quantities of frothy water, partly saliva and partly exudate from the lungs. It was useless to wipe it away, and at last it was collected in a wash bowl, which in an hour was nearly filled by this pinkish bronchorrhea. Hypodermatics of brandy were freely used, and warmth applied over the heart and to the extremities. The cough and flow of serum gradually lessened as circulation was re-established, but for two hours life was maintained only by the frequent subcutaneous use of the brandy. At five o'clock ammonia carbonate and tr. digitalis were added to the brandy and given by mouth. There was a decided relief from

the imminence of suffocation. Respirations averaged 40, the pulse was stronger and at 140. In the upper lobes could be heard faint vesicular murmur, but all the rest of the lung was drowned in the exudate. At 7, 8, and 9, gain was continuous, though slight. At 11 P.M. the attendants found the patient dead; relief was sudden and painless, and probably was due to cardiac paralysis from overdistention of the right heart.

The religion of the patient prevented an autopsy.

The case offers abundant chances for theorizing as to the etiology of pulmonary edema and cardiac failure in the parturient, the correctness of the management of this case, the propriety of venesection, medication, more immediate delivery, etc. but I stick to my text, giving now the facts and leaving the opinions to others.

Among the interesting clinical points of the case are: 1, its rarity. I can find no notice of the subject in any English obstetrical treatise, though there are scattered through home and foreign journals a very few reports of similar cases. Dr. Lusk (New York) writes me: "I have been unable to find a case exactly similar to yours." Dr. Parvin (Philadelphia) writes: "Your case is a very interesting one, a rare mode of death in the puerperal woman, but still it has been previously observed." Dr. Chadwick (Boston) writes: "I never had such a case as you describe, and cannot give you any reference to the literature." 2. The pinkish frothy exudate, which must have measured several pints. This peculiar bronchorrhea would seem to be a diagnostic element in differentiating pulmonary embolus from pulmonary edema (Ziemssen's *Cyclopedia*, Vol. V., article "Hyperemia of the Lung" and two cases reported below). 3. The uterine hemorrhage did not relieve the lung engorgement.

As said previously, I can give little reference to the bibliography of the subject. The best essay at my command is in Ziemssen's *Cyclopedia*, Vol. V., p. 265 *et seq.*, though no special notice is there taken of the lesion in pregnancy. Drs. Lusk and Parvin both refer me to Hervieux, "*Maladies Puerpérales*," p. 1,129, and to Charpentier's "*Traité des Accouchements*," Vol. II., q. v. Dr. R. P. Harris (Philadelphia), than whom there is no more eminent authority upon obstetrical literature, writes: "Pulmonary lesions associated with

cardiac trouble have often been reported, but are not easy to find in journals from their titles."

He kindly refers me to the following cases:—"Ferdinando Franzolini, Undini, Italy, April 11th, 1878. Woman almost moribund from bronchial catarrh, anasarca, pregnant with twins. Porro's operation. Woman died in thirty-six hours, infants also in less than an hour (*Annali Universali di Medicina*, Milan, 1878). 2. A. Martin, Berlin, March 24th, 1883. Woman suffering from alarming orthopnea due to endocarditis valvularis and pulmonary catarrh. Porro's operation; woman recovered from operation and child lived (*Centralblatt für Gynäkol.*, No. 36, 1883). Dr. T. G. Thomas made his operation of laparo-elytrotomy upon the first living woman for pneumonia. "Patient almost entirely pulseless, was cyanosed, breathing with loud laryngeal rattle and almost entirely unconscious." Child premature, and mother and infant died in about one and a half hours (*AM. JOURN. OBST.*, April, 1878).

Dr. Curtin, reported to Pa. Obst. Soc., August, 1878. Pulmonary congestion before delivery, coughing and expectorating large quantities of pinkish froth, head in hollow of sacrum, cyanosis, immediate delivery by forceps. Stimulants, turpentine stupes, tr. digitalis, improved after delivery and was safe in twelve hours post partum (*AM. JOURN. OBST.*, Vol. XII.).

CASE II.—*Labor at seventh month complicated by fibroids, eclampsia, sloughing of fibroid, septicemia, and death on ninth day.*

The lady was more than forty, had been married seventeen years, but was never previously pregnant. For years there had been pain and soreness in the right groin, dysmenorrhea, and, during the latter part of 1883, menorrhagia. Fibroids were recognized in December last, and possible pregnancy. For the latter month or two, there had been edema of the face and extremities, as well as dysuria. April 1st, she complained of severe headache and partial blindness. Three days after, she had a true uremic convulsion, followed by six others up to 2.30 P.M., when I first saw her. She was then unconscious, face swollen, tongue badly bitten, pupils contracted, pulse 110 and full. The cervix was an inch long, readily admitted the finger, and the child's head was felt. The vagina was small and barely admitted two fingers. External inspection showed that the abdomen pointed towards the right centre. Palpation showed that there was a hard body, resembling in feel a fetal head, in the right part of the tumor, while in its left portion the body and legs of the child could be indistinctly felt. Repeated auscultation gave no signs of life nor placental souffle. At the left cornu was readily felt a subperitoneal fibroid, the size of a hen's egg. Two-thirds of a pint of urine was drawn with the catheter, and a specimen boiled solid in the test tube. Morphia gr. $\frac{1}{4}$ was given hypodermatically, and repeated every two hours up to 10 P.M. Before

my arrival, the convulsions had recurred every forty minutes; after the first hypodermatic, there was one in thirty minutes, a second in sixty minutes, a third in one hundred and fifteen minutes, a fourth in forty minutes, and the fifth and last in thirty minutes. After the fourth convulsion it was perceived that labor had begun. It seems apparent from the interval between the recurrences that the morphia retarded them. Later, as uterine action began, the fits were more frequent until the cervix began dilating, when they ceased. The order in which the different groups of muscles were affected seemed to be quite regular as the patient was watched for several hours. The initiative movements were quivering of the eyelids and face, then violent rolling of the head from side to side, then strong flexion of the arms and legs, with fixation of the diaphragm and other respiratory muscles, then arrest of breathing and irregular flutterings in place of normal heart-beats. The acme would be perhaps thirty seconds. Then would begin hurried gasps for breath, growing deeper and more regular; the heart's action more rhythmical, then stertor, and utter unconsciousness, with flaccidity of the voluntary muscles. When premonitions of an attack were noticed, chloroform was used, and evidently cut short the explosion. Up to seven o'clock, eight hours after the first convulsion, it was decided not to interfere with natural delivery, because the convulsions were further apart. But about that time the patient had two attacks nearer together, and it was discovered that labor had commenced. The lower uterine segment was thinning, the cervix was opened, and the os distinctly contracted upon the finger. Manual dilatation was thought to be the best means of relief, and first one, then two, and finally the united fingers, were pushed through the cervix. Progress was steady for an hour; the os was then open two inches, the membranes presenting, and pains regular. For the next two hours, there was no farther gain. Some obstacle to dilatation was apparent, but not demonstrable. Under chloroform, and after an hour's fatiguing work because of the smallness of the vagina, the short forceps were applied through an opening of possibly three inches. The head could not be drawn down until partially rotated, when the instruments were removed. For another hour there was slow progression. Finally, thirty minims of ergot were given subcutaneously, Simpson's forceps applied, and the child extracted. It was dead and livid, weighed about five pounds, and had one turn of the cord around its neck. The uterus was firmly compressed during extraction, and there was little hemorrhage. Immediate examination showed that the cervix was blocked by a hard tumor, by the side of which passed the cord up into the firmly contracted body of the uterus. The entire hand was passed above the obstruction up to the placenta, which was finally withdrawn somewhat torn, and, as was thought, with a part of it left still adherent. A second attempt at removal was made, but severe shock came on, the breathing stopped, and the heart was badly affected. All further interference was given up. The pulse at this time, 2 A.M., was 135, patient unconscious.

Hot towels were placed over the heart, and warmth to the feet. Reaction slowly followed, and she was left at seven in the morning, comfortable though profoundly unconscious.

Sunday, at 10 A.M., I found her furiously delirious, incessantly calling for water which, though freely given her, would not satisfy. She could not be kept in bed by other than absolute force, and at last was chloroformed. Sleep lasted about an hour, but on awakening she was again violent and was again chloroformed. She was kept narcotized by an ignorant "professional" nurse through the day until late in the evening, when she slept soundly and naturally. At 11 P.M., pulse 125. Albumen the same, the urine boiling solid; micturition frequent through the day, very little flow. Morphia, infusion of digitalis, and acetate of potassium had been given since morning, every four hours.

Monday, thirty-six hours post-partum, she was rational, the mind somewhat confused. Still very thirsty, temperature $100\frac{1}{2}^{\circ}$, pulse 120, a moderate chill in early morning, uterus well contracted, abdomen tender, albumen one-third by bulk, urination frequent in small amounts. Several pints of milk and a good deal of water had been drunk in the twenty-four hours. Severe headache. Ordered vaginal injections of hydrarg. bichl. (1 to 2,000) every four hours. Quinine ten gr. at once, and four gr. every four hours, alternating with the digitalis and potassium acetate.

Tuesday. Condition the same, no proper lochia, but the injections brought away much debris. The stomach was irritable, and morphia was omitted, as it was found that it had always nauseated when given before the present sickness. Milk and lime-water with same medicines ordered.

Wednesday. Same condition, $100\frac{1}{2}^{\circ}$, 120, no nausea, less abdominal tenderness, same debris, albumen one-third. Medicines same.

Thursday. Condition unchanged, $99\frac{1}{2}^{\circ}$, 120. An enema brought away a large feculent stool in the afternoon.

Friday. No change, but decidedly more purulent discharge from uterus.

Saturday. Worse, 101° , 135 in morning. Mild delirium. At 6 P.M. 103° , 140, no odor to lochia, but feculent vomiting, sight much affected, could not count fingers ten feet away. Stimulants and medicine given by rectum.

Sunday. Condition same, but patient more feeble, $101\frac{1}{2}^{\circ}$, 140, A.M., rectum would not tolerate enemas, and the tenesmus was quieted by laudanum and starch injection. Milk, brandy, and tr. digitalis given per os.

Monday. Ninth day, moribund, 103° , 165, mild delirium, albumen one-quarter, passed twice after the vaginal injection small quantity of bright blood, incontinence of urine.

Tuesday, death at 12 M. Coma with suppression of urine since last twenty-four hours.

Autopsy made same night. Body and face much emaciated.

The change in her appearance during the week was very startling. The abdomen only was opened.

Both kidneys enlarged, pyramids deeply congested and contracted, interstitial tissue much increased. Cortices pale. Capsule not adherent. Blood extravasations abundant at lower ends of kidney tissue in the pyramids.

Peritoneum somewhat congested, firmly adherent over the right fundus and to the iliac fascia. Three or four ounces of pus in the right periuterine sinuses. Right Fallopian tube highly congested, and enlarged to size of finger. Left tube and both ovaries normal.

Uterus, weight twenty ounces. At the right cornu is a subperitoneal fibroid with short broad pedicle, size of hen's egg, four small fibroids scattered about on the anterior surface of body, as large as filberts. The cavity is about four inches deep, with smooth walls, placental site at middle of fundus, marked by enlarged vessels but without placental tissue. In the right half of the corpus are two mural fibroids, one just below the entrance of the tube, and the other further down, about the size of a small hen's egg. A submucous fibroid projects into the cavity, three inches in diameter, with its mucous surface sloughed off. The entire left half of the body is occupied by a fibroid, four inches in diameter, its centre broken down into a cavity an inch large. The cervix is open and slightly lacerated.

The bladder was empty.

It will be noticed that in the report of the labor the large fibroid was felt on the right and the smaller subperitoneal fibroid on the left, while at the autopsy they were found transposed. A possible explanation may be this: The only dilatable part of the uterus was a narrow strip in its middle unoccupied by tumors. The back part of the womb expanded more than the front. To accommodate itself to the lower abdomen, it semi-rotated in the direction of the least resistance.

The case was almost hopeless at the outset, and is another argument in favor of a professional supervision of pregnant women from the earliest months of gestation. This attention the patient did not have, and perhaps owed her death to such neglect. The extent of the renal lesions, the disabled uterus, with only a fractional part useful for parturient duties, and the sloughing of the submucous fibroid, were factors in warranting the gravest prognosis. Points of clinical interest are the influence of morphia upon the eclampsia; the freedom from post-partum hemorrhage, a dreaded complication (Playfair's "Midwifery," p. 352) (the ergot before delivery and systematic compression afterwards probably prevented it, but it

was a sequel of three of the five cases referred to below); the septicemia, which was caused in great measure by the bruising of the submucous fibroid; that intrauterine injections were contraindicated, 1st, because the autopsy showed that there were no decomposing matters in the uterine cavity, which therefore needed no washing out, and 2d, because the hemorrhage forty-eight hours ante mortem and the autopsy showed that the vessels of the fibroid were already exposed and partially ruptured, and it would have been an impossibility to pass a tube high enough beyond the tumor to be efficient without further lacerating them; and finally, it is worthy of notice that from some cause the patient had been sterile seventeen years; that then conception took place when the menopause was at hand and the uterus nearly destroyed by fibroids; that pregnancy continued up to the seventh month, and then was ended, not by the uterine agencies, but by chronic nephritis.

The following cases have been found similar in many respects to my own.

DR. T. FOSTER, Portland, Me.: Patient forty, with ten years backache and pelvic irritation; spurious pains more or less for three weeks; abdomen hard, and tumor size of fist in right iliac region per vaginam; pelvis filled with hard mass, with small canal on left side. Convulsions began twelve hours after his attendance; vertex presentation; forceps impossible; version prevented by firm contraction around child's neck. The complications decided in favor of Cesarean section. The womb had a fibroid, size of goose egg, in right middle third of body, and several smaller ones about body and fundus. Child removed through incision in midline of womb, and lived some hours. Ten silk uterine sutures, ends left outside, and silver abdominal sutures. Patient's urine highly albuminous. Died from exhaustion sixty hours post partum. Autopsy. No union of wound of uterus or abdomen; bowels healthy; uterus had large number of lesser tumors scattered about in the body, and in cervix was a fibroid as large as head of seven or eight pound child; left kidney comparatively healthy; right much diseased from chronic interstitial nephritis. (*Trans. Me. Med. Soc.*, 1870; also Playfair's "Midwifery," note. p. 351.)

DR. MUNDÉ reports myoma at full term; forceps delivery; no hemorrhage; sloughing of fibroid; death from septic peritonitis and pyelitis. (*AM. JOUR. OBST.*, April, 1880.) See also discussion in *N. Y. Obst. Soc.*, May 6th, 1884. Enucleation of Fibroid vs. Cesarean Section. (*N. Y. Med. Journ.*, Sept. 13th, 1884.)

DR. KESSLER: Fibroid, with fourth month abortion; no hem-

orrhage; placenta partially retained, and could not be removed; septicemia and death. (AM. JOUR. OBST., July, 1880; N. Y. *Med. Jour.*, Nov., 1879.)

LUSK: Primipara, twenty-eight; tumor on left side, fetus on right; cephalic presentation; tumor high in pelvis; eclampsia; chloroform and Magendie \mathfrak{M} v. twice hypod.; oclusion of cervix, dilated after three hours by Barnes' bags; podalic version; severe post-partum hemorrhage; still-born child resuscitated; mother died from peritonitis and collapse. (AM. JOUR. OBST., 1876, p. 94, and p. 286.)

BAKER: Ipara, thirty-four; full term; waters drained off; pelvis filled with solid fibroid and subperitoneal fibroid also; cervix could not be reached; sixty hours after labor began, Cesarean section; uterus opened six inches; child living and saved; placenta easily peeled off, with severe post-partum hemorrhage; four uterine silk sutures; five silver abdominal sutures and silk at intervals; phlegmasia dolens on eighth day, and complete recovery on thirtieth day. (AM. JOUR. OBST., July, 1881.)

I desire in this connection to thank Drs. Harris, Lusk, Chadwick, and Parvin for their trouble in writing me, and collecting references.

ELECTRICITY AS A GALACTAGOGUE.

BY

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AT a stated meeting of the Obstetrical and Gynecological Society of Washington, held April the 18th, 1884, Dr. S. C. Busey, in his closing remarks on the subject under discussion, which was "The Management of the Puerperium," made this (to us) most remarkable statement, in speaking of measures to be adopted to increase the lacteal secretion: "He had tried electricity which had restored the secretion, but he had been forced to abandon it because it set up inflammation"! Now this statement coming from such an eminent man as we are bound to acknowledge Dr. Busey to be, struck us with wonder and surprise, as we had been in the habit of using it for this very purpose for a number of years, and are so using it now every day, and have never yet seen any sign or symptom of

inflammation, or any other bad symptoms following its use for this purpose. It is true that electricity is a stimulant, is it not also one of the most powerful sedatives of which we have any knowledge? Yes, it certainly is, and for this very reason we use it in all cases of acute inflammation of the mammary gland and threatened mammary abscess.

We use it in all cases of deficient lacteal secretion, from the lying-in month to the weaning of the child at the completion of dentition. When a child is badly nourished and suffering from any of the diseases incident to dentition, we rely more upon the administration of electricity to the mother to stimulate the mammary glands to secrete the natural and healthful nutritive elements for the child than we do on the administration of drugs to the child itself.

Whenever the pale and haggard mother presents herself to us with her child that is "teething," has diarrhea, and "worries and tugs" at her flabby breasts all night, getting no rest itself, and not allowing her to procure any, we at once put her on an abundance of good food, and use electricity as a galactagogue, and in a great many instances the benefit has been almost immediate, and the woman has many a time said to us on her visit the next day, "the baby nursed when we first went to bed, then we both went to sleep and did not wake up till morning."

Now the question arises, how are we to reconcile the difference between the experience of Dr. Busey in the use of this agent, and our own?

The difference *may* be in the currents used, or in the method of the application; and as Dr. Busey did not say in what form he had used it, whether galvanism, faradism, or franklinism, we are left in the dark as to that.

In using electricity as a galactagogue, we use central galvanization combined with general faradization, sometimes using both methods at the same séance, but generally alternating them.

IS CRANIOTOMY UPON THE LIVING FETUS A JUSTIFIABLE OPERATION?

BY

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THE February number, current year, of THE AMERICAN JOURNAL OF OBSTETRICS contains the first annual address of the President of the Washington Obstetrical and Gynecological Society, Dr. Samuel C. Busey.

The question, "Is Craniotomy upon the Living Fetus a Justifiable Operation?" is discussed, and decided in the negative in a manner at once interesting and unique.

It is our purpose, in this paper, not to enter into any detailed criticism of this valuable contribution to American obstetrical literature, but simply to call attention to a few facts which have escaped the erudite writer's attention.

Dr. Busey is chiefly induced "to relegate craniotomy to the class of desperate expedients of doubtful propriety under any circumstances," by the consideration of the mortality statistics of craniotomy as compared with those of eight procedures instituted in the interest of mother and child, namely, delivery by the forceps, version, induction of premature labor, symphysiotomy, Cesarean section, laparo-elytrotomy, utero-ovarian amputation, and total extirpation of the uterus.

Delivery by the forceps, version, and the induction of premature labor may be eliminated from the discussion as ineligible procedures when craniotomy is performed under the indications generally admitted by those in favor of the operation, and stated at a later period in this paper. Morisani's attempt to revive the obsolete operation of symphysiotomy is of so recent occurrence, the number of operations so small, and the mortality statistics so subject to question, that pubic section may be excluded from consideration. The classical method of Cesarean section may be rejected for the reason that its maternal mortality is $66\frac{2}{3}\%$.¹ Laparo-elytrotomy, the operation suggested by Joerg in 1800, is still a new operation, and relia-

¹ Carl Braun's *Lehrbuch der gesammten Gynäkologie*, 1881, p. 803.

ble statistical evidence, sufficient to determine its position, does not exist at the present time.

Total extirpation of the uterus has not received special advocacy from Dr. Busey, and we presume he would elect an operation of a less murderous character.

The discussion is thus limited to craniotomy and Porro's method of hysterotomy.

Dr. Busey's craniotomy statistics are derived from Churchill and Tyler Smith. These gentlemen are quoted as stating the maternal mortality of craniotomy to be in the proportion of one to five.

The craniotomy statistics of Churchill and Tyler Smith are rendered invalid by the facts; (1) they do not differentiate between the mortality of the operation *per se*, and that resulting from the condition which indicated the operation; (2) they are collected from a variety of sources, some of which are totally unreliable; (3) they are collected through a period of medical history, when craniotomy was practised upon all occasions, with imperfect instruments and insufficient operative skill. It must be remembered that in the time of "Dr. Labbat,¹ at the Rotunda Hospital, the forceps were never once applied in 21,867 labors," and that, when Clarke and Collins governed that institution, craniotomy was employed three or four times as often as the forceps. For these reasons, the craniotomy mortality statistics of Churchill and Tyler Smith have long since lost all influence, and are no longer quoted as modifying modern practice by the obstetrical authorities of the day.

The statistics of the obstetrical department of the Vienna General Hospital have been collected in protocols, through a long series of years, from a large number of cases, by competent observers. They are official documents, appearing under the authority of the ministry of education, and bear the royal and imperial seal of Austro-Hungary. No reasonable doubt as to their absolutely accurate character can be entertained.

C. Rokitansky, Jr.,² in 1871 published an account of fifty-two successful craniotomies—forty-seven on the presenting, five on the after-coming head—which occurred in the lying-in wards of Carl Braun.

¹ Playfair's Midwifery, p. 485.

² C. Braun's Lehrs. d. g. Gynäkologie, pp. 789, 790.

In the eight years 1871-1878, inclusive, eighty-two craniotomies—sixty-three on the presenting, nineteen on the after-following head—were performed in the same department. Fifty-nine mothers—seventy-two per cent—recovered, and twenty-three mothers—twenty-eight per cent—died. The causes of death were: *eclampsia*, one; *peritonitis post partum*, six; *physometra sub partu*, two; *ruptura uteri spontanea*, fourteen.

Of these twenty-three patients, six were in health, or, at least, exhibited no elevation of temperature before the operation. The mortality of the operation was accordingly seven per cent. The remaining seventeen were seriously disabled during labor, before the performance of craniotomy, by *eclampsia*, *tympanites uteri*, and *ruptura uteri*. Twenty-one per cent of the total mortality was entirely independent of the operation.

Dr. Adolph Merkel¹ has collected one hundred craniotomies, which were performed in Credé's obstetrical clinic at Leipsic, during the six years 1877-1882, inclusive. The total maternal mortality was eight per cent. If one fatal case of spontaneous rupture of the uterus, diagnosed before the operation, and one case of abdominal typhus, with lethal termination during the *puerperium*, be deducted, the maternal mortality due to the operation would be six per cent.

Spiegelberg² reports fifty-eight craniotomies which were performed in the Breslau clinic during the twelve years 1870-82, with a total loss of nine mothers—sixteen per cent. Of these nine fatal cases, however, three were infected with septic matter before the operation. The mortality of the operation is accordingly fixed at ten per cent.

Bidder,³ of St. Petersburg, has performed craniotomy thirty-two times during the period 1873-76, inclusive, with success in every case.

Wasseige, in his work,⁴ "Des Opérations Obstétricales," Paris, 1881, reports thirty-five cases of cephalotripsy which he

¹ Archiv für Gynäkologie (1883), Drittes Heft, p. 461.

² Spiegelberg: Lehrbuch der Geburtshilfe (1882), Bogen 46-50, p. 756.

³ C. Braun's Lehrbuch d. g. Gynäkologie (1882), p. 790.

⁴ The Medical News, Phila., March 29th, 1884, p. 363.

has performed with six deaths—seventeen per cent. Two deaths—six per cent—are attributed to the operation.

With statistical evidence of this character before us, we cannot agree with Dr. Busey's statement:—

“No one has or can hope to attain the success of saving a possible fifty per centum of the lives at stake.”

Now, let us turn to the mortality statistics of Porro's operation of hysterotomy. We presume that the statement:—

“In the Santa Caterina of Milan, and the Krankenhaus of Vienna (Harris), the Porro-Cesarean operation has saved seventy-three per cent of women and all the children”

is a typographical error, as it is false.

In Vienna,¹ the maternal mortality of Porro's operation, in Carl Braun's clinic, is forty per cent, while in the department of Spaeth and Gustav Braun it is fifty per cent. It is customary to estimate the fetal mortality at twenty-five to thirty per cent, a low estimate. The statistics of the Santa Caterina are *reported* as more favorable, but not to the degree asserted by the distinguished writer.

Dr. Godson,² in his review of one hundred and thirty-four cases of Porro's operation, states the maternal mortality to be fifty-five and ninety-seven hundredths per cent.

Charpentier³ has collected one hundred and five cases of Cesarean section, after Porro's method, and he finds, in whole numbers, the maternal mortality to be fifty-three per cent, while the fetal mortality is twenty-four per cent.

Harris⁴ says with reference to the present status of the Porro operation, there have been performed one hundred and twenty-two true Porro operations, with fifty-nine recoveries, $49\frac{4}{11}$ per cent. It is probable that we are dealing with another typographical error, as fifty-nine recoveries out of one hundred and twenty-two operations, gives a recovery percentage of 48.36, and a mortality percentage of 51.64.

As there is some difference of opinion among different authors as to the mortality figure of Porro's operation, we add

¹ C. Braun's *Lehrbuch d. g. Gynäkologie*, p. 810.

² *British Medical Journal*, 26th Jan., 1884.

³ Charpentier: *Traité Pratique des Accouchements*, tome II., p. 770 Paris, 1883.

⁴ *Amer. Journal of the Med. Sciences*, July, 1884, p. 31.

statistics from a variety of sources, in the following comparative table :

Craniotomy.		No. of Operations.	Maternal mortality, per centum.	Porro's Operation.		No. of Operations.	Maternal mortality, per centum.	Fetal Mortality, per Centum.
C. Rokitansky, Jr.	1871	52	0	Godson.....	1884	134	55.97	
C. Braun.....	1882	82	7	Charpentier ¹	1883	105	53.33	23.80
Adolph Merkel....	1883	100	6	Pinard.....	1880		45.4	
Bidder.....	1876	32	0	Petit ..	1882		55.10	
Wasseige ..	1881	35	6	Maygrier.....	1880		58.49	
Spiegelberg.....	1882	58	10	Simpson ..	1881		58.3	
				Zweifel.....	1881		59.4	
Total		359	5 57	Lucas-Championnière...	1882		67	
				Charpentier	1882	99	56.56	29.30
				Harris.....	1884	122	51.64	

Dr. Busey, on page 182, makes the statement :

"In justo-minor pelves, craniotomy is inadmissible."

Our admiration is challenged by this sentence. It requires a bold, if not a youthful or inexperienced man to indulge in the universal proposition under all circumstances ; it demands a man of exceptional erudition and unusual judgment to employ the universal negative proposition in the science of medicine. In support of this extraordinary assertion, Dr. Busey quotes the *opinions* of Professors Lusk and Isaac E. Taylor. An examination of the writings² upon this subject of these gentlemen will disclose the fact that craniotomy is not condemned in *all* justo-minor pelves, but simply in the higher grades of this deformity.

In his history of "One Hundred Cases of Craniotomy," Merkel³ reports thirty-seven operations, with two deaths, in cases of the generally contracted pelvis. These thirty-seven cases embrace the varieties of the generally contracted, the generally contracted and flat, and the kypho-scoliotic, generally contracted pelvis. In two cases of the generally contracted and flat rachitic pelvis, the *conjugata vera* measured six centimetres ; both mothers recovered.

¹ Charpentier : *Traité Pratique des Accouchements*, Tome II., p. 770.

Gynecological Transactions, vol. iv., p. 358.

AM. JOURN. OBSTET., Aug., 1883, p. 811.

³ *Archiv für Gynäkologie*, Bd. xxi., Hft. 3, p. 478.

Carl Braun, Josef Spaeth, Schroeder, Credé, Spiegelberg, Charpentier, and Barnes explicitly advise the performance of craniotomy in the lower grades of the generally contracted pelvis.

Dr. Busey makes, on page 182, the following remarkable assertion :

“There is not perhaps living to-day a single obstetric authority of accepted repute who will claim the practicability of craniotomy in cases where the conjugate is one and one-half inches or less. Indeed, but few hold it justifiable when the conjugate is two and one-half inches or less.”

This statement is made with especial reference to the simple, flat, rachitic or non-rachitic pelvis.

Pajot¹ has performed his operation of repeated cephalotripsy, without traction, eight times, with six recoveries in pelves whose true conjugates ranged from six centimetres (2.37 in.) to twenty-eight millimetres (1.07 inches). Charpentier places the limit at four centimetres (1.57 in.) for the great majority of operators; Tarnier, at five centimetres (1.97 in.); Depaul, Guéniot, Joulin, Delore, at four centimetres (1.57); Spiegelberg, Hyernaux, at four centimetres (1.57 in.); Barnes, at thirty-one millimetres (1.22), or even twenty-five millimetres (.98); Playfair, at thirty-eight millimetres (1.47).

It is possible we are giving too much attention to the discussion of a question which is largely of theoretical interest. The attempt to establish the proposition, “craniotomy upon the living fetus is unjustifiable,” is quixotic. It is an effort to force a conflict with an imaginary foe, as veritable as any of those of the “gaunt country gentleman of La Mancha.”

The *relative* indication for Cesarean section is a principle in the practice of obstetrics which has met with very limited actual recognition. In Vienna, this “conditioned” indication is fully admitted. The conditions and field of the indication are clearly sketched in the words of Carl Braun.²

“Cesarean section on the living woman for the preservation of the living fetus in pelvic deformity, in which the child, dead and diminished in volume, can be extracted through the pelvic canal and the health of the mother can with prob-

¹ Charpentier: *Traité Pratique des Accouchements*, Tome II., p. 750-1.

² C. Braun: *Lehrbuch der g. Gyn.*, p. 691.

ability be preserved by the perforation of the child's head, is not permissible under the following conditions :

a. When the parturient woman in full consciousness, and without any indirect coercion, declines Cesarean section.

b. When the parturient woman is rendered unconscious by disease (eclampsia, meningitis, apoplexy, etc.), by medicines (chloroform, ether), by poisons or intoxicating drinks.

c. When the child's life has been imperilled by uterine contractions, attempts at version, or the forceps, or when the child is deformed or not viable.

For a series of years not a single parturient woman in the Vienna Lying-in Hospital has determined to submit to Cesarean section, upon the ground of the relative indication."

If in a land, whose social conditions frequently render offspring highly desirable even among the unmarried peasantry no woman out of the fabulously large number chooses Cesarean section, the probability of its election by an American female is exceedingly slight.

We fail to perceive that "wide-spread detestation of craniotomy" to which Dr. Busey alludes in such feeling terms.

Schroeder says:

"It is extremely rare that the operation is performed on *this account* (relative indication), and mothers almost always prefer the perforation of the child, and justly so."

Spiegelberg² does not recognize even this limited application of the *relative* indication for Cesarean section. He says:

"Very justly Credé remarks that most parturient women are unable to decide, and become still more so as labor advances, and the humane confession of this teacher, when the husband of a parturient woman, after he had explained to him the happy sides of Cesarean section, asked him whether he, under circumstances similar to those in the present case, even in event of his own wife, would allow the operation to be performed:" "As physician had I persuaded, as man I was obliged to say No emphatically."

This confession condemns better than further words can do this "*conditioned*" indication. Charpentier⁴ quotes that famous and just remark of Cazeaux:

¹ Schroeder's Lehrbuch d. Geburtshülfe, p. 203.

² Spiegelberg's Lehrbuch d. Geburtshülfe, p. 775.

³ Neue Z. f. Geb., xxx., 1851, S. 346.

⁴ Charpentier: Traité Pratique des Accouchements, Tome II., p. 752.

"That which is certain in the Cesarean operation is that more than the half of the women are immediately sacrificed, and that which has been well proven by the experience of the centuries is that, supposing all the infants to be living at the moment of their birth, the half of them will not attain the age at which their mother succumbed."

Charpentier concludes:

"For us, then, the Cesarean operation ought to be an operation of *absolute necessity*, and we do not admit these relative indications, and as long as the instrument destined to mutilate the fetus, cephalotribe, forceps-scie, transforateur, etc., will be able to pass, it is to it that we will give the preference."

Playfair¹ says:

"In this country, where the life of the child is most properly considered of secondary importance to the safety of the mother, we cannot fix one limit for the operation when the child is living and another when it is dead. Nor, I think, can we admit the desire of the mother to run the risk rather than sacrifice the child as a justification of the operation, although this is laid down as an indication by Schroeder."

Lusk² is of the opinion that,

"If, in any case, the decision is left to the physician, he should regard the welfare of the mother as of paramount importance."

These are the "professional brothers" who, we are told by Dr. Busey, are

"dizzied by the emotional appeals of a false and sentimental humanity," and whose "judgment" is "made to swerve from a sound discretion and logical conclusion."

On pages 179-180 appears the following sentence:

"Just here I will be confronted with the statement of those who have performed one, two, three, four, or five craniotomies, without the death of the mother. The assertion of such a fact is probable proof that the operations were hasty and unnecessary."

This sentence may be taken as the type of sundry statements made in Dr. Busey's paper, which forcibly suggest a profound contempt for the "Science of the Necessary Laws of Thought." That material fallacy termed the "Irrelevant Conclusion," finds continual expression in this truly remarkable literary production. Logicians say that this fallacy is the especial re-

¹ Playfair's *System of Midwifery*, 1878, p. 502.

² Lusk's *Science and Art of Midwifery*, 1882, p. 400.

source of those who have to support a weak case. Our brethren of the legal profession are popularly supposed to be adepts in what is known as *argumentum ad hominem*. The story is told of an attorney for the defendant in a lawsuit who handed the barrister his brief marked, "no case, abuse the plaintiff's attorney."

In the manifold attempts to excite feeling and prevent the formation of a dispassionate judgment, we recognize the unconscious employment of another form of the "Irrelevant Conclusion," which receives the technical designation of *argumentum ad populum*. Of course, Dr. Busey's employment of this fallacy is an entirely unconscious one, as when used with deliberate purpose, it is the great weapon of rhetoricians and demagogues.

We are moved by Dr. Busey's vague allusion to the "craniotomy hook and perforator" to make the assertion that craniotomy—as practised in Austria, Germany, and the United Kingdom—is an operation whose *technique* is approximately perfect. We refer in particular to the Vienna operation.

The¹ *conditions* of this operation are: retardation of labor, insufficiency of uterine contractions to accommodate the head, adequate dilatation of the mouth of the uterus, escape of the *liquor amnii*, and the presence of a spatial disproportion, in which the child, in the concrete case, can be born diminished in volume, but not in an intact condition.

The operation is indicated: 1. In the presence of pelvic contraction, when the child is mature and undoubtedly dead, craniotomy is indisputably indicated (without previous trial of the forceps, or version) when, after protracted labor, the impossibility of the passage of the intact, and the possibility of the passage of the head diminished in volume, is demonstrable. 2. Craniotomy is conditionally indicated if the mother's health permits of no delay in artificial delivery, when podalic version is not practicable, even with aid of chloroform, without danger to the mother on account of tetanic contraction of the uterus, when the application of the forceps would involve the employment of such violence as would imperil the life of mother or child, and under such circumstances there is no hope of the child's life. 3. If it is doubtful whether or no the child is liv-

¹ C. Braun's Lehrbuch d. g. Gyn., p. 791.

ing, its rescue is recognized as impossible, or extremely improbable, and longer delay would expose the mother to greatest danger of life, during the labor, craniotomy is permitted by many authorities.

A sinking life, a diminution in fetal viability, is usually inferred when the fetal heart-tones are retarded to or under seventy, and the presence of a physometra, rendering the mother's condition precarious, is inferred from a skin temperature of 39–40° C. and tympanitic percussion resonance over the uterus.

4. In a case furnishing the *relative* indication for Cesarean section—that is, when the child is undoubtedly living and in health, when the parturient woman decidedly refuses her consent to that operation, a delay in delivery is inadmissible, delivery without destructive violence is not practicable, and the excerebrated head of the fetus can be withdrawn through the pelvic canal without probable danger to the mother (Nägele d. J., Hohl u. A.), craniotomy is indicated.

The operation of craniotomy is divided into three acts :

1st Act.—The trepannation.

Trepannation is effected by means of C. Braun's (1855) curved trepan, an important modification of the straight instrument of Kiwisch. When employed according to the directions of its inventor, this instrument is incapable of inflicting injury upon the mother, and presents obvious advantages over the various scissors-like perforators. This instrument, we are told by an obstetrical writer¹ of prominence, has been introduced in America "as a curiosity," but has not "been adopted."

2d Act.—The excerebration.

A long curved metallic tube is introduced through the perforation, and the cranial contents are thoroughly broken up. Water is then forced through the curved tube by means of a piston-syringe or irrigator, and the cranial contents washed out.

3d Act.—The cranioclasia.

The head of the child is compressed and extracted by C. Braun's (1862) cranioclast—an important modification of Simpson's instrument. Both as regards compression and extraction,

¹ Am. Ed. (1878) Playfair's Midwifery, Robert P. Harris, M.D., p. 486.

the instrument possesses advantages over all forms of the cephalotribe.

It is probable that Dr. Busey has endeavored to consider the question in its scientific aspect, and entirely free from the influence of ecclesiastical doctrine. He doubtless remembers, however, the famous doctrine of the Roman church,¹ authoritatively proclaimed by the Theological Faculty of Paris. "If it is not possible to extract the infant without killing it, it is not possible to extract it without mortal sin."

Radford, years ago, made the assertion craniotomy "can be justified on no principle, and is only sanctioned by the dogma of the schools or usage." Still, in England, the mother's safety has always been, and is to-day, considered of paramount importance. Osiander and Stein, joining in an affectation of sensibility, sought to condemn the operation of decapitation, and endeavored to banish the operation from text-books and practice. The experience and common sense of the Germans, however, have not, up to the present time, supported their efforts. Dr. Busey, we fear, in his "interest of a broader humanity and a far wider field of usefulness," will follow "the shining pathway to the stars," a solitary traveller.

2330 INDIANA AVE.

A CASE OF PROLONGED ASPHYXIA IN THE NEW-BORN BABE.

BY

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ON the morning of May 14th was called to attend Mrs. H. in confinement. My telephone rang at 4.18, and I arrived at her bedside, two miles distant, at 5 o'clock. Found the child and placenta both delivered. Was informed that the child came "feet first," and that the head and placenta came away at 4.30. The child was cold, and as it had shown no signs of life, I laid it to one side, considering it dead.

Not wishing to appear indifferent as to the life of the child, I placed my ear over the heart, not with any expectation of hearing

¹ "Si l'on ne peut tirer l'enfant sans le tuer, on ne peut sans péché mortel le tirer."

it beat, but to my surprise found it still thumping, though very feebly, at about 30 per minute.

I at once began artificial respiration by the direct method, inflating the lungs every ten seconds. Improvement in the action of the heart encouraged the continuation till at the end of fifty minutes the child gasped very slightly. Continued the artificial respiration fifteen minutes longer, when it was gasping three times per minute. At this stage I plunged it into a bucket of hot water (105° F.), taking care to keep its face out of the water; after rubbing and tumbling it about for ten minutes, and getting it warmed up, it improved in color and breathed ten times per minute. One hour later it was breathing twelve times per minute. The child improved till about 12 M., when the mother noticed the breathing became more difficult. The child died at 2.30 P.M.

The possibility of resuscitating a child which had been born thirty minutes, and still more it being a breech presentation, with head and placenta coming away at the same time, seems impossible.

For this reason I report this case, that some one who may be more fortunate than I in having a head presentation, or less time having elapsed before reaching the bedside, may be encouraged to make the most use of even the least indication of life, and be rewarded by the saving of a valuable life.

TRACTION UPON THE CORD DURING THE THIRD STAGE OF LABOR.

BY
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HAVING read, with much interest, the various articles in this JOURNAL upon the delivery of the placenta, and the method of conducting the third stage of labor, I am well aware that the recent teaching upon the subject favors the method of Oredé and ignores the cord entirely as a means of assistance. But having reason to believe that the cord is not so universally ignored in practice as we are led to believe by the various articles in the journals, I would like to make a few practi-

cal observations as to the method which to me seems proper, with some of the reasons for the same.

I am not unmindful of the fact that, in the light of recent literature upon the subject, it may be considered by many as taking a step backward to advocate traction upon the cord as a means of assisting the delivery of the placenta. But the object to be attained is the emptying of the uterus of its contents, that its contraction may be more perfect, and the greater the facility with which it can be accomplished, and at the same time with the least amount of manual interference, the better for the patient.

The introduction of the hand or any part of it into the vagina, and still more into the womb itself, and especially when there exist certain unfavorable surroundings, is a matter to be deprecated according to recent views as to the etiology of the various forms of metria.

The period immediately after the delivery of the child, before there has been time for the parts to recover themselves and take on tonic contraction, is above all others that most favorable for the easy expulsion of the secundines. The earlier this is accomplished, the more thorough and perfect the contraction that can be secured and the less the loss by hemorrhage, and consequently a lessening of the tendency to absorption from without, whether septic or not.

I am aware that many good authorities recommend letting the womb remain quiet for twenty minutes or more with the placenta inside, until the blood shall have time to coagulate in the uterine sinuses which have been torn open by the separation of the placenta. If this coagulation takes place, what becomes of the coagula when the womb contracts, and what possible after-effects may come from their decomposition or from their becoming loosened by the recurring after-pains which it would seem but reasonable they might aggravate by their irritating presence, similar to clots in the uterine cavity?

Acting upon these views, it has been my constant practice, and I think with good results, certainly with a saving of at least half in time, which is of no small importance to the peace and comfort of the woman's mind who is so constantly harassed with anxiety lest there are adhesions or may be hemorrhage,

or something else wrong, until assured that all has been delivered, that she is through.

Briefly, the method is this : placing the hand upon the abdomen, follow the uterus down as the child is delivered ; when this is accomplished, *never remove the hand from the abdomen*, but continue by gentle friction to stimulate the uterus to contract, and when it is found in the act, by its increasing hardness, grasp it firmly with the hand already on the abdomen, and produce all the compression possible, at the same time pressing downward and backward, simulating the method of Credé as much as possible with *one hand*, at the same time with the other grasp the cord and make traction in the axis of the uterus as determined by the knowledge obtained by the relation of the two hands, watching carefully, and cease the traction upon the cord at the first evidence of relaxation in the womb, or just before. If the placenta is not delivered with the first effort, which it often is, continue the frictions upon the abdomen as before, and repeat the compression and traction with each recurring pain ; *never allow the womb to be out of the hand until the placenta is delivered*, except when fully realizing that there is peril to both patient and accoucheur by so doing ; and *never produce traction upon the cord except when the womb is in a state of contraction*, as positively determined by the hand on the abdomen.

The child is cared for by the hand which makes traction upon the cord, is laid as much out of the way as the length of the cord will allow, and simply covered to protect it from cold, the cord not being tied until the placenta is delivered, unless in case of unusual delay.

Some of the advantages are a saving of time to the mother, which also means a saving in the amount of hemorrhage, the allowing the womb to contract immediately, thus closing the uterine sinuses before coagulation can have taken place in them, a less liability to post-partum hemorrhage in consequence, and less after-pain. There are no clots to be removed, and there is no necessity for carrying the hand into the vagina for that purpose, thus avoiding the possibility of carrying with it the germs of disease.

Traction thus applied acts upon the placenta as a compact body, and not locally upon the site of insertion of the cord

until it has passed beyond the influence of the uterine contraction, then it readily becomes inverted, and is usually so delivered.

To the objection that there is danger of inverting the uterus, I do not hesitate to affirm that that is an impossibility by any traction which the cord will withstand while the womb is in a state of *contraction*.

The danger from traction upon the cord has been that it was wont to be made quite as much while the womb was relaxed as when contracted, no distinction being made. The patient was instructed to cough, to bear down, or was induced to sneeze by taking snuff, to make efforts at vomiting by a wisp of hair, a candle, or what not thrust into her throat, while the faithful self-constituted midwife held firmly to the cord with both hands lest it should get away, and passing up into the vagina be lost.

No one can doubt but that traction upon the cord during the intervals between the pains is dangerous, but during the contractions any amount of traction which the strength of the cord will allow is not only legitimate, but wise, and shortens this stage just in proportion to the amount of force used, and further this method insures that careful and intelligent oversight of the condition of the womb during the third stage which is so valuable a feature in the method of Cr  d  .

As a reason for my confidence in this method, I may say that, in over 1,350 cases since I have practised it, I have not, to the best of my recollection, been called back to but a single case of post-partum hemorrhage. That was a case of twins, and after six days pain and hemorrhage coming on, I found the os distended with a mass of placental tissue unlike the fully developed placenta, but resembling one of two or three months. At the time of confinement, the placenta was examined as usual, and found to all appearances complete. There have been only two cases of flooding during the third stage previous to my leaving the patient, and in one of these, efforts to resuscitate a still-born child claimed my attention, and when at length I returned to the delivery of the secundines, found we were getting quite free hemorrhage. I will only add that it is an invariable custom to give a full dose of ergot

as soon as the placenta is delivered, and watch the uterus carefully until it has had time to take effect.

THE OBSTETRIC SIGNIFICANCE OF LACERATION OF THE CERVIX UTERI.

BY

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OBSTETRICIANS have long remarked changes in the physical appearance of the mouth and neck of the womb, wrought by parturient lacerations. These latter take place in primiparæ always; at least, if exceptions are possible, general opinion accords them the prominence of very great rarity. Thomas supplies the following list of causes: precipitate labor; manual delivery; instrumental delivery; labor with rigidity of os; cicatricial material in tissue of cervix; cancerous degeneration of cervix; section of cervix during labor; too early evacuation of liquor amnii; abortion.

Some physicians declare they are more readily produced when labor transpires with unwonted tardiness than when it pushes forward with excessive rapidity. No matter how originated their clinical history afterwards is much diversified. Thus the edges may coapt and heal by first intention, or remain apart and become covered up with cicatricial tissue. Then, each of the foregoing processes may occur in the same wound, the former in the upper, the latter in the lower portion; and lastly, there may arise inflammatory developments with concomitant hypertrophy and eversion.

Emmet's discerning observation and respected judgment have recently made palpable the dependence, often, of many troubles on persistence of the textural lesion.

Other writers on diseases of women aver the correctness of this author's deductions, and insist upon the magnitude and importance of his newly established pathology.

Gynecological literature, though, properly enough, takes cognizance of the offending state only in the absence of pregnancy,

or during pregnancy only prior to the period of accouchement. The disturbing entity, however, continuing till a new delivery, as it does in myriads of instances, loses nothing of its capacity for evil on that occasion, but becomes too frequently both the predisposing and exciting cause of manifold dystocias. Concerning these the volumes on obstetrics, and the pages of various medical journals are strangely and profoundly silent. So far as the writer is aware, the history of lacerated cervix during labor has never been written. It is a condition potent and prolific in disastrous accompaniments, yet the mechanical rationale and scientific philosophy subtending the fact of its morbid influence have not been formulated.

The purpose of this paper is to register briefly thoughts and conclusions in this regard, based upon orthodox anatomical, physiological, and pathological premises and emanating from personal experience. Probably there are accidents cropping out of the faulty circumstances which have been entirely overlooked, but those herewith enumerated constitute at least a modicum of such as are sometimes thus developed: prolongation of first, second, and third stages of labor; irregular and spasmodic contractions; premature rupture of the membranes; precipitate labor; ante-natal hour-glass contractions; rigidity of the os uteri; puerperal eclampsia; adhesion and retention of placenta; post-natal hour-glass contractions; post-partum hemorrhage.

The *modus operandi* by which the above are produced is variable, the result being accomplished directly in one instance and indirectly in another. A thorough apprehension of the indirect manner of development is attained from simple illustrations, and in a short exposition of the two methods its treatment shall be ceded precedence.

To begin, Simpson voices the opinion of the profession concerning dilatation of the os, when he asserts that this is effected by "muscular uterine action and mechanical pressure."

Pressure is made primarily, of course, by fluid in the advancing membrane, whose presence operates like a wedge on the cervix, preserves proper relationship between the womb and its contents, and increases effectually the power of the uterus by maintaining a straighter direction of its contracting fibres. Laceration of the neck, as will be seen hereafter, commonly

promotes too early rupture of the bag of waters, and by destroying, through that calamity, the preceding circle of harmonious correlations, becomes a fruitful source of rigid os, and of the trouble styled "ante-natal hour-glass contraction," or "tetanoid falciform constriction of uterus."

Anon it will be remembered that delayed involution, subinvolution of a part or the whole of the uterus, and corporeal endometritis stand forward amongst the remote or immediate effects of the topical disfiguration. Byford is but one of several who declare post-partum bleeding a familiar consequence of the first result, and metrorrhagia an equally constant associate of the second. The third likewise occupies a position of disastrous influence in obstetrical pathology, being accredited instrumental in prolonging the stages of labor. This it does through the formation of decidual adhesions, and the production of irregular spasmodic contractions.

Dr. Trenholme, of Montreal, says: "Contractions of this sort arise most frequently from abnormal adhesions between the uterine walls and decidua;" and "adhesions" quoting this time from Playfair, "most probably result from a morbid state of the decidua, which is produced by antecedent disease of the uterine mucous membrane.

Viewed according to chronological succession and tested by the accepted logic of cause and effect, the foregoing facts and opinions establish firmly an indirect dependence of the morbid phenomena at times on a partial disintegration of the cervix which antedates them.

Reference has been made to laceration as an agent involving too early rupture of the amnion. In order to a better conception of this involvement, it will be advantageous to revert preliminarily to some physiological data of pregnancy connected with the diminution in length of the uterine neck, the dilatation of the os, and the subject of pain or contraction. Cazeaux states that, in the ninth month of pregnancy, the whole length of the neck disappears by being confounded with the cavity of the body. "The mechanism of this fusion," he goes on to say, "is very simple. The ring at the internal orifice having lost all power of resistance from its ramollissement, opens so as to admit the finger's extremity, and this dilatation gradually augments under the influence of those feeble contractions by

which the uterus, in the last fortnight of gestation, seems to prelude the labor of childbirth, and as soon as this is sufficiently advanced to permit the inferior part of the ovum to engage in the cavity of the neck, we can understand that the latter is promptly trespassed upon." He also observes that "the internal orifice sometimes opens too soon." Thus Desormeaux declares that he "touched" the membranes at the end of seven months over a space of an inch and one-third in extent." These facts apply pertinently to the subject. One can readily comprehend in either condition of which he speaks that, while the tissue of the cervix remains unbroken, the support afforded on all sides to the engaging ovum will admit of such encroachment with perfect impunity.

Likewise it is conceived with facility how a solution of continuity, reaching as it often does the summit of the vaginal portion, would make the neck at that point of its circumference afford diminished support to the corresponding area on the periphery of the invading sac, allow it to project irregularly through the fissured surface, subject it to the vacillating pressure brought about by uterine contraction and endless vicissitudes of occupation or position, and render it exceedingly liable to be prematurely ruptured.

Quite a respectable proportion of the summary of this accident will be discovered to have originated in this way when time and clinical experience shall have aggregated sufficient reliable statistics.

Hour-glass contractions and other disagreeable manifestations after delivery are, by similar processes of deduction, made indirectly attributable to the same source.

More interesting and more important perhaps is the theory of direct interference with the smooth adjustment and regular progress of events attending normal parturition. During pain in labor, when the anatomy of the uterus is correct, the whole circumference of the os is simultaneously affected by the dilating traction of the longitudinal fibres, and each successive pain marks increased dilatation.

Where there is a lacerated cervix, the line of sequences is not rarely of another character. The sides of the tear are drawn widely apart by fan-like expansion, until the force of the contraction begins to subside, when they again approxi-

mate, leaving as a result the mouth of the womb somewhat enlarged, but materially less so than in the former instance. The reason for this obvious difference is nearly altogether mechanical, growing largely out of failure on the part of the torn edges to resist yielding to the powerful traction exercised upon adjacent cervical tissues. Besides, there is wanting here the firm pressure of the waters and fetal head against the inner circular fibres of the neck that so happily expedites affairs when the outline of the cervix is entirely normal. The receding fibres create an obstacle of varying degree to such desirable pressure, and the stimulus to uterine contraction arising therefrom being proportionately diminished, the pains assume an abortive type, growing inefficient in force, regularity, and completeness. At length, in both instances, the opening is duly accomplished, but not in the second till a tedious procrastination of the process by which it is attained has made the detrimental capacity of the local disorder thoroughly appreciated. Sometimes, owing to peculiar features of the tear, the most usual being when it is bilateral or multiple and of moderate depth, results are attained diametrically opposite to those described in the preceding statement. The external portions of muscular canals, wherever located, are naturally the most powerful in contraction and qualified to oppose most obstinately the forces employed to effect their relaxation.

Where the wounds in the cervix are superficial, the resistance begotten by the inferior segment of the latter to the opening of the os and expulsion of uterine contents is, within certain limits, diminished gradually in correspondence to their extent. Sims' and many other valuable operations for the relief of mechanical dysmenorrhea have been evolved out of the practical application of this knowledge.

It also supplies a share of the explanation why parturitions succeeding the first are generally accomplished with increased promptitude.

Associated with tenderness of apices or edges, the same lacerations are, in cases by no means isolated, productive of relentless ergot-like contractions, and uncontrollably precipitate labor, inducing at least marked prostration of the parent, and often enough leading to fatal asphyxia of the child.

Hemorrhages following labor, traceable directly to a fresh

laceration, have been recorded. Dr. Pallen, of New York, has reported a case where, after failure of milder measures, the flow of blood was successfully controlled by introducing Sims' speculum and sewing up the visible plaga.

That puerperal convulsions are to be included amongst the pathological developments from the organic disruption is an idea neither far-fetched nor improbable, though, in the absence of positive clinical demonstration, its foundation must rest upon collateral reasoning. A portion of this, brief and crude, is herewith submitted.

Authors differ as to the propriety of artificial dilatation with the finger in cases of rigid os uteri; some, like Playfair, insisting upon its safety and benefit, and others—Milne for instance—objecting that the excruciating distress arising therefrom almost always precludes its employment. The solution to this divergence of opinion, the writer believes, consists in the fact that in the practice of those physicians advocating the measure, the rigid os was associated, in a majority of instances, with an untorn condition of the cervix; while, in that of those opposed thereto, a preponderance of lacerations accompanied this cause of dystocia.

Again, it is known that the torn site, besides being the centre of unremitting pain, is capable, like rectal and other fissures, of giving rise to the highest manifestations of cerebro-spinal irritability.

Sutton ("Am. Gyn. Trans.," 1880) records an interesting example of "catalepsy in the unimpregnated female, where the convulsions could be produced at will by pressing the finger into the angle of laceration, though they did not occur on any other manipulation of the cervix." Finally, amidst the confusion and doubt surrounding the etiology of puerperal eclampsia, the marvellous therapeutic achievements of chloroform, opium, blood-letting, and forceps delivery in some cases could be pleasantly and perfectly accounted for upon the hypothesis of their promptly removing nervous irritation, resulting from pressure of the fetal head on the seat of injury.

It may be well, even at the expense of tedious emphasis, to state again that from ratiocination of the above character conclusions are derived regarding the attitude borne by the local injury to the terrible convulsions. Undoubtedly there are

instances where parturition pursues the even tenor of its way, not modified by the presence of a well-marked laceration.

Whether this or the reverse shall transpire in a given case may depend on the existence of certain conditions in the vicinal structures and the happening of obnoxious fortuities.

Recalling the list of troubles, traceable directly or indirectly to the cervical derangement, together with the explanation of their mode of development, it will be realized that, outside of the extent and tenderness of the wound, the springing up of such troubles is particularly favored by the occurrence, *pari passu*, of multiple pregnancy, hydropic amnios, fragility of the membranes, breech and transverse presentations, blows, injuries, and unusual exertions.

The matter of diagnosis is generally simple. The patient herself may have positive knowledge of a laceration, or the physician may have attained similar enlightenment from examinations instituted antecedent to *accouchement*.

Early in labor, the finger recognizes the condition by feeling one or more somewhat vertical and angular fissures interrupting the smooth continuity of the cervical margin, and by the scissors or fan-like expansion and closure of these during and after each uterine pain. The sac of waters may also present an irregular projection or off-shoot at some portion of its circumference.

Later, when the membranes are ruptured, the os dilated, and the head well down in the passages, a novice may overlook their presence, but they still discover themselves plainly to those possessing *tactus eruditus*.

The prognosis is modified according to associated complications, the duration of labor, and the methods of treatment adopted.

When there are no complications, the outlook is universally favorable. As to treatment of the difficulties referred to, this must be both prophylactic and remedial.

Prophylaxis requires that, in the interim of pregnancies, the sufferer shall be afforded the opportunity and advantages of Emmet's hysterio-trachelorrhaphy. During the fortnight preceding confinement, she should be enjoined against the heavier physical exertions of housekeeping, requested to avoid ascending and descending high flights of stairs, and admonished to protect herself as carefully as possible from such accidents as

might *a priori* be supposed to render likely an untimely evacuation of the amniotic liquid.

When labor has begun, and while the membranes are still intact, the recumbent posture should be perseveringly maintained.

It is clear, from the title and tenor of this writing, that its scope does not embrace the discussion of measures intended to prevent laceration, but only such abnormalities in the course of parturition as are liable to supervene thereafter.

Remedial treatment must be varied to meet the special indications, and only a few suggestions, therefore, shall be offered under this head. Before proceeding to them, let it be accepted as understood and duly appreciated that, whenever *vis medicatrix nature* is properly adequate to the undertaking at hand, artificial interference becomes culpable mismanagement.

Chloroform, cannabis indica, opium, chloral, and ergot, with hot injections, hip baths, belladonna ext., orificial irritation, and the hypodermic injections of Horton fulfil satisfactorily, under judicious selection, the requirements arising from precipitate labor, rigidity of os, spasmodic and irregular contractions, puerperal convulsions, hemorrhage, and a number of other harassing dilemmas, the tangible fruition of a previous tear.

A most interesting and important necessity arises in cases where, notwithstanding the pains are strong and regular, and the waters ruptured, the softened and completely severed cervix yields tardily, the head fails to engage therein properly, the uterus threatens to give way under continually increasing tension, the woman betrays premonitions of impending exhaustion, and other incongruous features with dreaded significance loom up portentously.

This is not a usual form of inefficient action, the source of delay being chiefly mechanical, as was understood while furnishing the causation and symptomatology. Ergot is forbidden by reason of the ordinary contraindications, Kristeller's abdominal pressure, singly or combined with digital traction on the cervical rim, is often devoid of benefit, and the very nature of the premises precludes the inference that assistance could be derived from the hydrostatic pressure of Barnes' dilators. Now, then, if ever the intelligent application of forceps

to the head within the cervix, as practised at the Rotunda Hospital, is the scientific and legitimate remedy, the alternatives, ergot, version, craniotomy, Cesarean section, and Micawber-like patience, multiply inordinately the fetal and maternal peril.

Treatment of fresh lacerations by immediate operation is advocated especially by Dr. M. A. Pallen, who urges advantages from the procedure identical with those accruing from similar treatment of ruptured perineæ. Should it fail, the ordinary precautions are to be used against septicæmic infection through the raw and divided surfaces.

It was my contemplation to terminate this paper with a cursorily detailed recountal of the management of several parturitions made troublesome by lacerated cervix, but the length of this paper induces me to defer their recital to a future occasion.

A CASE OF TAIT'S OPERATION.

BY

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ON November 28th, 1882, I was called to see Mrs. Alice M. C., twenty-seven years old, twice married, and no children. Menstruation commenced at the age of twelve, regular but painful. Soon it grew more profuse, lasting a week or ten days, and she began manifesting more or less nervousness and constitutional disturbance. Even at this early age there was doubtless hyperæmia of the ovaries, and, as one writer justly says: "There is no cause of deteriorated general health so certain for a young woman as profuse menstruation due to ovarian hyperæmia."

A few months before her marriage, which took place at the age of eighteen, she had a severe attack of what was then pronounced to be "inflammation of the bowels," but which, no doubt, was an attack of cellulitic and peritoneal inflammation, from which sickness, her mother said, she never fully recovered; and probably at that time the hyperæmia of the ovaries developed into acute ovaritis, and also probably commenced that tubal difficulty which finally proved so disastrous.

Her health continued to grow worse, she commenced having spasms and fainting spells, and we are told that "a physician attended regularly, and burnt ulcers;" but quite likely at this

time there was no special disease of the womb, but an increasing disease of the ovaries, which was more or less affecting the nervous system. These spasms and convulsions recurred at every monthly period. She would first complain of intense pain and distress in the region of the ovaries, then faint away, have a spasm, recover, again scream with the intense pain, have another spasm or convulsion, and so she would frequently have as many as ten or fifteen during the night. In these convulsions she was at times unconscious, "acted strangely," twisting and throwing herself into all conceivable attitudes and positions, sometimes opisthotonos, neck swelling out enormously; then she would spring straight up in bed, showing almost superhuman strength, or she would be all doubled up in agonizing pain.

These spasms first commenced in the fall of '76, and year by year they continued to grow worse. Sometimes in conversation she would spring into a spasm, muscles rigid, teeth clenched, difficulty in breathing, etc. It did not seem possible that she could live. Many times she would wake out of sleep with pain, go into a convulsion, or perhaps she would crouch down under the bed clothes, screaming with fright, and trying to keep imaginary persons from killing her, and sometimes she would mount on the foot of the bedstead, and play she was at opera, etc., etc.

During these years, she was from time to time attended by various physicians, and had various kinds of treatment—took quantities of bromides, tonics, hypodermic injections of iron, had electricity applied, etc. Nothing seemed to help her or give her any relief. She finally came to depend upon hypodermic injections of morphia, never going out, even for a few hours, without taking with her the hypodermic syringe, using daily two, three, or four grains of morphia. During '77, a prominent gynecologist was called in, saw her in one of her spasms, said there was something growing behind the womb, and that she would not live three days. About the same time, there was called in consultation another physician, a specialist in nerve diseases, and said she would not live three weeks. In one of her spasms he adopted the method first suggested by Charcot, of strong pressure upon the ovary; and one might imagine it suspended the spasm, as it is said she violently slapped him in the face, leaving an impression that he must have felt for hours.

In November, '82, when I first saw her, she was pale, feeble, emaciated, confined to her bed most of the time, nervous, sleepless, restless, no appetite, constipated bowels, feeble pulse, yellow inactive skin. There was no disease of heart, lungs, liver, or kidneys. All her distress she located in the pelvis, where she had continuous and sometimes extremely severe pain. Her whole nervous system was in a very weak, excitable condition. At times she manifested a little confusion in her thoughts, but naturally she was bright, quick, and intelligent. She had at this time just passed through her supposed menstrual period, and had suffered all the night previous with tonic spasms and clonic convulsions. These convulsions and tetaniform contractions were

now no longer confined to the menstrual period; but every few days she had these jerkings and twistings of the limbs and horrible contortions of the body. Frequently her legs would be spread rigidly apart till each was at right angles with the trunk of the body, while her feet and limbs would be drawn and jerked by continued spasms, head rolling, and grimaces of the face, and she at intervals crying with "the agonizing pain in the womb."

I considered it a case of hystero-epilepsy due to reflex irritation. Her attacks were preceded by an aura rising from the region of the painful ovaries. Evidently there was a large hysterical element in these epileptic seizures; her feet presented the appearance called by Charcot "*le pied hystérique*," and many of her positions reminded one of a woodcut of Ler, of the Salpêtrière, made from a sketch taken by M. Charcot. Well has one of our best writers said: "Hystero-epilepsy is the most frightful affection to be found in the whole range of medicine."

I made an examination of the pelvic organs, found the uterus retroverted and retroflexed, cervix lifted up well under the pubes, fundus acutely flexed at the internal os, and bound firmly down by inflammatory adhesions; continuous with the fundus on her left was a mass, size of an ordinary orange, extremely tender and immovable, some portions of it hard and firm. This mass could be nothing else than the ovaries and Fallopian tube in some diseased condition. The left Fallopian tube could be easily mapped out, and seemed distended by some cystic formation, most probably pyo-salpinx.

On her right there was no enlargement, but great tenderness.

The condition of the ovaries and Fallopian tubes was without doubt the cause of her ill-health and of her serious nervous symptoms. I first directed attention to improving her general health, and as much as possible reducing the peritoneal and pelvic inflammation. Still I felt there was but one way to relieve her or give her any chance of being cured, and that was to remove the diseased structures. If not removed, they could only be a continued source of danger and distress. Every attention was given to her general hygienic condition, and she seemed to improve; her spasms seemed less frequent and less terrific. In December, she returned to her home in Maine; while there she had a fright from a runaway horse, and February 28th she came back to Brooklyn, not so well, spasms more frequent and more severe, her menstruation showing more irregularity and scantiness, lasting scarce an hour, almost *nil*. I felt still there was but one hope for her, viz., operative interference; but she was in so depressed and wretched a state of health that I could not think of so dangerous an operation. She urged it and urged it very strongly, almost grew angry when I put it off, said:— "She wanted the operation if she died under it." But I could do only what I thought would be best for her. Her whole system was so prostrated and so demoralized that not a single organ was acting vigorously. Yet it was evident that she could not live without

help, and so frightful were her convulsions that her excellent husband once said, "He would rather she would never come out of ether than be as she had been."

I invited Dr. B. F. Westbrook to see the case; he thought an operation was demanded, but recommended first the trial of massage. Massage had been given her, but with no apparent benefit; still I ordered it again and regularly for a month, and with no better results.

In April, I requested Prof. B. F. Dawson, of New York, to see the patient. After a thorough examination, he gave it as his opinion that the operation should be done, and done as soon as she could be made ready; that it was her only hope, and that if not done she could look forward to only a short while of miserable existence, and probably end her life in the insane asylum.

The 14th of May was fixed for the operation. Every precaution was taken as to perfect antisepsis. There were present Dr. B. F. Dawson, Dr. J. H. H. Burge, Dr. F. W. Rockwell, and Dr. C. N. D. Jones. I made an incision of two and one-half inches between the umbilicus and the symphysis pubis, passed two fingers into the pelvic cavity and verified the cystic formation. The ovaries, womb, and Fallopian tubes seemed completely matted together, and bound down in one inflammatory mass; firm adhesions in every direction, so terrific that I invited Prof. Dawson to help liberate the organs. The incision had to be enlarged two inches; Prof. D. thought he had never seen so bad a case; firm adhesive bands, some of them half as thick as the little finger, were attached to the pelvic walls, to portions of the intestines, and the whole of the posterior surface of the fundus of the uterus was completely covered with the inflammatory adhesions, binding it down and causing an acute flexion, the left Fallopian tube was distended into a cyst some two inches in diameter, and it coiled sausage-like around the atrophied ovary; while to the ovary was so closely adherent the fimbriated extremity that the Fallopian canal was entirely occluded. At first it was difficult to define the ovary, so atrophied was it, and so firmly was it held in the iron grasp of the tight adhesions.¹ All the special or gland structure of the ovary was entirely destroyed, no appearance of any Graafian follicles, and the organ was no longer capable of any functional action.²

¹ If the attempt had been to remove only the ovaries, it would have been impossible, like a case which Dr. Battey related before the Am. Med. Association, when he attempted to remove the ovaries without success. The number of reported incomplete operations show the difficulties that may exist. Dr. Engelmann says of one of his cases: "The ovaries were so deeply imbedded within the folds of the broad ligament, and with them so firmly bound down to the sides and the floor of the pelvis, that it was impossible to move them." He enlarged the incision to two inches above the navel, and removed the intestines from the pelvic cavity before he could remove the ovaries.

² Many have made the objection that the removal of the uterine ap-

There were a number of small parovarian cysts in and around the broad ligament, each one filled with a clear transparent fluid. Many of these had no doubt from time to time burst, causing circumscribed attacks of peritonitis, which steadily increased the amount of adhesions.

After freeing the adhesions, a double ligature was passed through the left broad ligament, and the left ovary and left Fallopian tube were removed. The continued oozing from the posterior surface of the uterus was checked only by the actual cautery. Some large adhesions had, when severed, to be ligated at each end and cauterized. The right ovary and right Fallopian tube were examined, found to be somewhat diseased, but it was not considered advisable to remove them.

The cavity of the pelvis was carefully cleansed, the toilet of the peritoneum made, the abdominal walls closed with silver sutures, wound dressed, and she put in bed covered with blankets, and bottles of hot water placed around her. She recovered beautifully from the immediate effects of the operation, seemed cheerful and comfortable, no fever or peritoneal inflammation, and was apparently doing well till the fifth day, when the temperature went up to 103. In the afternoon of the same day, she commenced vomiting; next morning, sixth day, temp. 105, then 107; septicemia, the end. So rapidly did "the poison chase through the gates and alleys of the body."

Dr. Dawson very kindly saw her on the morning of the sixth day. No one expected such conditions, every symptom had been so good that we reasonably concluded on her recovery. On the third day at 12 m., the temp. was 100; pulse 84. On fourth day at 10 p.m., temp. 100 $\frac{1}{4}$. During her sickness she took, and seemed to tolerate large quantities of morphia. And in these first three or four days she rested and slept well, took nourishment, passed her urine regularly, talked and conversed with more apparent calmness and healthful condition of the nervous system than she had manifested for months previously. She seemed natural, talked of her future plans, her arrangements, and her household affairs. And she had not at any time the least symptom or manifestation of any convulsion. Her nervous system seemed normal and placid, and no unfavorable symptoms.

The day after her death, the post-mortem was made by Dr. C. N. D. Jones, Dr. Dawson and myself present. The abdominal wound had healed. The part from which the ovary and Fallopian tube had been severed was healing nicely. One lymph band which had been ligated had oozed some; other conditions were good.

The specimens were exhibited at the Post-Graduate School of Medicine, and at the Obstetrical Society of New York, and pendages takes away all capability of having children, but most frequently disease has already destroyed all capability of procreation, as it most certainly had in this case.

as we look at their utterly diseased condition, we see how her only chance of life was their removal. No doubt, seven or eight years before, there was agglutination of the fimbriated extremity of the left Fallopian tube, and even at that time, the only way by which she could have been relieved of her suffering, and given any tolerable degree of health, was by the removal of the left uterine appendages. Battey's three questions were here well answered ; it was a grave case, it was curable by no other means, and it was reasonable to expect a cure by this method. Had the left annexæ then been removed, she would have been saved years of suffering, had years of health added to her life, and possibly have been the mother of children ; for at this time the right ovary and Fallopian tube were in good condition.¹ And to have become a mother, her happiness would have been complete. She was then just within the portals of womanhood, bright, active, energetic, with a large heart, kindly sympathies, and great capabilities of usefulness. But the diseased structures were left, disease extended, inflammation increased, adhesions became more general, and she grew continually worse.

But to go still further back in the history of this case, to the time immediately subsequent to her marriage, when she first began having the severe attacks of pelvic pain and spasms. At that time there was more or less peritonitis, ovaritis, and salpingitis, and, no doubt, these conditions caused the peculiar nervous manifestations. And at that time an exploratory incision should have been made into the abdomen, the cause found out and removed. Other measures had been tried ; all had failed. Why allow her to go on and suffer ? One of the greatest of English authors on this subject says : " I do not think I will willingly allow a case of peritonitis to die without an effort to save it by an operation. I am fully persuaded we might save many cases by boldly opening the abdomen and cleansing out the cavity."

But to go further back in her history, to her early menstrual life, when she had probably only hyperemia of the ovaries.

¹ Chaussier tells of a woman who had entire absence of tube and ovary of left side, and had borne ten children. And we have many instances of women bearing children after the annexæ have been removed from one side of the uterus.

She was then a lithe, graceful little creature, her finely organized nervous system was then all alive and quick with excitement, yet she was still more stimulated and excited by being brought before audiences, singing at concerts, and making elocutionary displays ; it was also stimulating the ovaries abnormally, and predisposing them yet more to disease ; it was using up her nerve power and biotic force, which was necessary for and should have been expended in physical development. Had her vital forces been so applied, and if she had had plenty of sleep, rest, out-door exercise, and sunshine, she would doubtless have grown strong and vigorous, every organ fully developed, and probably all her subsequent suffering and physical ills would have been prevented. To such a person, and with such conditions, early marriage was objectionable. She needed the opposite of ovarian stimulation.

This case shows clearly how that conditions that are at first indicated only by slight menstrual disturbances may develop into most serious disorders. It also shows how disease of the ovaries and Fallopian tubes may affect the general health and nervous system ; also suggests the desirability of early recognizing these affections, and not delaying the necessary surgical help, for there are some conditions, as we see clearly in this case, which cannot be helped in any other way than by Tait's operation—an operation that has saved many lives, that has had its triumphs both in this country and in Europe, and is yet to be a still greater blessing to humanity. The author of it has crowned himself with glory both for his advances and for his successes.

While, at the last, the operation on Mrs. McC. was her only hope, her only chance of life, yet at that late period there was much, almost everything against her, and against the chances of success. First, there was the entirely worn-out and prostrated condition of her system from nine years of suffering, nine years of pelvic inflammation of those organs which are so closely connected with life and so control the vital domain. Her strength was still further exhausted by frequent spasms and consequent loss of rest and sleep ; and this great prostration was still more increased by want of proper nourishment ; even the food she took was not assimilated, so the system was impoverished as well as exhausted. Her skin was so riddled

with little pustules that its functional action was almost destroyed, and it is recognized by every one what an important factor the skin is in the processes of life, and how essential it is to our well-being.

The immense quantity of morphia she had taken for years was a large item against her welfare. And the morphia produced such constant constipation of the bowels, which constipation rendered the blood impure, and was thus more or less vitiating to every organ, and disturbing its vital action. Probably, too, after the operation unhealthful gases were emitted from the loaded bowels, which might have poisoned the wound, and have been one cause of septicemia. One of our best teachers and writers says, in the large edition of his work: "After the operation for abdominal ovariectomy, it is essential that the bowels should be kept constipated for a week or ten days." Still the question comes up how a mass of impurity or of refuse material retained in the lower bowel will not do more harm than if it gently passes off from day to day. Mr. Lawson Tait says in his late excellent work: "I never take any steps to prevent the ordinary motion of the bowels; indeed, the administration of laxatives within a few hours after an operation is becoming quite a common practice with me; this innovation, in my opinion, being possibly conducive in some measure to my increased success. If the bowels show no indication to move of their own accord by the third or fourth day, an enema is given, and repeated at intervals till it has the desired effect."

Conclusions.

1st. This operation should have been performed on this patient five or more years before it was.

2d. There was no other way to relieve her but by the operation.

3d. Without it she was in constant danger from the pyosalpinx.¹

4th. The operation to her was not as much suffering, nor as much of a shock, or as great a trial as was a week of her usual suffering.

¹ The day before the operation I saw her in agonizing pain, and when afterwards I saw the distended Fallopian tube and its thin walls, I marveled that it had not then burst, and probably with fatal results. She was all the time on the brink of a dangerous precipice.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF NEW YORK.

Meeting, March 4th, 1884.

A SUCCESSFUL CASE OF LAPARO-HYSTERECTOMY FOR UTERINE FIBROIDS.

Dr. P. F. Mundé presented a specimen, with a written report of the case sent by Dr. ELY VAN DE WARKER, of Syracuse. Dr. Mundé remarked that the case was notable in that the patient recovered, and also because of the rapidity with which the operation was done. The question as to the best manner of treating the stump had been raised in this case, as at the last meeting of the Society; certainly the extraperitoneal plan was the one which had been successful in the hands of German surgeons. Recently the elastic ligature, clamped with lead, had been allowed to remain on the stump, to take the place of the clamp, by some operators. In spite of reported successes, one could hardly conceive how inflammation and abscess would not attend the presence of a rubber tube and a bullet in the peritoneal cavity. We had learned, however, that the peritoneum at times would stand almost anything.

REMOVAL OF THE OVARIES AND FALLOPIAN TUBES, FOLLOWED BY THE DISAPPEARANCE OF SYMPTOMS OF SPINAL ATROPHY.

Five years ago, Dr. MUNDÉ was requested to see a patient by Dr. Allan McLane Hamilton, she being at that time twenty-five years of age. Three years previously, while returning home one evening from a party, during the menstrual period, she was seized with what was called a fainting fit, and was unable to walk. She remained paralyzed in the lower limbs for some months. Gradually some power returned, but she remained unable to walk, being only able to put one foot a little before the other, dragging the left one as a patient affected with hemiplegia would. Dr. Mundé was asked to see the patient, because, at her menstrual periods, she suffered a great deal from pain in the left ovarian region and back, and from nausea and vomiting, which continued for two weeks out of every four. She became emaciated in consequence and had been all these years a bed-ridden invalid. A vaginal examination showed retroversion of the uterus and prolapse of the left ovary. The ovary was not much enlarged, but very tender. Pessaries and other means for maintaining the womb in an improved position gave rise to pain, intravaginal and abdominal counter-irritation gave but temporary relief, and finally treatment was abandoned. Oöphorectomy was suggested, and the patient was anxious for it; but it was not countenanced, because Dr. Hamilton had examined the patient very carefully and found undoubted symptoms of

chronic myelitis in the lumbar region of the cord. Dr. Thomas and Dr. Emmet had seen the patient before she came under Dr. Mundé's care, and both had also discountenanced an operation, believing that the disease of the cord would in no way be benefited thereby. In this opinion Dr. Mundé entirely concurred. During the next three years, Dr. Mundé saw the patient only occasionally, when she suffered more than usual from ovarian pain. Last autumn, she again raised the subject of removal of the ovaries, and was more anxious than ever to have the operation performed, especially after she had been shown the report of a recent case (in the *Centralblatt f. Gynäkologie*) of a woman who had been for seven years in a similar condition, unable to walk and forced to take nourishment through a tube, because of continuous trismus, and who, six months after removal of the ovaries, was able to walk six miles, and made a perfect recovery. After due deliberation, Dr. Mundé finally decided to give the patient the chance of possible benefit from the operation, provided a consultant agreed. Dr. Emmet again saw the patient, and he also thought that, while the operation probably would be of no benefit as regarded the ataxia, and perhaps of none as regarded the general debility and nausea, still, as there was no other possible chance of relief before the climacteric period, nearly fifteen years hence, she might as well be given this chance, knowing, as she did, the risks. On the 6th of January, both ovaries and tubes were removed, the uterus being elevated with a repositor. No special difficulty was experienced during the operation. The ovaries were not adherent. The highest temperature after the operation was 99.5° F. The patient, who was an unusually intelligent lady, called his attention, four days after the operation, to the fact that she was able to move the toes of the left foot, which she had not been able to do during the preceding seven years. About a week later, she was able to bend the left knee. About the fifteenth or seventeenth day, she was moved to a lounge and began gradually to learn to walk, beginning like a child. Instead of dragging the left foot, as she had formerly done, she placed it directly forward in the normal manner. It was now two months, lacking two days, since the operation was done, and Dr. Mundé had seen the patient this afternoon, when she walked the full length of the double room without assistance, and apparently as well as anybody could. He confessed that he was surprised to see such marked improvement in the spinal symptoms. The gastric symptoms had also greatly improved. There had been no sign whatever, not even a molimen, of menstruation since the operation.

SUCCESSFUL REMOVAL OF THE OVARIES AND FALLOPIAN TUBES.

DR. MUNDÉ also presented the tubes and ovaries removed, a week ago, at Mt. Sinai Hospital from a patient, twenty-four years of age, who had been married for three years, and for two years had suf-

ferred severely from dysmenorrhea and from hystero-epileptic attacks. Dysmenorrhea had existed before, but in a less marked degree. The patient was unable to work, and the epileptoid attacks, which occurred at irregular intervals and as often as three or four times a week, left her in a very prostrated condition. She had been unable to obtain any relief, and was therefore very desirous of oöphorectomy. Although she had been sent to him by her physician with the express purpose of having the ovaries removed, Dr. Mundé was skeptical about the propriety of the operation until after he had had her under observation for some weeks in the hospital. While sufficient time had not elapsed to speak of the permanency of the result, it would seem that the patient would be markedly benefited by the operation. There were extensive adhesions, and considerable difficulty was experienced in removing the ovaries, but the patient was making a very good recovery; the highest temperature occurred on the third day, reaching only 100° F. [She ultimately recovered perfectly.]

In reply to a question by Dr. J. G. Perry, Dr. Mundé said the left ovary in the first case contained three small cysts. He further said that there had been no flow, nausea, vomiting, or pain. The appetite had greatly improved. In the second case, there was distinct salpingitis on both sides.

THE TREATMENT OF CERTAIN CASES OF RETROVERSION OF THE UTERUS BY CAUSING POST-CERVICAL ADHESION.

DR. J. B. HUNTER said there were certain cases of retroversion of the uterus, unattended with flexion, in which the malposition seemed to be due to the mere force of gravitation, so easily was the organ replaced. It seemed to him that, if in such cases the cervix could be fixed to the posterior wall of the vagina, the uterus would cease to fall backward. With this idea he had recently denuded a portion of the posterior border of the cervix and of the vaginal vault, and fastened them together with sutures. When union had taken place, he would remove the sutures. The suggestion had been derived from seeing a case in which the same object was accomplished by cicatricial bands between the posterior wall of the vagina and the cervix, following inflammation.

THE EFFECTS OF A PRETENDED OÖPHORECTOMY.

The cases related by Dr. Mundé suggested the following one to Dr. W. R. GILLETTE. A German girl entered St. Francis Hospital with a history that she had been in nearly all the hospitals in the city, for severe dysmenorrhea, pelvic pains, and epileptic seizures. She professed to live without eating, but it was found that she took bread in some surreptitious manner. The Sisters watched her very closely and concluded that she was a hystero-epileptic. Dr. Gillette found prolapse of the ovaries. The patient was very anxious to have an operation done, and her mother stated

that doubtless she had been a real sufferer for several years. Dr. Gillette thought it a good case in which to try the influence of mind over matter, and made all the necessary preparations for oöphorectomy, placed the patient upon the operating-table, made an incision into the subcutaneous fat of the abdominal walls, and closed the wound. The patient improved wonderfully after the pretended oöphorectomy. He had heard, however, that she had lately had a return of the symptoms, and had presented herself at several hospitals, desiring that something more than the ovaries be removed.

DR. MUNDÉ said that he had reported a case to the Society, a year ago, in which the operation was unsuccessful as regarded relief from the symptoms. The patient had since committed suicide.

DR. W. G. WYLIE (present by invitation) mentioned the fact that he once removed both ovaries and tubes, and the patient had menstruated regularly ever since.

DR. W. M. CHAMBERLAIN said the case reported by Dr. Gillette was not altogether unique. He had been told by a friend that a gentleman in Brooklyn had cut into the abdominal wall nearly down to the peritoneum in a certain case, and then sewed up the wound, and that for six weeks afterward the patient was entirely relieved of dysmenorrhea and other symptoms. He had not heard in the case since.

Meeting, March 18th, 1884.

OVARIOTOMY.

DR. J. B. HUNTER narrated two cases of ovariectomy, in both of which the patients died. The first case was that of an unmarried woman, thirty-five years of age, who for nine months (and she was certain for not more than nine months) had suffered from pain in the pelvic region. A tumor was noticed in the right side for the first time last December. She came under Dr. Hunter's care March 1st. After a careful examination, he made a diagnosis of ovarian tumor, probably malignant. The patient was very much emaciated, and had the appearance of a woman suffering from a malignant disease. The tumor filled the whole abdominal cavity, and caused much pain by pressure. The patient's appearance was that of a woman of sixty-five. A consultation was held with two of his colleagues at the hospital, and they agreed that an operation was demanded, although the prognosis was unfavorable. There was some albumin in the urine. The operation was performed on the 10th of March. The usual abdominal incision was made. Some ascitic fluid was found and a tumor of large size on the right side, extending up to the diaphragm and attached to the abdominal walls and the intestines. The contents were evacuated, and the adhesions separated without much difficulty or hemorrhage. The pedicle, which was short, was transfixed and tied. Another tumor, of about half the size of the first, was then

found upon the left side, including the left ovary. Both tumors were multilocular. Within the left tumor were several papillomatous, cauliflower-like growths projecting from the walls. A small portion of the left tumor could not be removed, as it was very firmly adherent to the pelvic fascia. The operation was not of long duration, but the patient's condition was very low; she had had a high temperature for some time before, and death took place ten hours after the operation. There had been but little hemorrhage. The specimens had been examined by Dr. Welch, who pronounced the growth malignant.

The other case was that of an unmarried woman, eighteen years of age, who for four years had noticed some abdominal enlargement, which had increased decidedly during the last year. She had been a patient of Dr. A. N. Brockway. For the last year there had been a constant dull pain in the pelvic region, and for six weeks past a severe pain, requiring morphine for its relief. The tumor was so tense as to be readily mistaken for a fibroid. The patient had menstruated regularly, but had a metrorrhagia lasting one month one year ago. Ether was given February 28th, as a satisfactory examination could not be made without it, in consequence of the pain it caused. The mass was central and so hard that it was still considered doubtful whether it was uterine or ovarian. The depth of the uterus was two inches and a half. From that time until the date of operation, March 12th, the temperature varied from 100° to 104°, with nausea and vomiting, notwithstanding the use of the cold coil and quinine, and constant severe pain. The tumor increased in size, and the diagnosis of a suppurating ovarian cyst was then made. Dr. Thomas and Dr. Emmet agreed in the diagnosis, and advised immediate operation. On making the usual abdominal incision, a tense cyst was reached, which was so vascular and hard that it had the appearance of the uterus. A small puncture with Peaslee's needle was made, and a jet of extremely fetid pus escaped. None of the fluid was allowed to enter the abdominal cavity. The patient was turned on the side, and the cyst washed out thoroughly with carbolyzed water until the fluid returned clear. Then, on making an exploration, the cyst was found firmly adherent to the intestines, to the abdominal walls, and apparently to the uterus and other pelvic viscera. Very little investigation showed that it was impracticable to remove it. The abdominal wound was sewed up in the usual way, and the edges of the sac were stitched to the lower portion of the wound, so as to separate the cavity of the cyst entirely from the abdominal cavity. The operation lasted about an hour. There was but little hemorrhage. The patient rallied, but died early the following morning. The pulse before death was strong and rapid, and the temperature in the vagina rose to 107°.

The following report of the post-mortem examination had been furnished by Dr. W. H. Welch:

“Upon careful examination, it appears that the tumor is formed

partly by a suppurating intraligamentous ovarian cystoma and partly by an intraperitoneal abscess. The cystoma originates from the right ovary, which is therefore not to be discovered in the specimen. The left ovary is present, buried in adhesions. The inner surface of the posterior wall of the cyst is lined by cylindrical epithelium. In front, where the posterior surface of the uterus forms a part of the cyst-wall, there is no epithelial lining. The formation of the tumor may be explained as follows: There grew primarily a cystoma from the right ovary, between the layers of the right broad ligament. This cystoma consists of one large main cyst and several secondary cysts, which can still be made out, partly with purulent contents. Suppuration occurred in the main cyst, which resulted in a partial destruction of its walls, and the formation of an intraperitoneal abscess, shut in by adhesions, but communicating with the main cyst. The adhesions encapsulating the intraperitoneal abscess were so formed as to include the abdominal extremities of both Fallopian tubes, the mouths of which opened into the abscess cavity, and hence indirectly into the cystoma. The wall of the cystoma corresponding in its relations to the posterior wall of the uterus was entirely destroyed, so that here the uterine wall is also a part of the cyst-wall. The inner wall of the cystic tumor is therefore partly peritoneum and partly the mucous layer of a cystoma, but, except by microscopical examination, it is not easy to tell how much of the common cyst belongs to the cystoma and how much to the intraperitoneal abscess. The ovarian origin of the tumor is proved by its multilocular character, by the lining with cylindrical epithelium, and by the absence of the right ovary. The intraligamentous mode of growth is proved by the peritoneal covering of the surface, and by the incorporation of the Fallopian tube and the round ligament in the cyst-wall. The partial formation of the cyst out of an intraperitoneal encapsulated abscess is proved by the defect of the cystoma-wall in places and its replacement by peritoneum (posterior wall of uterus), and by the communication of both Fallopian tubes with the interior of the cyst, a communication difficult to explain upon any other supposition. Around the margins of the openings of the tubes into the cyst the fimbriæ can still be made out, so that dilated tubes form no part of the cyst."

Had it been possible to make an examination two or three weeks earlier, the patient's life might probably have been prolonged by an operation. If, however, tapping had been resorted to as a means of diagnosis, the cyst being so tense, some of its putrid contents would doubtless have escaped into the abdominal cavity and caused fatal peritonitis.

DR. J. E. JANVRIN asked of what the second patient really died; whether of shock, or of septicemia which had existed for some time previous to the operation, as indicated by the persistent high temperature. He was rather of opinion that death took place

from septicemia, perhaps aided somewhat by the shock of the operation.

DR. HUNTER agreed in this opinion, and said he thought that if the patient had been let alone she would certainly have died, within two or three days, of septicemia.

DR. POLK asked Dr. Hunter, with regard to his first case, what he thought would have been the effect upon the ultimate result of the case of leaving the small, most firmly adherent portion of the cyst alluded to in the pelvic cavity.

DR. HUNTER replied that it would in all probability simply have remained adherent without undergoing decomposition. He did not think it called for drainage.

DR. POLK said he had had a case which in many respects presented almost the identical features observed in Dr. Hunter's first case. A small portion of the sac had to be left. The patient died on the sixth day after the operation, of shock, and the condition found at the post-mortem showed that the result would not have been changed if a drainage tube had been introduced. The patient was older than Dr. Hunter's, being sixty-seven. This fact would probably render adhesive inflammation less likely to take place.

DR. JANVRIN had assisted at an ovariectomy about a month ago which terminated fatally. The patient was unmarried, forty-five years of age. He saw her only the day before the operation. The physician and patient stated that the abdominal tumor had been present only six months. The tumor, however, was of immense proportions, and the patient was extremely emaciated and weak. Dr. Janvrin found evidence of more or less adhesions and a large firm mass in the right *cul-de-sac* which was thought to be a portion of the ovarian tumor above. It was firmly impacted, it being impossible to dislodge or move it. The uterus was normal, and lay a little to the left anteriorly. The operation was done at St. Elizabeth's Hospital. At first an incision about two inches and a half in length was made, and the tumor found apparently to be uniformly adherent. In trying to separate the adhesions of the sac from the abdominal parietes the cyst was very slightly torn and a hole made of the size of a pin's head into its cavity. Dr. Janvrin then advised immediate tapping, which was done with a large trocar and canula. The contents were found to be colloid in character. The adhesions were so extensive that the incision was necessarily extended nearly up to the umbilicus. A great deal of difficulty was experienced in fully detaching the cyst from the parietes, principally on account of its soft and friable state. Finally it was thoroughly removed. Unavoidably some of the colloid material entered the peritoneal cavity. The entire cavity was thoroughly washed out with hot water slightly carbolized. The tumor grew from the right ovary, and, in extracting it, a small rent was made in the broad ligament from which a little hemorrhage took place. It was controlled by two ligatures. The cavity having been washed out, as before stated, and being perfectly clean, the incision was closed. The operation consumed forty-five minutes. During the operation the pulse disappeared and the patient seemed to be dying. Ten or twelve hypodermic injections of brandy were given, the feet were elevated, and, as the legs were extremely edematous, rubber bandages were sent for, with which they were wrapped, forcing the blood and serum into the body. The pulse almost immediately became perceptible, and the patient

was put to bed. Reaction took place in about three hours. The drainage-tube was left in. At the end of forty-eight hours it became evident that there was more or less serum in the abdominal cavity, as indicated by septic symptoms. The dressings were raised and a pint of serum was withdrawn. The patient lived nine days, and finally died, apparently of exhaustion, all symptoms of septic poisoning having disappeared about the fourth day. There was no further accumulation of serum. The points of interest in the case were, the immense size of the tumor, its colloid character, the extreme feebleness and almost moribund condition of the patient during the operation, and the very favorable effect which applying elastic bandages to the extremities had by forcing the serum back into the general circulation, answering almost the same purpose as transfusion, which had been suggested during the operation.

DR. POLK asked Dr. Janvrin whether he thought, in view of the extensive adhesions, the operation should have been done. He made the inquiry because he himself had been placed in a similar position, and he would like to know the opinion of others.

DR. JANVRIN replied that he had seen many cases in which the adhesions were as extensive, and yet the operation proved successful. In this case the patient was sinking very rapidly, and an operation offered the only chance of recovery; it was for that reason that he advised the doctor to remove the tumor as soon as possible.

DR. POLK remarked that, where the adhesions were extensive and firm, one could not tell beforehand how long a time the operation would require, how much handling would be necessary, nor the possible amount of the hemorrhage, and the patient's chances were consequently likely to be greatly diminished.

DR. HUNTER mentioned a case in which the adhesions were very extensive in every direction, and the patient recovered; and another of a similar nature, operated upon by Dr. Thomas, in which the adhesions extended to the diaphragm. The patient surprised everybody by making a good recovery.

DR. POLK remarked that in his experience with such cases the adhesions had been principally in the pelvic cavity, but, if they occurred higher up, and even on the diaphragm, they were less vascular, the operation was not so bloody, and occasioned less shock. Their extent, their vascularity, and the great length of time required for their separation within the pelvic cavity, and the attendant shock, made it questionable whether firm adhesions should be interfered with in this locality, especially if they were extensive.

DR. JANVRIN thought that in such a case the upper portion of the sac might be removed and the lower part, where it was most firmly adherent, be allowed to remain and drainage be established. He had done this in several cases.

DR. P. F. MUNDÉ had operated upon a patient a little more than a year ago in whom two tumors were present—one high in the abdominal cavity and the other firmly adherent to the entire pelvic cavity. No particular difficulty was experienced in removing the first tumor, except that the care with which it had to be stripped from its extensive attachments required considerable time. The second tumor was of about the size of a large coconut, had apparently developed within the broad ligament, and had to be seared off with Faquelin's cautery from the posterior surface

of the uterus down to the *cul-de-sac*. No pedicle was left. Some large arterial twigs in the seared surface were tied by deep sutures. The abdominal wound was closed and a drainage-tube inserted. Dr. Mundé had since concluded that he had closed the wound too soon; he should have waited until it was certain that every bleeding point was permanently secured, for the patient was in good condition, although the operation had lasted an hour and a half. She rallied well, with scarcely any shock. But a good deal of bloody serum escaped through the drainage-tube, and, after twenty-four hours, her condition suddenly changed, the pulse went up to 150, and symptoms of collapse appeared. In spite of all efforts at restoration, she died the next morning. A post-mortem was not allowed, but Dr. Mundé was of the impression that secondary hemorrhage had taken place, and that in similar cases it was better to prolong the operation and take the chances of shock, in order to make sure that all bleeding was permanently arrested.

DR. POLK said the question which had been in his mind was whether it was good practice to persist and break up all the adhesions where they were very extensive. It seemed to him, from the discussion which had taken place, that the method suggested by Dr. Janvrin was the proper one to pursue—namely, to remove the upper portion of the sac, if possible, and leave the portion firmly adherent within the pelvis and wash it out. He had observed the plan suggested by Dr. Mundé of persisting in removing all the sac down in the pelvis and waiting until all oozing had ceased in two cases, and he lost both of the patients from secondary shock. Not a teaspoonful of serum had exuded in either case. He believed that both patients would have been saved had he been less anxious to get away all the stump.

DR. MUNDÉ had operated upon a patient a year and a half ago, in which case the sac was so broken down that it had to be removed piecemeal. The patient died of shock. He therefore thought the suggestion which had been made, to leave a portion of the firmly adherent sac and establish drainage, a good one, as subjecting the patient to a less degree of shock. He had observed this plan in one case, and the patient recovered.

With regard to Dr. Janvrin's second case, Dr. F. P. FOSTER asked whether the immediate beneficial effect of the application of the rubber bandage was not due rather to forcing the blood than the serum from the extremities directly into the general circulation.

DR. A. JACOBI said that, while the serum could not be forced directly from the tissues in which it was found into the general circulation, it required but a short time for it to reach it indirectly, under pressure, for the lymph-ducts terminated in the subcutaneous tissue with open orifices; and, although it might have swelled the volume but slightly, it was known, from the results of transfusion of salt water, that a few ounces would have the effect of restoring the patient. The fact that the edematous limbs were found to be reduced to their normal size after removal of the bandages showed that the serum must have gone to the circulation within the body. He thought, therefore, that some of the effects were due to the return of the serum as well as of the blood to the general circulation.

Meeting, April 1st, 1884.

PUERPERAL ECLAMPSIA.

DR. B. F. DAWSON read the following history of a case: "Mrs. S., thirty-seven years old, the mother of ten children, consulted me on the 14th of March, being then in the seventh month of pregnancy. She said that she had never 'felt life' during the existing pregnancy. At some previous labor she had suffered lacerations of the perineum and cervix, which were closed by myself about two years ago. The operations were followed by perfect relief from the symptoms and by this pregnancy. She had not been feeling well for some months, and on the 10th of March she was suddenly seized with a severe chill, and had fever and obstinate vomiting, with very scanty urine. She called in a physician, who told her that she was suffering from acute Bright's disease, and gave a very unfavorable prognosis. When I first saw her she was constantly retching, was unable to retain anything on her stomach, and showed on the face, neck, and arms an erythematous blush, accompanied by sore throat and reddening of the tongue. There was no edema of the face or ankles, but there was slight pitting on pressure along the crests of the tibiæ, with a varicose condition of the veins of the legs. There was no headache, stupor, or disturbance of vision. The bowels were constipated. I gave her five grains of calomel, to be followed by a saline cathartic, and requested a specimen of the urine to be sent for examination.

"The next day Dr. Hawley examined the urine, and found little abnormal about it. The blush was not now so marked, but the throat and tongue were in the same condition as before, and the nausea was still intense. She had passed two fluid ounces of urine during the preceding twenty-four hours, and it contained about fifty per cent of albumin. I gave her a grain and a half of elaterium, in divided doses.

"On the following day she had passed no urine, and the bowels had not moved. A catheter was introduced, and one drop of dark urine was brought out in the instrument. Infusion of digitalis was given by the rectum, in doses of half a fluidounce, and drachm doses of fluid extract of jaborandi were also administered.

"On the following day she had had a small, semi-solid movement from the bowels, but no passage of urine. The catheter was again passed, but with a negative result. The pulse was 102, the temperature normal. There was no headache, disturbance of vision, or edema, but the nausea and vomiting were constant, and the throat was swollen and sore. Three drops of croton oil were given, cups were applied to the loins, and the patient was kept in a hot-air bath for the whole night.

"The next morning she was feeling better, but had passed no urine, and her bowels had not moved, nor had any perspiration been induced by the hot-air bath. About a fluidrachm of dark

urine was drawn with the catheter. She was now suffering from cramps in the legs and arms. The digitalis and jaborandi were continued, and she was quieted with small hypodermic injections of morphine.

"On the succeeding morning she was suddenly delivered of a dead child, apparently of about seven months and a half. There was no hemorrhage, and the uterus was well contracted. She had passed a small amount of urine, and felt much better. Still there were no marked symptoms of uremia. The hot-air bath was repeated at night.

"On the following day, one week from the time when she was first seen, she was found stupid and slightly delirious. The rectal temperature was 105° F. There was no odor to the vaginal discharge. The vagina and rectum were washed out with a solution of bichloride of mercury, 1-2,000, and about a fluidrachm of urine was withdrawn with the catheter. At noon she had a slight tonic spasm, and quietly died. An autopsy was not allowed."

Dr. Dawson added that the chief interest of the case related to the absence of any of the usual marked symptoms of uremia until near the close of life, notwithstanding there had been almost complete suppression of urine during the nine days of the patient's sickness. She had remained perfectly rational, and said she felt well, with the exception of nausea, up to just before death. The case would have been less remarkable had there been any vicarious action on the part of the alimentary canal or the skin. There was but very little material vomited, and it was impossible to make the skin and rectum act.

Dr. Polk said the case presented many features in common with one which he narrated at the Society about two years ago. He referred to the case in which by mistake he removed the only kidney. In that instance, however, the stomach and the intestine seemed to take up the work of the absent kidney, and, after the third or fourth day, the patient began to have about three very copious fluid discharges from the rectum every twenty-four hours, and vomited as many as five or six times during the same period, the vomited matters containing a large amount of urea. Her mind remained perfectly clear up to within twenty-four hours of death. She died, as Dr. Dawson's patient did, while in a light convulsion this being the only convulsion which she had during the eleven days of her life after removal of the kidney. He believed a number of cases had been reported in which children had lived several days, perhaps as many as fifteen, with entire suppression of urine.

RUPTURE OF THE REPAIRED PERINEUM AT SUBSEQUENT CONFINEMENTS.

Dr. H. F. Walker said that within two months past he had attended three women, multiparæ, each of whom had renewed rupture of the perineum. The first patient had had three children, and he had attended her in her first and third confinements. There was rupture of the perineum at the first confinement, which he

repaired immediately. The second time she was delivered in Philadelphia. The physician who attended her closed a rupture of the perineum, and it healed by first intention. The third child was born about six weeks ago, and again there was rupture. In the two other cases two children had been born, Dr. Walker having attended one of the patients in both confinements, and the other only in her last. In both there had been rupture of the perineum at the first confinement, which was closed, and again rupture and closure at the second confinement, the operation on each occasion being attended with success. He inquired whether it had been the experience of other members to have to repeat perineorrhaphy.

DR. J. B. HUNTER had repaired a rupture through the sphincter ani in one case, and only slight laceration took place at a subsequent labor, the sphincter remaining perfect. In three other instances he had avoided renewed laceration at confinements by retarding the progress of the head, but the perineum was not left so strong as before labor.

DR. C. MACKENZIE had seen one lady who had had rupture of the perineum at three successive confinements. The perineum was of that dark-brown color which had been said to indicate want of proper elasticity. The lesion seemed not to occur each time in the same place. Repair had been effected without difficulty.

DR. H. J. GARRIGUES had attended a very blonde lady in three confinements. In the first two a laceration of the perineum occurred, which healed entirely after primary operation; in the third no rupture took place, although the child was as large as the others had been, if not larger.

DR. POLK had recently attended a woman who was first confined about twelve months before. On both occasions there was rupture of the perineum followed by successful repair. The second rupture was not exactly in the line of the first. It might not be inappropriate to say a word in this connection about a method which he had been accustomed to employ for the prevention of rupture of the perineum. It was not new, but he did not think it had been described in a perfectly plain manner, or in such a way as to attract the attention of the reader. The method consisted in the prevention of extension of the head until the suboccipital region pressed well up against the arch of the pubes. By thus bringing the suboccipital region squarely up against the pubic arch before allowing extension to take place, we could cause the shorter diameters of the head to be presented as it passed through the ostium vaginae, relieving the perineum of pressure proportionately.

DR. GILLETTE had practised this method a good while, and had found it of great benefit in the prevention of rupture of the perineum.

DR. HUNTER said that this method had been taught years ago by Dr. Thomas, but that he (Dr. Hunter) had found it difficult to put in practice. It was not an easy matter to take hold of the head at any point and regulate the direction of its diameters. He had found the most safety to lie in retarding the progress of the head by making pressure on it or on the perineum.

DR. POLK thought this amounted to the same thing as the procedure of which he spoke.

DR. WALKER had found more lacerations occur from the passage of the shoulders than from that of the head.

DR. POLK corroborated the statement, but said that, according to his observation, when the perineum was lacerated by the passage of the shoulders, there was previously a nick in the posterior vaginal wall, which the shoulders plowed through and enlarged. In reply to a question by Dr. Mackenzie, he said that, according to the method to which he referred, pressure was made on the forehead, flexing the head, resisting extension, and thereby forcing the occiput out of the ostium vaginae before permitting extension to occur.

LARGE OVARIAN CYST.

DR. HUNTER presented a specimen which had been removed that day. The patient was thirty-four years of age. Four years ago she noticed some enlargement of the abdomen, which within the last year had increased rapidly. Dr. Hunter saw her for the first time a week ago, when he found the abdomen enormously distended by an ovarian tumor. It was a question at first, however, whether the distention was due to ascites or to a cystic tumor. On making an abdominal section, he found the sac everywhere adherent in its upper part to the abdominal walls and to the intestines. The cyst was evacuated and then enucleated, but, when the lower half was reached, it was found to be non-adherent. A small portion had to be cut away and the pedicle tied. The main pedicle was also tied and dropped back. The entire cyst, sac and contents, weighed fifty-five pounds. It was multilocular, but the larger portion of it consisted of a single cyst. The fluid was dark straw-colored. There was some oozing, and a few points were tied, but all bleeding had ceased when the abdominal wound was closed. No drainage-tube was inserted. That evening the patient was doing well. The peculiarity of the case was that the upper portion of the tumor was firmly adherent, while the lower half was free. Had this fact been known beforehand, he did not know that the operation could have been done differently.

Note by Dr. Hunter, April 14th.—The patient made a good recovery, having had no bad symptoms whatever.

PUERPERAL ECLAMPSIA.

DR. GARRIGUES read the history of a case:

The patient was twenty-one years old, unmarried, pregnant for the second time. After her first confinement, three years before, she had been insane for two years. Besides enormous edema, there was hyperesthesia of a great part of the skin. The urine contained forty-five per cent of albumin and many granular and hyaline casts. She had violent headache. She had two attacks of eclampsia. Eight ounces of blood were withdrawn, and a drachm of chloral hydrate was injected into the rectum. The external os formed a transverse slit just admitting the tips of two fingers. She was put under the influence of chloroform, the os

was dilated with Barnes' dilators without any difficulty, and a living male child weighing seven pounds and eleven ounces was extracted by turning. After delivery she had one more convulsion. The following days she was treated with the hot pack and derivatives over the kidneys and bowels, and made an excellent recovery, except that her insanity reappeared in the form of melancholia with fits of violence.

He reported the case because several authorities deprecated artificial dilatation and turning, stating that it made the convulsions worse, but he thought this teaching was a relic from the times preceding the discovery of anesthetics.

DR. W. M. CHAMBERLAIN said that, in a case of his, the eclamptic phenomena ceased as soon as dilatation of the cervix was accomplished, although delivery was not complete until seven hours later. Since then he had seen another case in consultation. He performed *accouchement forcé*, and in this instance also there were no convulsions after dilatation was considerably advanced until several hours after delivery was effected. This constituted his third case in which convulsions were recurring with considerable frequency up to the time when dilatation was secured, were then suspended until after delivery, and came on again apparently with post-partum contraction of the uterus. Two of the patients recovered; the third one died.

DR. MACKENZIE inquired whether the patients became conscious when dilatation was effected.

DR. CHAMBERLAIN replied that the convulsions ceased, but the comatose condition continued.

DR. GILLETTE did not know but the observation made was an important one, namely, that puerperal convulsions ceased with dilatation of the cervix. He had seen a number of such cases, but, on the other hand, on several occasions he had practised *accouchement forcé* with the result of intensifying the convulsions. He remembered distinctly one case in which he found the patient suffering from profound coma. Labor had just begun, and the os was dilated but very little. He effected rapid dilatation, but the convulsions continued. He could not see that delivery had any effect whatever in moderating the convulsions. The patient lived forty-eight hours. There was complete suppression of urine. He did not think there was any general rule which would cover all cases. Each case must be a law unto itself. Undoubtedly some cases were aggravated by the mechanical process of dilating the cervix. When dilatation was effected, no doubt some of the reflex irritation was removed, but he did not think that, from the observations thus far made, any proposition could be shaped regulating the management of all cases. There was scarcely any condition, unless it was placenta previa, in which there was so great a scope for the display of ingenuity as in eclampsia with commencing labor. It was perhaps the disposition of every one in such cases to hasten delivery, and yet he believed he had done injury in some instances by interfering.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, October 2d, 1884.

The President, R. A. CLEEMANN, M.D., in the Chair.

DR. B. F. BAER exhibited specimens from a case of
SUBMUCOUS AND INTERSTITIAL FIBRO-CYSTIC TUMOR OF THE UTERUS,
IN WHICH HEMORRHAGE WAS ABSENT.

Mrs. H., æt. thirty-six years, has had two children, the youngest being twelve years of age. Four years ago she suffered from severe metrorrhagia which was caused by a submucous and interstitial fibroma of the uterus. This was removed, and after passing through a severe attack of periuterine inflammation, she recovered. After the removal of the growth, her catamenial periods became regular in time and quantity, and she remained well until about one year ago, when she began to have a very fetid watery discharge in the intermenstrual periods, with sacral pain and uterine tenesmus. On July 17th, her physician, Dr. R. Armstrong, of Lock Haven, requested me to see her with him, when examination showed the cervix to be twice its normal size, with swollen and gaping lips, making the os and cervical canal quite patulous. The body of the uterus was as large as at the third month of gestation but it was not symmetrically developed, being larger on the left than on the right side. The left broad ligament was indurated and seemed to be the seat of an old inflammatory process. Pain had been present in this region since the operation. The sound met with an obstruction at the internal os and was deflected to the right, passing to a depth of nearly four inches. It could be made to pass around a mass of some kind in the cavity of the uterus, giving an indistinct sensation of the presence of an abnormal growth. I expressed the opinion that, although there had been no hemorrhage, there was a submucous or polypoid fibroma present, and advised its removal. Seven tents were introduced, and twenty-four hours later, ether was administered, when, with the assistance of Drs. Armstrong, Walls, and Ball, I proceeded to remove the tents and explore the uterine cavity. On passing my finger within the internal os, I detected a smooth oval-shaped mass of tissue resembling in consistency the inverted uterus enlarged to about double its normal size. I carried my finger up with some difficulty and found the base or attachment of the tumor to be located at the fundus of the uterus, where it was narrowed somewhat, forming a sort of pedicle. The tumor felt rather soft for a fibroma and his, together with its shape, caused me to suspect inversion of the

uterus, and when I remembered that the organ is sometimes inverted by the operation for the removal of an interstitial fibroid which requires great traction, as was necessary in this case four years previously, I became much more anxious to investigate fully before attempting to remove the mass. By very careful and thorough bimanual manipulation I convinced myself that the uterus was not inverted, there was no indentation anywhere on its surface; I therefore felt warranted in adjusting the wire of an *écraseur* around its attachment, and proceeded to tighten it, but the traction and manipulation which were necessary in placing the noose broke the surface of the tumor and exposed a peculiar looking membrane which resembled the peritoneum. I was alarmed at this, fearing that I had really to deal with a partially inverted womb and that the smooth membranous surface was the peritoneum. I removed the *écraseur*, the wire of which had broken, and then passed one finger into the bladder and another into the rectum for the purpose of determining more certainly the condition of the peritoneal surface of the uterus. Now while an assistant made traction on the supposed tumor, I was enabled to satisfy myself fully that the organ was not inverted. I then removed the tumor by enucleation.

As you will see in the specimen which I present, there are a number of cysts. These cysts contained the semi-opaque coagulable fluid usually found in fibro-cysts, and gave to the tumor its softness which, together with its shape and the appearance of the cyst-walls when its surface was broken, made it resemble an inverted uterus. The patient made a good recovery.

The case is very unusual, for the reason that, although the uterine cavity was distended by a large submucous tumor which was becoming polypoid, not the slightest hemorrhage resulted. I do not remember to have met with a similar case. I have, however, met with cases of small polypi where there was no hemorrhage, two indeed were discovered after the menopause had been fully established, and are worthy of record because of the reflex symptoms which they seemed to induce.

UTERINE POLYPUS IN WHICH HEMORRHAGE WAS ABSENT, BUT WHICH
GAVE RISE TO SYMPTOMS OF PREGNANCY.

Mrs. C. was forty-six years of age; she had been married twenty years, but had never been pregnant. The menopause had occurred one year previous to the date at which I saw her. Soon after the cessation of the catamenia, her abdomen began to enlarge, and she thought she was pregnant. Various irregular reflex symptoms of pregnancy developed, and she became so convinced that she engaged the services of an accoucheur and nurse, and went into labor in due time. Her physician, my friend Dr. John H. Musser, was unable to discover the least physical sign of gestation, nor anything else which should give rise to the almost perfect labor-like pains which she seemed to have at irregular intervals.

He informed her that she was not pregnant. She became indignant, and asked him to call another physician to confirm what he said. He consented, and requested me to see the patient. I excluded pregnancy, but found in the cervical canal a fibrous polypus not larger than an ordinary marble. This I at once removed, and the pains and other signs of gestation immediately subsided.

This was one of those cases of spurious pregnancy which we sometimes see developed in a sterile woman about the period of the menopause. The desire for offspring is strong. The cessation of the menses starts the delusion, and it is kept in existence and made to grow by being constantly fed by a morbidly susceptible nervous system. But there was a local irritation here to account for the reflex symptoms of gestation, as I believe there is in the majority of these rare and interesting cases. It is three years since this patient was under treatment, and there has been no return of the reflex disturbance.

The other case to which I wished to refer occurred in the practice of my friend Dr. B. Trautman, who kindly asked me to see the patient with him.

UTERINE POLYPUS IN WHICH HEMORRHAGE WAS ABSENT, BUT WHICH SEEMED TO INDUCE SEVERE REFLEX HEAD SYMPTOMS.

Mrs. K., æt. 52 years, has had two children, the youngest being twenty-five years of age. The menopause had occurred four years previously, and she did not complain of the slightest local symptoms of uterine disease; but the flushings and other nervous manifestations which often attend this period had not yet subsided. The disturbances, however, which concerned her most, and for which she consulted the doctor, were a pain and pressure of a very aggravated form on the top of the head. Many remedies had been prescribed for the relief of this, but with only temporary benefit. A uterine examination was made, and a polypus resembling in size a small walnut discovered in the canal of the cervix. This was removed some months ago, and I believe the patient has been relieved of the headache and other reflex symptoms which seemed to result from its presence.

The influence which these small growths have on the nervous system is something remarkable, but the absence of hemorrhage, especially in the first case, is more notable when we recall its size and location, and remember that death has resulted from the hemorrhage caused by polypi not larger than a pea, as recorded by Locock, Klob, Courty, and others. I have no doubt some of you can recall cases, as I can, where death would doubtless have resulted from the hemorrhage produced by a small polypus, had not the cause of it been removed. The following is an illustrative case:

UTERINE POLYPUS ATTENDED WITH GREAT HEMORRHAGE.

Mrs. P. consulted me on September 20th, 1883. She was thirty

years of age and has been married eight years, but has been sterile. Two years ago she began to suffer from menorrhagia with uterine tenesmus. Soon after she lost blood at irregular intervals and in large quantities. During the past year she had not often been free from metrorrhagia or a profuse and offensive leucorrhœa. The hemorrhage would sometimes last a whole month continuously, and leave her so prostrated and anemic that it was thought she could not rally. She had lost thirty pounds in weight and was blanched in appearance.

I will confess that I was surprised to find, on examination, that my patient had a polypus not larger than a Concord grape, but the mucous membrane of the cavity of the uterus was hypertrophied and granular. The pedicle was attached far up in the cavity of the uterus. The tumor was removed by means of the curette. The patient now menstruates regularly. This case contrasts strongly with the three others in its hemorrhagic character, and it presents the history commonly met with in these growths. There is no doubt that the location of the tumor has great influence in the causation of hemorrhage in these cases, much greater than the size of the growth; but much also depends upon its histological character and the condition of the endometrium. Thus, when a fibroid tumor or polypus is situated in the cavity of the uterus proper, more hemorrhage is likely to result than when it grows from the tissues of the cervix, because if located in the former position, it is often of the muscular variety, and therefore more vascular, and the mucous membrane of the uterine cavity, which is the direct source of the hemorrhage, is usually hypertrophied and granular, as in the last case narrated. Moreover, when the cavity of the uterus is the seat of a polypus, the uterine and pelvic circulation is stimulated by its presence, somewhat in the manner in which it is affected by the presence of a fecundated ovum which has been blighted; it is a foreign body, and the uterus tries to expel it, but by the effort, the circulation is excited in that direction, and hemorrhage results. My first case, however, furnishes an exception to the rule that hemorrhage attends when the tumor occupies the uterine cavity, but as tenesmus was present, it is possible that hemorrhage might have occurred later had the tumor been allowed to remain.

DR. GOODELL remarked that the question of hemorrhage in polypi is a curious one. It seems less likely when the tumor is in the body of the uterus, and checks the amount of circulation by exciting tonic contractions, than when it is protruding into the vagina like the clapper of a bell. In one case, where the hemorrhage had produced extreme anemia, dialyzed iron was given to relieve the anemia, and it also checked the hemorrhage. In another case, operation was refused, and death resulted from hemorrhage three days after the visit.

RAPID DILATATION OF THE UTERINE CANAL.

By WM. GOODELL, M.D.—For many years I enlarged or straight-

ened the uterine canal, according to the requirements of the case, either by tents or by Sims' operation, and preferably by the former. Having had several serious warnings in the shape of inflammation following these operations, I began to perform them with fear and trembling. Yet nothing very untoward happened until the year 1878, when two grievous mishaps befell me.

A charming young lady, the centre of a large number of admiring friends, came from a neighboring State to consult me about a dysmenorrhea which grew worse and worse every year. The cervix was so bent forward, and the stenosis of its canal, *per se* as well as by angulation, was so marked that I unhesitatingly performed Sims' operation. Within a few days septicemia set in, soon the parotid glands swelled up, and on the ninth day she died. True it is that at the same time two piles also were tied, but this latter operation I had and have performed so many times with impunity, that I was, and am still, disposed to attribute the blood-poisoning to traumatism of the cervix and not to that of the rectum. Hardly had I time to recover from this bitter blow, when a case of exhausting menorrhagia fell into my hands. The lady was the young bride of a husband well advanced in life, who doted on her as only old men dote on much younger wives. I dilated the cervical canal with tents and curetted many vegetations from the endometrium. A furious peritonitis set in, and in less than three days this young wife lay dead and the husband was frantic with grief.

The anguish which I felt at the death of these two ladies, and the heartrending scenes which I witnessed at their bedsides—scenes which I cannot now recall without emotion—urged me to try any remedy that gave promise of efficiency combined with greater safety. In the search for a substitute, I tried rapid dilatation, which Ellinger and others had proposed, and since that year—that *annus iræ*—I have not once performed Sims' operation for dysmenorrhea, and I have so narrowed the field for the use of tents that I now very rarely resort to them. In short, rapid dilatation has proved, in my hands, so safe and so efficient an operation that I wish to urge its claims before this Society.

The instruments which I would recommend are two Ellinger dilators of different sizes. These are the best on account of the parallel action of their blades. The smaller of these dilators has slender blades, and it pilots the way for the other, which is more powerful, and with blades that do not feather. I have had the beaks of these dilators changed from an obtuse angle to a slight curve, so that it can be reversed within the womb. The light instrument needs only a ratchet in the handles, but the stronger one should have a screw with which to bring the handles together. Lest the beak should hit the fundus uteri and seriously injure it when the instrument is opened, the blades are made no longer than two inches, and are armed with a shoulder which prevents further penetration. The larger instrument opens to an outside

width of one and a half inches, and it has a graduated arc in the handles, by which the divergence of the blades can be read off. The instruments which I now exhibit to you, and which I can recommend highly, have been made under my supervision by Messrs. J. H. Gemrig & Son, of this city.

In a case of dysmenorrhea, or of sterility from flexion or from stenosis, my mode of performing the operation of rapid dilatation is as follows: The patient is thoroughly anesthetized, and a suppository containing one grain of the aqueous extract of opium is slipped into the rectum. She is then placed on her back and drawn to the edge of the bed, the knees being supported by her nurse. The light must be good so that the operator may clearly see what he is about. By the aid of a strong tenaculum, applied through my bivalve speculum, the cervix is steadied and the smaller dilator is introduced as far as it will go. Upon gently stretching open that portion of the canal which it occupies, the stricture above so yields that when the instrument is closed it can be made to pass up higher. Thus by repetitions of this manœuvre, little by little, in a few minutes' time a cervical canal is tunnelled out which before could not admit the finest probe. Should the os externum be a mere pin-hole, or be too small to admit the beak of the dilator, it is enlarged by the closed blades of a straight pair of scissors which are introduced with a boring motion. As soon as the cavity of the womb is gained, the handles are brought together. The small dilator being now withdrawn, the larger one is introduced, and the handles are then slowly screwed together. If the flexion be very marked, this instrument, after being withdrawn, should be re-introduced with its curve reversed to that of the flexion, and the final dilatation then made. But in doing this, the operator must take good care not to rotate the womb on its axis, and not to mistake the twist for a reversal of flexion. The ether is now withheld, and the dilator kept in situ until the patient begins to flinch, when the instrument is closed and removed. A few drops of blood trickle out of the os. Occasionally a slight flow of blood will last for several days after the operation, simulating the menstrual flux. Often this flux is precipitated or renewed if the operation follows or precedes it too soon. The best time for dilatation is therefore midway between two monthly periods.

When compared with the cutting operation, this one looks like rough usage, yet the woman rarely needs more than two or three suppositories, and complains merely of soreness for one or two days. To forestall any tendency to metritis, she is kept in bed until all tenderness has disappeared. Pain is met by rectal suppositories of opium, and by large poultices laid over the abdomen. I have seen slight pelvic disturbance arise from this operation, but it has always been readily controlled and has not given alarm.

In the great majority of cases I dilate the canal, not to the fullest capacity of the instrument, but to one and a quarter inches. Sometimes in an infantile cervix which does not readily yield and

might give way, the handles are not screwed down more than three-quarters of an inch or an inch. Tearing of the cervix has happened in two of my cases. In one, that of a virgin, the cervix was split half-way down to the vaginal junction. The other case was that of a multipara, whose uterine canal had been nearly closed up by applications of silver nitrate, made by her physician with the view of curing what he supposed was an "ulceration of the os," but which was a bilateral laceration. The tissues, rendered cicatricial and brittle by the caustic, were torn by the dilator for about half an inch on the right side also. Here the hemorrhage was free enough to need styptic applications and a tampon. I could have stopped it by wire sutures, but this was not done as it would have defeated the object of the operation.

In slight dilatations, such as, for example, office treatment of antelexions and of stenosis, or for the introduction of the curette, or of the applicator armed with cotton, the more delicate instrument is quite strong enough, and an anesthetic is not needed. Sometimes, in a very sharply antelexed womb, the dilator cannot be made to pass the os externum. This difficulty is overcome by first passing in a surgeon's probe, and then, along it as a guide, the dilator.

After a forcible dilatation under ether, the cervical canal rarely returns to its previously angular or contracted condition. Since lateral extension of elastic bodies antagonizes their length, the cervix shortens and widens; and the plasma provisionally thrown out by the submucous lesions sustained by the dilated part serves still further to thicken and stiffen its tissues. In other words, the stem-like neck of the pear-shaped womb is shortened, widened, strengthened, and straightened. Hence for straightening out antelexed or congenitally retroflexed wombs, and for dilating and shortening the canal in cases of sterility or dysmenorrhea arising from stenosis or from a conical cervix, the dilator will be found a most efficient instrument. In its results it is not infallible. I have twice been obliged to repeat the operation, and would like to have done so in several other cases, had the women permitted it. In a very few instances I have been forced, as a final resort, to nick a pin-hole os externum; but I had not then learned how far I could safely stretch open the uterine canal, and the operation of dilatation was not so efficiently performed by me as it is now, through a larger and riper experience.

But it is not to cases of dysmenorrhea that I limit the operation of rapid dilatation. As stated before, I use it to stretch open the canal for the admission of the curette and of sponge-tents, or for the purpose of making applications to the uterine cavity. In cases needing the irrigation of the uterine cavity, I first dilate the canal with this instrument and introduce the nozzle of the syringe between the separated blades. This gives a free avenue for the escape of the liquid, and robs of its dangers this form of intrauterine medication. I also resort to the dilator in order to

explore the womb with the finger. For instance, in any given case of menorrhagia in which a polypus or some other uterine growth is suspected, instead of using tents, I put the woman under an anesthetic and, after the rapid dilatation of the cervical canal to the utmost capacity of the instrument—viz., one and one-half inches—am enabled to pass my finger up to the fundus. This is accomplished either by drawing down and steadying the womb by a volsella forceps, or, in thin subjects, by forcing the womb down upon the finger through suprapubic pressure on its fundus. In this way I have over and over again at one sitting discovered a uterine growth, twisted it off, and removed it. Usually in these cases I experience more difficulty in removing the polypus or other growth through the small canal than in twisting it off from its uterine attachment. It often has to be wire-drawn before it can be removed, and sometimes the os uteri has needed a few nicks. Usually, when the menorrhagia is free, the cervical tissue is so loose that there is no difficulty in the introduction of the index finger up to the fundus, but sometimes only its tip can be made to pass the os internum. Yet even this limited degree of penetration is commonly quite enough to decide the presence of an inside growth. If it is not enough, I invariably search for a growth with a small pair of fenestrated forceps and I have repeatedly seized and removed one, the existence of which was merely suspected. After such operations, the uterine cavity is thoroughly washed out with a solution of carbolic acid or of potassium permanganate. I am sorry to say that I have not kept full records of all my cases of rapid dilatation. For instance, I have never recorded those office cases of dilatation in which ether was not given. Nor has any note been made of cases in which dilatation was d under ether for curetting, for digital exploration of the endometrium, or for the removal of uterine growths. I have tabulated merely cases of dysmenorrhea in single or in married women. In the married, with but three exceptions, which will be noted, painful menstruation was accompanied by sterility. Including all the cases of dilatation performed under ether, I must have had over three hundred. I have limited myself to these cases, because the use of an anesthetic implies full dilatation—one in which serious injury, if ever, would most likely be sustained. Yet there has not been a death or a case even of severe inflammation in my practice, and the results have been most satisfactory, far more so than when the cutting operation was performed by me. The following are the statistics of my cases of dysmenorrhea:

Unmarried, 80

Married, 88

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Of the unmarried, eighteen were unheard from after the operation, leaving sixty-two from which any data could be obtained. Of these, thirty-eight were cured, seventeen more or less im-

proved, and seven not improved at all. Of these seven that were not benefited by the operation, five subsequently had their ovaries removed, one of them by another physician, and four by myself; of the latter, one died. In each one, the ovaries had become so altered by cystic or by interstitial degeneration as to make the dysmenorrhea otherwise incurable. Of the seventeen improved, there was one on whom oöphorectomy was also performed; for, although the dysmenorrhea was greatly relieved by dilatation, ovarian insanity and menorrhagia were not. The operation was a successful one, and my patient was not only cured of her hemorrhages, but she regained her reason. Out of these cases, the majority, although not wholly cured, were greatly improved. For example, one of them was formerly bed-ridden during the whole period of her menstrual flux and had then to take large doses of morphia. She also suffered at those times from hematemesis and epistaxis. Since the operation, she experiences pain for merely two hours, needs no anodyne, and has lost her ectopic hemorrhages. Her gain in health and flesh has been great. Another one, who was wholly crippled by her suffering and made nervous by the dread of them, is now a busy nurse. For one hour at every period, she suffers a great deal, but she is too much afraid of taking ether to have a second dilatation performed.

Of those cured, two had Sims' operation performed previously without benefit, and were afterwards dilated; two were dilated twice before a cure could be brought about. The history of several cases merits more than a mere allusion. The sufferings of one of my patients at every monthly period had always been great; but while she was at a boarding-school, they grew so intense as to cause furious delirium at those times. This finally developed into permanent insanity, with suicidal impulses. While in this condition, she was placed in my hands. After rapid dilatation of the cervical canal, the dysmenorrhea wholly disappeared. The exemption from pain toned down some of her more extravagant delusions, but she did not wholly regain her reason until a few months afterwards. She is now free from all menstrual pain and in the complete possession of her mental faculties.

A Hebrew lady whose health had suffered from dreadful dysmenorrhœa, was improved so much at one sitting that her physician and friends were amazed. Not long afterward he asked me to perform the same operation upon another one of his patients, who was, if anything, worse. Her sufferings were so intense that he wrote: "I fear that another period might kill her," and urged an immediate operation. The cervix in this case was conical and very dense. Fearing a tearing of the parts, I screwed the instrument slowly up to one and a quarter inches, and kept up this amount of dilatation for some twenty minutes. The cervix sustained no injury. The canal has since remained patulous and she is free from all menstrual pain.

Of the married, fifty-three were heard from. Of these, thirty-

nine were cured, ten improved, and four unimproved. Out of these fifty-three cases, nine were not in a condition to conceive, three of them from fibroid tumors, two from destructive applications of nitrate of silver to a lacerated cervix, three from being over forty-one years of age, and one from being a widow. This leaves but forty-four capable of conception, and of these, eight, or a little over 18 per cent, became pregnant. But the ratio is in fact larger, for several of my patients, fearing pregnancy, employed preventive measures after the operation. Then again, I believe that others who consulted me merely for painful menstruation have not reported their subsequent pregnancies. For instance, two months ago, I learned through the merest accident that the wife of a clergyman, whose cervical canal I dilated six years ago, has since been making up for lost time by giving birth to twins within a year after the operation, and later to several other children. She had been married eight years before she came to me, and had had her cervical canal dilated by tents, and slit up with Peaslee's metrotome by a skilful surgeon. I have also had several cases of pregnancy following office dilatations of the uterine canal in which ether was not given, and consequently the lumen of the canal was not much enlarged. But such slight operations were not deemed worthy of record, and they therefore have no statistical value.

DR. HARRIS inquired about the danger of lighting up a former ovaritis by dilatation. The operation is successful, but that is its danger.

DR. GOODELL has not hesitated to operate, but always uses opium first, and by the time the operation is over, the patient is under its influence. He keeps them in bed and under the opium until all tenderness has passed entirely away.

DR. WM. GOODELL exhibited a

DERMOID CYST OF THE RIGHT OVARY.

A saleswoman, aged twenty-seven, was obliged to give up her situation because she found herself unable to stand for any length of time. Her physician discovered a pelvic tumor and called in Dr. Goodell to see her. The diagnosis was obscure, but he leaned to a fibroid degeneration of the right ovary. The woman was otherwise well, suffering no pain whatever except when she was in the upright position. The operation was performed on September 8th, and the tumor turned out to be a dermoid cyst. Being enveloped in the broad ligament, it was removed with difficulty. It is stuffed with hair and contains a plate of bone, the sharp edge of which was readily felt per vaginam; but it threw no light on diagnosis as it was mistaken for a fibroid spur. His patient did uniformly well, and is now out of bed. He stated that in his experience these tumors are very vulnerable, and often resent even so slight an operation as aspiration; inflammation and suppuration quickly setting in. A physician had to-day brought to his office a

young woman who had been tapped last June with a trocar. Long hairs and much sebaceous matter escaped through the opening, which had not yet healed up, and it was for this reason he had been consulted. Upon passing in a uterine sound he struck a foreign body, which from its density and the sharp click it gave, he was disposed to think was a tooth. He advised dilatation of the fistulous track and the removal of the offending body.

TWO CASES OF OÖPHORECTOMY.

DR. GOODELL also exhibited the ovaries which he had removed on September 17th and 29th, from two patients who also were doing well. He stated that the amount of tissue change in these ovaries was very slight, and yet the suffering of each patient had been great. One had been an invalid for several years and bed-ridden for the past six months. She had lost much flesh and was always groaning from left ovarian pain unless under the influence of large doses of morphia administered hypodermically. The left ovary was found to be undergoing cystic degeneration, but the right one was so sound that in its removal he was glad to have the backing of Professor W. S. Playfair, of London, who was present at the operation. For he believed that in most cases needing oöphorectomy the results usually showed failures unless both ovaries were removed and the menopause established. Convalescence after the operation had been retarded by great and painful swelling of both parotid glands, which developed without any marked rise in temperature and without acceleration of pulse, and declined without suppuration—behaving exactly like mumps. This made his third case of parotitis following the removal of the ovaries. Not one had ended fatally, and from the very slight febrile movement, he thought that the complication was not symptomatic—as in blood-poisoning—but sympathetic, and that a strong kinship, recognized by laymen, existed between the sexual organs and the cervical glands. Since the operation all pelvic pain had ceased.

The other patient was a poor woman aged 30, the mother of seven children. She was sent to him by Dr. George S. Hull, of Chambersburg, Pa. Three years ago she began to suffer from double ovaralgia. The pain never left her wholly, but it began to increase in severity a week before the period, culminated during the flux, and faded off afterwards. Large doses of anodynes were also needed in this case, and she was unable to work. The case was clearly one of ovarian dysmenorrhea, and he believed she would be permanently cured.

DR. CHAS. H. THOMAS asked Dr. Goodell his experience of the result of oöphorectomy. What proportion of cases are relieved?

DR. GOODELL could not reply definitely. He intends to report his cases before this Society at some future time. In the majority of cases, menstruation ceases and that element of trouble being removed, the patient is to that extent always improved. The neur-

asthenia resulting from previous suffering may remain, but it is far more amenable to treatment after the cause has been taken away. One such case has occurred to him recently. Dysmenorrhea caused a virtual insanity with a mind constantly wandering. The removal of the ovaries at once cured the dysmenorrhea. The patient is now able again to walk and the mind is improving. The operation removes the major element.

DR. THOMAS has now in his care a case which he thinks typical. The patient is a literary woman, overworked and crushed by family anxieties and depressing emotions. He prescribed rest and feeding. Massage proved of but little use and electricity yielded negative results. Forced feeding became impossible. During menstruation she suffered for two or three hours with moderate dysmenorrhea. Signs of ovaritis developed with swelling and hardening in the right iliac region. The patient was etherized and a careful examination resulted in finding nothing materially wrong. As soon as anesthesia was complete, all the induration and tumefaction disappeared. There was an ulcer of the rectum and moderate ante flexion of the uterus. The ulcer has since been cured, but there is no sensible relief. She suffers from a violent pain in the right ovary, extending to the coccyx and across the abdomen; it is cutting in its character at all times, and terribly severe. Formerly it ceased at night, but does not now. Hypodermic injections of morphia night and morning are necessary. Riding increases the pain which often extends down the right leg. She cannot sit up long without increasing the pain, which is evidently getting worse day by day. She has been totally disabled for nearly ten months. Is this pain hysterical? Can it be relieved by oöphorectomy?

DR. GOODELL remarked that oöphorectomy is, in any case, a question requiring serious consideration. In the cases just related by him, the patients had neither the means nor the time for prolonged treatment. Whenever possible, everything should be tried before resorting to an operation. One bedridden case under his care, very analogous to Dr. Thomas', had been relieved by the long-continued use of the constant current passed through the affected ovary. A feeble current was kept up for many hours, sometimes for a whole night at a time. The patient ultimately got well, bore several children afterwards, and is now earning her living by teaching.

TRANSACTIONS OF THE OBSTETRICAL AND GYNECOLOGICAL SOCIETY OF WASHINGTON, D. C.

Stated Meeting, June 6th, 1884.

DR. S. C. BUSEY, *President, in the chair.* ●

DR. H. H. BARKER read a paper entitled

“THE PERINEUM—ITS MANAGEMENT DURING LABOR.”

After referring to the frequency of lacerations of the perineum, Dr. Barker said: “When we consider the deplorable condition of women with extensive lacerations of the perineum; the prolapsus uteri; the cystocele and rectocele, with all their annoying symptoms and complications, we cannot doubt the importance of the perineum as a supporting body to the vagina and uterus, or the sad consequences of its destruction.” He then detailed the various ancient and modern methods of supporting the perineum, and expressed his preference for that of Goodell. He thought most cases might be managed without rupture taking place, by the adoption of means to secure relaxation and dilatation, such as stretching, emollients, hot water, etc., and when these fail, resorting to chloroform, which he thought was the best remedy to promote relaxation, and at the same time retard too hasty delivery. When there was improper presentation, this should be corrected, and if the child was too large, it might be necessary to make incisions, such as the physician deemed necessary.

DR. SMITH said he had only had laceration occur in two cases in his practice. The first occurred ten years ago. Labor was slow, forceps were applied, and the perineum gave way as the head passed. He tied the patient's knees together, and she made a good recovery. His second case occurred last Sunday. The patient was a primipara, thirty-five years of age, with a tumor in the left side, difficult of diagnosis, but probably a sub-peritoneal fibroid. Labor was tardy. The os was dilated, but the head pressing upon the bladder caused such distress, that he finally gave ether and delivered by forceps. He would have waited had it not been for fear of injury to the bladder. The head was readily brought down, and he intended to remove the forceps to let Nature complete the delivery, but a severe pain came on, and the perineum tore, like a piece of wet paper, down to the sphincter. He at once stitched it up, and the patient was doing well. He had had his share of hard cases of labor, and the only precaution used to preserve the perineum was to make a sort of horse-shoe of the thumb and fingers, and gently draw the perineum back, as the head passed over it, and at the same time gently push the head under the symphysis.

DR. FENWICK inquired of Dr. Barker if, in his experience, rupture was more frequently produced by the passage of the shoulders than by that of the head.

DR. KLEINSCHMIDT said rupture of the perineum was one of the accidents not expected by the woman in labor, and no matter how blameless the physician may be, he still had to bear the onus of its occurrence. For this, if for no other reason, it would be a good thing to have a sure method of preventing it. From Dr. Barker's paper it did not appear that we possessed such a method, the percentage of lacerations being about the same under all the methods, even in that advocated by Dr. Broomall of incising the perineum. The only exception to this seemed the successful result in one thousand cases in Dr. Tunstall's practice, where the perineum was drawn back toward the coccyx by the fingers in the vagina, the muscles being tired out by this procedure. Dr. Kleinschmidt was led to believe that if the perineum was going to give way, it would do so in spite of all our efforts. Of this he was vividly reminded by a remark made by the President, in a conversation some days ago, that it seemed sometimes as if the nose of the child cut the perineum in its passage, even after safe delivery of the upper part of the head. He also thought that ether increased the risks of laceration. True, it relaxed the muscles, but it also took away their elasticity, and thus the head, forced down by undiminished uterine contractions, encountered an almost inert barrier, through which, instead of over which, it was driven. Most of the cases of laceration he had seen had occurred under ether anesthesia.

THE PRESIDENT suggested discussion of the following:

What relation does anesthesia bear to the frequency of perineal lacerations?

What relation does the position of the head bear to the frequency of perineal lacerations?

What relation does the after-coming head bear to lacerations?

DR. J. F. THOMPSON had long since become convinced that we should prevent laceration of the perineum in primiparæ by making two parallel incisions close to the raphe on either side, and thought it impossible to prevent laceration without this expedient. The question of preserving the perineum was an important one, though not for the reason stated by Dr. Kleinschmidt. We should in this connection consider the comfort of the husband, for if we consider how sutures are applied to a ruptured perineum; how the vagina is lost, in part, because even after the best operation it was not made perfect, we should use means to prevent this accident. Therefore he advocated the lateral incisions when laceration was impending. It was a simple thing; the edges brought together healed at once and left a perfect perineum. He agreed with Dr. Kleinschmidt in a want of confidence in the different methods of support.

DR. BARKER, in closing, said, in reply to Dr. Fenwick, that lacerations were more frequently produced by the shoulder than by the head. He did not think Dr. Kleinschmidt's objection to the use of ether was tenable. As to the action of the nose of the child in cutting the perineum, Dr. Goodell spoke of this and advised sliding the perineum over the bridge to prevent the accident.

INTERNATIONAL MEDICAL CONGRESS.

HELD AT COPENHAGEN, AUGUST, 1884.

SECTION FOR OBSTETRICS AND GYNECOLOGY.

Reported by LEOPOLD MEYER, M.D., Copenhagen. (Translated from the *Centralblatt für Gynäkologie*.)

(Concluded from p. 1101.)

August 12th, Afternoon Session.

President, OLSHAUSEN (Halle).

HALBERTSMA (Utrecht) read a paper on

ALBUMINURIA GRAVIDARUM.

He defends the view, long ago advocated by him, that albuminuria gravidarum depends essentially on compression of the ureters, as demonstrated by him in Volkmann's "Sammlung klinischer Vorträge," No. 212. H.'s doctrines culminate in the following three theses: 1. Facts do not warrant us in ascribing albuminuria of the pregnant woman to a reflex contraction of the renal arteries. 2. Albuminuria of pregnant women is chiefly observed where there is a disproportion between the size of the gravid uterus and that of the abdominal cavity. 3. The cause of albuminuria of pregnant women lies most frequently in tension and compression of the ureters.

INGERSLEV (Copenhagen) admits that the importance of ureteral compression, which H. has sought to establish for eclampsia, may indeed often exist in this condition. But then it would be difficult to explain why eclampsia is not of more frequent occurrence. For Stadfeldt has shown, as early as twenty-three years ago, how often there is dilatation of the ureter, especially the right, in the parturient. With still greater reserve should we accept the theory of ureteral compression for the pathogenesis of nephritis of pregnancy in general. Later investigations of this subject have shown how obscure the pathogenesis and etiology of this affection still are. The albuminuria occurring with eclampsia teaches us but little in this respect. The frequent chronic course of the nephritis of pregnancy after delivery harmonizes ill with the doctrine of a quickly subsiding pressure. On the other hand, careful examinations of larger series of pregnant women have not furnished the proof that albuminuria gravidarum occurs by preference under such conditions as favor compression and which are often present with eclampsia, as for instance, first pregnancy, hydramnios, twin pregnancy, etc.

PAUL BAR (Paris) is likewise opposed to considering eclampsia and albuminuria from the same standpoint. But the speaker is particularly unwilling to have albuminuria always looked upon as the expression of a nephritis. This is by far not always the case.

But the albuminuria which occurs during pregnancy may have many different causes, as has albuminuria in general. This fact has been taught us, among others, by later investigations on "albuminurie passagères." For this reason we cannot speak of one special cause of albuminuria gravidarum. Examination of the urine does not show whether the albuminuria depends on a nephritis or not; from the presence or absence of casts we can draw no conclusion in this respect. Most probably it is of the utmost importance that we should be able to distinguish the various modifications of albumin in the urine. Until we know more about this, we can form no opinion of the question as a whole.

HALBERTSMA desires to emphasize particularly that he, too, is of opinion that ordinarily we have not to deal with kidney disease, but only with the consequence of ureteral compression. Nor is it a congested kidney that is present, but an anemic kidney. The remark that, according to his own explanation, all women should have eclampsia is certainly not correct. Edema of the lower extremities, likewise, occurs only exceptionally, especially in cases where the tension of the abdominal wall is increased, *e.g.*, in twin pregnancy. That the explanation by ureteral compression is too simple is no argument against it, for : *simplex veri sigillum*.

A. MARTIN (Berlin) presented a paper on

DIAGNOSIS AND TREATMENT OF TUBAL DISEASES.

The diseases of the tubes, especially their diagnosis, receive too little attention in most text-books. Still this chapter is of the utmost importance, and the diagnosis is not nearly as difficult as is generally supposed. By bimanual examination, if necessary performed during narcosis, the tubes can generally be clearly palpated, particularly when they are diseased. In order to be sure that it is really the tubes which are between our hands, we must be able to feel their connection with the uterus distinctly. Among one thousand women, about sixty-three have diseased tubes. The treatment should be medical rather than surgical, for the extirpation of tubal tumors seems to be very grave. While the speaker has lost but three out of his last hundred ovariectomies, one of them from sepsis; among 18 salpingotomies for tubal disease, 5 died, and not less than 4 of these from sepsis. The results of medical and dietetic treatment, too, are often better than is generally assumed. In many cases improvement resulted, in two cases even recovery from sterility of many years' standing. In conclusion, the speaker briefly discussed tubal pregnancy. His former opposition to Veit's proposition, to extirpate the ovisac in this condition as early as possible, he has ceased to maintain, in the light of his present experience.

KUGELMANN (Hanover) in the main sides with Martin. He particularly emphasizes the frequency of parametritis in tubal disease.

KALTENBACH (Giessen) confirms the statement that normal tubes can very frequently be palpated by the combined examination under narcosis. When the tubal sacs enlarge, they appear frequently, not in the form of a club-shaped tumor, or one com-

posed of a series of separate constrictions increasing in size laterally, but it is not at all unusual that the portion of the sac which adjoins the occluded abdominal end projects prominently into the abdominal cavity, while a number of rosary-like vacuoles in immediate proximity of the uterus are compacted into an intraligamentous conglomeration which seems to represent a single tumor covered with the tense layers of the broad ligament. Of great importance for the diagnosis of pyo-salpinx are the violent attacks of colic which have their seat in the tumor and can be referred only to an organ encompassed by muscular walls.

SÆNGER (Leipzig) emphasizes the great importance, etiologically and diagnostically, of gonorrhea in reference to disease of the uterine adnexa: the views of Nöggerath on this point are far from having met the recognition they deserve. The speaker has operated four times, always successfully, for tubal disease, yet he would not call this operation salpingotomy, but *extirpation of the uterine adnexa*.

HEGAR (Freiburg) calls attention to the importance of examining per rectum. He perfected the picture drawn by Martin and Kaltenbach of the various forms of tubal tumors. Among the symptoms, besides those peculiar colicky pains observed already by Prochowick, the very prominent nervous symptoms are notable. The cause lies in gonorrheal infection, but aside from this the disease is very frequently due to an abortion, more rarely to a labor at term. These affections, when they have existed for some time, can never be completely cured, more especially because the tubal funnel is sure to become early occluded by adhesion. The operative removal of the diseased structures is therefore said to be often necessary. Nearly always the ovary is likewise diseased and must be removed along with them. The operation, however, is difficult, especially the ligations, and the elastic ligature is here almost indispensable. But the results of the operation as regards removal of the symptoms are not very favorable, inasmuch as frequently recurring inflammations may occur after successful operations, even subsequently.

SIMPSON (Edinburgh) finds it necessary in some cases to examine also from the bladder. Besides the previously mentioned circumstances which may render the diagnosis difficult, there are two others, viz., 1st, the frequency with which adhesions form between the tube and the surface of a cyst or a neighboring abscess; and 2d, the frequency of displacement of the diseased tube. The speaker reported cases in point, in which the diagnosis was exceedingly difficult. An exploratory incision is said to be permissible in such cases.

A. MARTIN confirms most of the points made by the previous speakers. Only despite Hegar's statements he must maintain that he had often obtained perfect cure.

August 14th, Morning Session.

President, BREISKY (Prague).

ENGELMANN (St. Louis) read a paper on

PERIODICITY OF SYMPTOMS IN UTERINE AFFECTIONS.

The author practises in one of the worst malarial regions, the Mississippi Valley, where malaria exerts its influence on nearly all diseases, but particularly on neuralgias, and where there is

hardly any affection which can be treated without quinine. In gynecological patients, however, the speaker has observed a peculiar form of periodicity of symptoms which are nowhere mentioned, and which are especially distinguished by the fact that they are not at all influenced by quinine or other antiperiodics, but disappear whenever local treatment has effected an improvement or a cure of the sexual affection. The author does not refer to the symptoms appearing monthly at a certain time in the intermenstrual period and dependent upon the menstrual wave, such as pains, fever, discharge, which occur month after month at the same time, most commonly from one to seven days before the onset of the catamenia or in the middle of the intermenstrual period, but which are *continuous* during their presence. The symptoms to be discussed here are such as combine the daily and monthly periodicity, unite in themselves, so to speak, the menstrual and malarial type. They appear every month at the same time, most frequently not long before the onset of the catamenia; but then, during their continuance, they return every day at exactly the same time, vanish after so and so many hours, leaving the patient free for the rest of the day. These symptoms are fever, pain in the pelvis, neuralgia and discharge, most frequently fever and pelvic pain. As has been stated, quinine does not exert the slightest influence upon them, but they yield to a local treatment. The question is, whether this periodicity in the symptoms depends upon the wave motion which we observe in other vital functions, especially in that of the nerves, or whether they are in some way connected with malaria.

KUGELMANN (Hanover), a great many years ago, had observed in a patient suffering from endometritis a neuralgia uterina intermittens tertiana which resisted quinine and arsenic, but finally yielded to a preparation of cinchona bark.

GORDON (Portland), although not residing in a malarial neighborhood, has observed the same symptoms as E., but not so violent. Inasmuch as ovulation and menstruation, it seems, do not occur at the same time, they may possibly be due to ovulation.

HEGAR (Freiburg) also points out that periodical symptoms connected with the sexual system and its functions are exceedingly frequent; withal they are variable in their nature and have different causes. We have, among others, the periodical uterine colic in cancer of the womb: intermittent colic in tubal affections; we see that inflammatory conditions become aggravated at the catamenial period, and this not alone in the sexual organs, but in more remote parts, as the conjunctiva, the mamma; in the same way, neuralgias are often dependent upon menstruation. The cause of the periodicity must be determined in each case.

ENGELMANN said he had been misunderstood altogether. He must emphasize again that he is not dealing with the symptoms appearing with the monthly type, but with those only which combine that type with the daily one—which at a certain time in every month occur daily at definite hours. Apparently, *these* symptoms have not been observed by physicians present from non-malarious regions, and hence it is most probable that they

should be ascribed to a remarkable combination of miasmatic and menstrual influences.

STADFELDT (Copenhagen) read a paper on

THE TREATMENT OF THE THIRD STAGE OF LABOR.

He had set himself the task of ascertaining the results yielded by the three chief methods—the expectant treatment, Credé's method, and the so-called Dublin grip—as it is pre-eminently these three methods around which the various views have been grouped. In the form of *expectant treatment* employed, the spontaneous detachment of the placenta, as well as its unaided fall into the vagina was waited for: but as a rule the waiting for these events was not prolonged beyond about a couple of hours. In practising the *Credé grip*, the method indicated by Credé was followed: vigorous circular frictions of the fundus uteri immediately after the birth of the child, and during a strong pain—generally the third—the placenta was expressed from the uterus and sometimes out of the vagina. The *Dublin grip* is understood variously by different authors. Most frequently the greatest stress is laid on exerting pressure upon the fundus uteri immediately after the delivery of the child. Thereby, however, the antelexion of the uterus is easily increased, and thus the escape of the after-birth from the cavity of the organ is rendered more difficult. The greatest stress was laid, therefore, on friction of the fundus uteri, as in the first tempo of the Credé grip, and after the placenta had descended into the vagina, it was removed with two fingers by conjoined slight traction on the funis. These statistic records commencing with April 1st, 1873, and the antiseptic treatment having been introduced into the maternity hospital in 1869, all labors have taken place under antiseptic precautions. Excluded from the statistics are: all early abortions, and all cases in which the removal of the placenta was indicated immediately after delivery of the child (placenta previa, sanious uterine contents, etc.). The results of the figures were the following:

	Expectant, 1780 Parturients.	Credé's Method, 1611 Parturients.	Dublin Grip, 971 Parturients.	Expectant (new series in 1884), 198 Parturients.
Hemorrhages during the expulsion of the placenta	5.8%	2.3%	1.3%	3.0%
Detachment and removal of the placenta....	1.3"	0.6"	0.8"	1.0"
Tearing off of membranes or parts of the placenta	1.8"	2.3"	1.8"	1.5"
Hemorrhage during the puerperium.....	0.8"	0.3"	0.3"	0.0"
Puerperal morbidity	24.0"	18.3"	12.0"	12.0"
Puerperal mortality.....	1.9"	0.6"	0.7"	0.5"

We see from this table that Credé's method shows favorably in

comparison with the expectant treatment. Tearing and retention of membranes and of small parts of the placenta indeed is somewhat more frequent with expression, but the excess is not large, and the dangers are certainly not great under the protection afforded by the antiseptic method. The opponents of Credé's method, on the other hand, lose sight too much of the dangers associated with the expectant method during the time of waiting, especially outside of maternity hospitals. However, the fact that the morbidity and mortality are greater with the expectant method than with the others must not be unconditionally ascribed to the waiting. In the course of years, antisepsis has been employed with ever increasing energy, and much depends hereon; had the beginning been made with Créde's method, the result might perhaps have been different. But the same is true, *mutatis mutandis*, of the statistics presented by the opponents of Credé's method, where the latter was generally commenced with. Hence, after what has been stated, there is no indication that the expectant method, with all its evils for the obstetrician and parturient, is to be preferred. Credé's method, however, requires so much accuracy and intelligence that Stadfeldt does not consider it right to instruct *midwives* in the use of the grip. But for these the Dublin grip is quite appropriate.

LEOPOLD (Dresden) fully coincides with the deductions of the previous speaker. The expectant method can hardly be carried out in practice; neither physician nor midwife has time to wait from one to two hours. After-hemorrhages, too, are more frequent with it than with Credé's method. The tearing of the membranes, which is undeniably rather more frequent with the latter, is in most cases of no material importance. Credé's method, however, must be employed neither too soon nor too late, on an average fifteen to twenty minutes post partum. Observations made during Cesarean section have convinced the speaker that the manner in which the detachment of the placenta is ordinarily represented is not correct; a retroplacental hemorrhage is not normal.

SIMPSON (Edinburgh) finds it very interesting to observe the variations in the treatment of the third stage of labor. In the last century, the too frequent manual removal of the placenta brought about a reaction; Hunter and others defended the expectant method. But when this was abused, the Dublin grip was gradually developed. Abuse of Credé's method had now again produced a reaction in Germany. The speaker had tried the expectant method, but found that it offered no particular advantages; in private practice it was unsuitable and dangerous. Dr. Barbour, of Edinburgh, had likewise studied the detachment of the placenta on two uteri removed by the Porro operation. According to these studies, it seemed as if Ahlfeld had artificially produced the detachment of the placenta described by him. Retroplacental hemorrhage plays no part in the detachment.

PAUL BAR (Paris) exhibited

TARNIER'S NEW BASIOTRIBE.

The cephalotribe is a very good instrument of demolition, but a very bad tractor. The cranioclast, on the other hand, is a good

tractor, but breaks up the base of the cranium very little (unless the face presents). The basiotribe, however, combines the good features of both instruments. The instrument consists of three parts. The cranium is opened with the perforator which is then advanced to the base. Then one blade of the forceps-like instrument is applied. This blade, however, is long enough to reach to the neck. The perforator articulates with the forceps-blade, and by a screw arrangement the half of the basis cranii between these two parts of the instrument is broken up. Then the second still longer blade is introduced, articulated, and the other half broken up. (A more detailed description is impossible without illustrations.) The whole instrument is not large, made of metal throughout, and graceful. The results, both clinical and experimental, are excellent, according to the speaker.

SIMPSON (Edinburgh) welcomes the instrument as an addition to our armamentarium, but he also recognizes faults in it. Inasmuch as two blades must be introduced between the head and the uterine walls, there is danger of injuring the soft parts of the mother, and also of the introduction of germs. Moreover, the compression in one direction would entail an enlargement in another diameter. The instruments of the future would certainly act only intracranially, as for instance the *basilyst* shown by the speaker. Whenever all the bones of the base had been torn away and broken up, the head was able to adapt itself perfectly to the genital passage, and the extraction was easier.

MUELLER (Berne) also believes that, with the new instrument shown, one main advantage of the cranioclast is lost, inasmuch as it prevents, by the application of the two lateral branches, the elongation of the head and thereby the diminution of the skull. The soft parts of the mother, too, are exposed to injuries just as with the cephalotribe.

LAZAREWITSCH (Charkow) exhibited various instruments with which the bones of the head can be torn out and the head subsequently extracted.

JENNINGS (London) finds the instrument too powerful, for the head is not to be powdered. With the cephalotribe invented by the speaker, delivery could be accomplished with a conjugata narrowed to $1\frac{1}{2}$ inches; where the conjugata is still narrower, the Porro operation should be performed.

BAR defends the instrument against the criticisms. The diminution in one direction produces only an elongation, not a lateral enlargement. The intracranial instruments intended to be screwed into the bones of the base are likewise very dangerous for the soft parts of the mother. The instrument is easily kept aseptic.

August 14th, Afternoon Session.

President, SIMPSON (Edinburgh).

LAZAREWITSCH (Charkow) read a paper on:

[THE CONGENITAL LATERAL DISPLACEMENTS OF THE UTERUS.

Congenital lateral displacements of the uterus are frequent, especially those toward the left. When they are due to a defective development of the broad ligament or of the lateral vaginal wall,

they are immutable and not influenced either by the pressure of growing tumors or by pregnancy. One result of this lateral divergence of the uterus is an alteration in the form of Douglas' pouch which becomes obliquely oval. The ovary of one side finds room enough and often drops down; but the other ovary can find no place near the uterus and rises above the pelvic inlet. If now one or the other ovary undergoes cystic degeneration, the tumor lies at first in the lesser pelvis, pressing the uterus forward and upward, or else it lies at once in the abdominal cavity above the uterus. The pedicle of the ovary which finds sufficient room by the side of the uterus is generally long, that of the other ovary is short.

The laterally situated womb cannot bend fully forward or backward, and thus lateroversions and lateroflexions arise. In such cases pessaries will be of no use because they cannot maintain their hold, owing to the difference in the length of the lateral vaginal walls. The pains at the side of the lower part of the abdomen, especially frequent during the catamenia, are often erroneously ascribed to the ovary; but they not rarely come from the laterally displaced uterus.

The influence exerted on the mechanism of labor by complete lateral displacements differs from that of the lateroversions. With the latter, the uterus preserves its form, only the direction of the axis is changed, and this can easily be remedied by a corresponding change in the position of the woman or by manual aid. But with the true lateropositions the direction of the axis is unchanged; the form, however, is influenced by the fact that the lower segment can but little if at all dilate toward the side approximated to the pelvic wall. In such cases it is sufficient during labor to introduce one finger into the os and draw the cervix to the middle of the pelvis.

LEOPOLD MEYER (Copenhagen) calls attention to the frequency of congenital difference in the length of the two ovarian ligaments. In 58 bodies of new-born children, there was found 42 times a difference in the length of the two ligaments; in 32 of these the difference was greater than one millimetre. Usually, but not always, the uterus is drawn toward the side of the shortened ligament. A corresponding difference is not found in adult women.

INGERSLEV (Copenhagen) presented a paper on

THE MORTALITY OF PUERPERAL FEVER IN THE DANISH PROVINCIAL CITIES AND IN COPENHAGEN DURING THE PERIOD FROM 1867-1883.

The speaker submitted to the Section a printed pamphlet containing the main results of his two former papers on this subject, to which are added the investigations for the years 1882 and 1883. I.'s investigations having been mentioned at the time (comp. *Centralbl. f. Gynäkol.*, 1880, No. 15, p. 355 and 1883, No. 51, p. 817, the reader is referred to the respective numbers. The results of the investigations of the last two years yield for the provincial

cities a puerperal mortality of 1 : 343 and 1 : 296; for Copenhagen, 1 : 272 and 1 : 324; for the maternity hospital, 1 : 228 and 1 : 188. These results, however, are *corrected*; that is to say, the speaker has examined all the death certificates, and in all doubtful cases of peritonitis, pyemia, phlebitis, etc., conferred with the respective physicians. The speaker emphasized the fact that on comparing the number of deaths from puerperal fever with the total number of deaths in the years from 15 to 45, the quantity of the former appears particularly alarming. But we must hail with joy the fact that the progress made in this respect in the maternity hospitals is now also shown outside of them—unquestionably due to a more general employment of antiseptic principles in private obstetric practice (especially by midwives).

BONNAIRE (Paris) communicates the results attained, in the Maternité directed by Tarnier, from a strict carrying out of antiseptis by the employment of bichloride of mercury. They may be called excellent, especially since a change was made in that the wash of all the hospitals and the Maternité is no longer cleansed together. During the last term, not a single fatal case of puerperal fever occurred in the Maternité.

PAUL BAR (Paris) exhibited some catheters devised by Budin for intrauterine irrigation. They are made of celluloid in different thicknesses, and are tubes having the form of a half-gutter, whereby a free discharge is always secured.

BREISKY (Prague) expressed himself as opposed to the practice of prophylactic intrauterine injections post partum. He avoids them as a rule, and admits them only on definite indications. The results of the Paris Maternité as described by Bonnaire are certainly brilliant, but thus far they extend over but one term. Breisky has had equally good results for some years with limitation of the intrauterine injections on principle.

KÜSTNER (Jena) likewise spoke against prophylactic intrauterine irrigations. The catheters shown by Bar, like that of Bozeman, are constructed on a false principle. The irrigating fluid finds egress too easily, and the irrigation of the uterine cavity is only partial and thus fails in its purpose.

BONNAIRE (Paris) exhibited the latest model of

TARNIER'S AXIS-TRACTION FORCEPS.

After a brief explanation of the axis-traction forceps generally, he pointed out in what respects the latest instrument had been modified. The main difference consists in that the traction rods have been detached and articulate with the proper forceps blades in such a manner that it is easy to keep the point of attachment clean. (An accurate description is impossible without illustrations.)

LAZAREWITSCH (Charkow) expresses himself against the principle of the too complicated forceps of Tarnier. As much can be accomplished with the straight forceps devised by the speaker.

SÆNGER (Leipzig) finds the principle of the axis-traction forceps correct, but thinks that a chief hindrance to the universal employment of Tarnier's instrument is to be found in the national diversity of forceps.

Simpson, therefore, has gained great merit by showing that traction rods can be applied to all forceps without changing their fundamental form. The speaker has constructed similar auxiliary apparatus for the German forceps, partly traction cords of leather, partly traction rods of steel.

SIMPSON (Edinburgh) thinks it incorrect to have the traction rods detachable on the latest model of Tarnier's forceps. *For axis traction is always to be employed with forceps extraction.* Whoever has become somewhat familiar with the axis-traction forceps will readily perceive that they have unquestionable advantages in apparently the simplest cases, for instance where the head already presses on the perineum. In a maternity hospital it may be useful to employ several models of forceps, but the practical obstetrician needs *one* kind of forceps with which he is familiar and on which he can rely.

CORDES (Geneva) made

CLINICAL REMARKS ON SOME UTERINE MEDICAMENTS.

Sulphate of *quinine* is a good "utéro-moteur" and effects the expulsion of foreign bodies, remnants of placenta, etc., from the uterine cavity. *Viburnum prunifolium*, on the other hand, is a "utéro-sédatif," to be recommended particularly in threatened abortion.

August 15th, Morning Session.

President, KOEBERLÉ (Strassburg).

HEGAR (Freiburg) read a paper on

SPAYING AS A REMEDY FOR NERVOUS AND PSYCHICAL AFFECTIONS.

A remedial effect can be secured by spaying in two ways—a degenerated organ is removed, or the climax is anticipated. The former requires no explanation. Of the anticipated climax, however, we know as yet but little; we know that menstruation disappears and that the uterus atrophies, but of the consequences produced on the whole body by the artificial climax we know so little that we can find therein no indication for oöphorectomy. It follows, therefore, from what has been said, that an indication cannot be formulated without demonstrable anatomical alteration of the sexual organs. But given a neurosis and a sexual affection, the question arises whether the former depends upon the anatomical changes of the sexual organs. In this respect, great weight has always been laid on the location of the neurosis; and particularly the so-called lumbo-spinal symptoms (sacralgia, dysæsthesiæ in the abdominal wall, in the external genital and urinary organs, in the rectum, in the hips and thighs, etc.), have been looked upon as dependent upon a sexual affection. The lumbar spine, however, can also be involved from other parts of the central nervous system and from the peripheral nervous system, and then produce a sexual malady secondarily. But at all events very marked lumbo-spinal symptoms in general point to an affection of the genital apparatus. So does the aura which often starts from

the sexual organs; or the ascending progress, in the course of the disease, of the symptoms to ever more distant segments of the nervous system. The temporal relation between the appearance of the neurosis and the sexual affection must, of course, be likewise borne in mind. The intensity of the nervous affection, on the other hand, often does not coincide with the magnitude of the pathological alteration of the sexual organs. How the latter provokes the neurosis can but rarely be definitely determined, still it can be done sometimes, for instance when the neurosis is made to disappear by the straightening of the flexed uterus. The causal connection between neurosis and genital disease having been demonstrated, it does not follow therefrom that the former will be cured by oöphorectomy. If the ovary or the tube be diseased, this might be expected with greater probability than when the uterus or the broad ligament is affected; the question then is, whether the malady will be favorably influenced by the cessation of ovulation. The indication for oöphorectomy in neuroses may be formulated as follows. With a neurosis dependent upon a pathological alteration of the sexual apparatus, oöphorectomy is indicated if this neurosis cannot be cured or materially improved by any milder measure, and if it endangers life or psychical health or prevents any occupation and the enjoyment of life. The cause of the neurosis must be completely removed, or at least a causative factor done away with, without the removal of which, cure or improvement is not to be thought of. The speaker has compiled the results of his oöphorectomies performed for nervous complaints, but has included only those cases which have been under observation for a sufficient length of time. The results are quite favorable, failures comparatively few. It is of the utmost importance to find the causes of these failures. Most frequently the result is impaired, probably, by circumscribed inflammations which existed previously or were incited by the operation. We might explain this by supposing that the nerves are irritated by these ganglia which are usually seated at the ovarian pedicle, acting centrally in about the same manner as the irritation of ovulation. Faulty cicatrizations of the ovarian pedicle may likewise act injuriously. The persons operated on, too, must remain under observation for a long time: for not only has the long-standing neurosis produced alterations which take a certain time to disappear, but the disturbances associated with the climax manifest themselves with special facility in the previously affected segment of the nervous system. Finally, the abdominal herniæ which are not quite rare must be named as causes of failure. In conclusion, the speaker emphasized the necessity of an understanding between gynecology and neuropathology—a co-operation of the representatives of these specialties.

KOEBERLÉ (Strassburg) believes that oöphorectomy is but rarely indicated in nervous affections. Neuroses are due far more frequently to social conditions, faulty education, etc., than to sexual

diseases. He should like to ask Sir Spencer Wells whether the latter had often observed neuroses in patients with ovarian cysts.

SIR SPENCER WELLS (London) said that he looks upon the combination of neuroses with ovarian tumors as an exception. Most of the patients operated upon by him offered no special symptoms from the side of the nervous system. The speaker has performed ovariectomy on four women with mental disease: in two of these no material improvement was secured. In two previously mentally sound women, disease of the mind occurred after ovariectomy. The speaker has not found any intimate connection between diseases of the mind and those of the ovaries. Hysteria in young women is based far more on faulty education than upon affections of the ovaries. To extirpate the latter, unless they are very sensitive on pressure or pronouncedly enlarged, is no more justifiable than the castration of mentally diseased men.

MARTINEAU (Paris) holds that most frequently we have to deal with a primary neurosis which is exacerbated by some affection of the uterus. If the metritis be treated, the neurosis improves or disappears.

OLSHAUSEN (Halle) finds it very difficult to decide in a concrete case whether oöphorectomy is really indicated, whether the neurosis depends on the sexual affection, and whether improvement is to be expected from the cessation of ovulation. The speaker has operated four times for nervous or psychic affections. In three cases the effect of the operation was *nil*, although in one of the cases in particular the disease seemed to be entirely dependent upon menstruation. In the fourth case, although cure did not result, there was material improvement. On the whole, the speaker believes that the indications of oöphorectomy in nervous affections cannot yet be defined.

GUSSEROW (Berlin) in the main sides with Sp. W. and O. It is of the utmost importance to know *how long* the cure persists after spaying. For a temporary improvement is often observed in the nervous and hysteric after every operation.

KUGELMANN (Hanover) has likewise had the experience that nervous diseases depend much more rarely upon the ovaries than on anemia, uterine affections, exudative processes in the parametria, etc. To consider the word *hysteria* as an indication for oöphorectomy is reprehensible.

GORDON (Portland) protests against the statement that the operation is permissible only when disease of the ovaries is demonstrable on exploration. Very often we find, in cases where the symptoms alone have led to the operation, but where a morbid state of the ovaries could not be demonstrated, that there was present pronounced disease of these organs (cystic degeneration, etc.).

PRIESTLEY (London) has never yet found the operation indicated. The disease often has its seat in the central nervous system, the sexual affection is only a local manifestation of it, and then the general health of the patient is impaired rather than improved by the extirpation of the ovaries. To remove the ovaries when no palpable alterations can be demonstrated is altogether reprehensible. But even when such changes are found, much can be done by medicinal and moral treatment. In many cases of nervous disease apparently associated with irritation of the ovaries, Weir Mitchell's treatment has done excellent service.

ENGELMANN (St. Louis) defends the operation against the attacks

made upon it. After all other resources have been exhausted, when the ovaries are diseased and appear as the central point of the entire group of symptoms, the operation is justified. The fact that large ovarian tumors do not provoke neuroses does not prove anything; very often continual irritation or insignificant pathological alterations produce the most violent nervous symptoms. In conclusion, the speaker emphasized, with Hegar, the importance of the hystero-neuroses as the bridge between gynecology and the rest of medicine.

HEGAR expresses himself very similarly to E. in reference to large ovarian cysts. The speaker, moreover, again lays stress upon the fact that the operation is permissible only when palpable alterations of the genital organs are present: it is not enough that the psychosis or neurosis exacerbates at the time of menstruation.

MARTINEAU (Paris) read a paper on

THE IMPORTANCE OF UTERINE ADENO-LYMPHANGITIS IN THE DEVELOPMENT OF THE SO-CALLED PERIUTERINE SYMPTOMS AND IN THE TREATMENT OF METRITIS.

Uterine and periuterine adeno-lymphangitis is very closely connected with inflammation of the womb. Acute or chronic metritis cannot exist without the uterine or periuterine lymphatic system participating in the inflammation of the uterine tissue. The periuterine adeno-lymphangitis forms the source of the so-called periuterine inflammatory manifestations, such as phlegmons of the broad ligament, periuterine phlegmons, perimetritis, pelvic peritonitis. To term these symptoms, respectively, adeno-phlegmons of the broad ligament, periuterine adeno-phlegmons, pelvic adeno-peritonitis, is appropriate as recalling the origin of these affections. Inasmuch as periuterine adeno-lymphangitis corresponds to uterine inflammation, everything that increases metritis will also be followed by an augmentation of the periuterine adeno-lymphangitis and the symptoms depending upon it. In the treatment of metritis it is of the greatest importance always to bear in mind the significance of periuterine lymphangitis, and especially to abstain from any energetic local treatment while the latter remains.

APOSTOLI (Paris) read a paper on

A NEW ELECTRIC TREATMENT OF PERIMETRITIS.

Electricity acts very favorably on inflammations of the periuterine tissue, not only in the chronic, but also in the acute forms, in which it is able to prevent suppuration. The faradic current is chiefly used in such a manner that both poles, very near together, attached to some kind of uterine sound, are introduced into the cavity of the womb. Only in the chronic cases can the constant current, likewise applied within the uterus, be used as an auxiliary. The results attained by the speaker with this method exclusively are excellent.

*August 15th, Afternoon Session.**President, GUSSEROW (Berlin).*

KNOWSLEY THORNTON (London) read a paper on

EARLY OVARIOTOMY.

In the first place, we must answer the question whether it is justifiable to tap an ovarian cyst. We see that men like Spencer Wells and Keith speak in favor of it, while others are against it. What advantages appertain to tapping? A few cases of cure of cysts of the broad ligament, and some still more doubtful cases of cured simple ovarian cysts. Very often, however, they refill, and cysts of the broad ligament frequently become papillomatous subsequently, and infect the peritoneum through the still open point of puncture or in some other way. For diagnostic purposes the exploratory incision is not much better. And although the experienced operator may escape the dangers of tapping, such as hemorrhage, inflammation, sepsis, etc., he cannot prevent the evacuation of the small cells of papillomata, with infection of the peritoneum and the abdominal wall. At all events it is noteworthy that Keith, who has tapped frequently, complains that so many of those cured after ovariectomy, die of malignant new-formations. The speaker himself, some years ago, removed a cyst the interior of which was covered with papillomata. Of the contents, nothing escaped into the abdominal cavity. The patient died a few months later of cancer of the abdominal cavity which had sprung, not from the incision, but from a cicatrix left by a former tapping. Altogether the speaker has performed 423 ovariectomies with 40 deaths, 12 of which were due to preceding tapping. The speaker is nearly in full accord with Stilling in that tapping is a crime; there are but few exceptions. But it is not enough to abstain from tapping; we must operate early lest the various dangers associated with ovarian cysts be developed. When should an ovarian cyst be operated upon? Not too early, while it is still in the lesser pelvis; for then the operation is often very difficult, the cicatrix in the abdominal wall does not become as solid as when the abdominal wall has been stretched, and the differential diagnosis between small ovarian cysts and small fibromata is often difficult at that period. Only when the tumor causes hemorrhages or other grave accidents of some sort should it be removed. But the tumor should always be extirpated as soon as it has enlarged sufficiently to become abdominal and stretch the abdominal wall, so that the incision is made against the cyst-wall and not the intestines. If we wait any longer, the patient is exposed to various dangers. In the first place, we have the same dangers as after tapping, viz., that the tumor, at first perhaps benign, may in time undergo malignant degeneration, and infect the peritoneum by rupture or in some other way. Among the first 400 ovariectomies of the speaker, there were 40

cases in which cyst fluid was found free in the abdominal cavity. Three of these died; of the remaining 37, 3 are now suffering from cancer of the peritoneum, 3 have died of the same disease, 1 died of general sarcomatosis, 1 with supposed malignant tumor of the brain, and 3 whom the speaker could not trace, probably have likewise died. Among the 400 ovariectomies were 34 cases of torsion of the pedicle with 4 deaths—a proportionately large number. The formation of adhesions—another frequent result of waiting—has generally no great importance for the operation; but later they often cause trouble to the patients and may also cause internal incarceration. Moreover, it is probable that the increased nutrition of the tumors due to the adhesions may give rise to their malignant degeneration. If the tumor continues to grow, the general health of the patients suffers, the abdominal organs are compressed, nutrition is impaired, etc. There can be no doubt, if we abstain from tapping and operate early, we shall improve upon the brilliant results of ovariectomy.

HOWITZ (Copenhagen) in general agrees with the previous speaker, but thinks that there are not a few cases in which tapping, as a preparation for the operation, is permissible, as in very large tumors, in order to gain time; in acute diseases; in order to enable the patient to travel to the operator, etc. In tumors where the operation is very difficult, and in cysts of the broad ligament, especially when they are largely adherent, tapping may also be attempted as a radical measure. The time for ovariectomy being ever farther advanced, the speaker fears that others would go still farther than T. and remove all ovarian tumors as soon as they can be diagnosed. In this respect he calls attention to the fact that Dr. Kaarsberg, in 100 prostitutes, had found 17 times ovarian tumors from a hen's egg to a man's fist in size: in 11 cases they were elastic and more or less fluctuating. And still, as far as the speaker knows, ovariectomy has never been performed in Denmark on a prostitute. Nor must we forget that women with small ovarian cysts may become pregnant.

SIR SPENCER WELLS (London) defends tapping in those rare cases where an ovarian or parovarian cyst, or a cyst of the broad ligament, is really unilocular. At times it is also advantageous to tap a few days prior to the operation. Besides, there are the cases, not quite rare, in which the patients object to ovariectomy and demand tapping. Deaths after tapping, at all events, are fewer than after ovariectomy, amounting to about 4 to 8%.

KOEBERLÉ (Strassburg) cautions against undertaking the operation too early. Where the abdominal wall is not tense, intestines may be encountered between the latter and the wall of the tumor. The cicatrix, too, then will be longer and not so firm as otherwise.

THORNTON shows that in the main the participants in the discussion have agreed with him. The cases mentioned by Howitz as appropriate for preparatory tapping are admitted by the speaker as exceptions; but tapping a few days prior to the operation, with a view, as it were, to divide it, is of no value. In reply to Spencer Wells, the speaker points out that the question is not that of deaths immediately after tapping, but of the dangers caused by it

subsequently and those associated with the ovariectomy possibly to be performed later.

SÄNGER (Leipzig) presented a paper on

RESECTION OF THE PARIETAL PERITONEUM.

During the radical removal of large desmoid tumors of the abdominal wall, the parietal peritoneum connected with them can be resected, while excising any size portions of the abdominal muscles, to such an extent that the margins of the peritoneal wound can no longer be brought together, so that the defect has to be covered with the skin only. This operation, first performed antiseptically, and with the formation of a very considerable defect, by Esmarch and Sklifossowsky, has also been done once successfully by the speaker. The tumor was excised, leaving the greatly thinned abdominal muscles, after double ligation in arcade form, all around, of the segments of the parietal peritoneum to be divided; a number of short drainage tubes were inserted into the abdominal cavity, the skin was drawn together and firmly stitched with deep and superficial sutures. After excision of the peritoneum, retroperitoneal emphysema is very liable to occur; this may be removed by applying a pressure bandage for some hours. Experiments on rabbits have shown that on the skin facing the abdominal cavity no re-formation of peritoneal endothelium occurs, but the intestines become rapidly adherent to it. Resection of the peritoneum will also be applicable to the extirpation of intra-abdominal tumors.

KOEBERLÉ (Strassburg) cannot help raising some objections to this procedure. The dangers of vascular adhesions are not diminished by it. The speaker thinks that enucleation of connective-tissue tumors from the muscles and the parietal peritoneum is very grave by reason of the hemorrhages occurring therewith, and he himself has cause to regret several fatal cases in similar operations.

SÄNGER does not propose to enucleate the tumors from the muscles for fear of hemorrhage, but with the tumor removes the muscles adherent to it, after ligation round about it.

GORDON (Portland, Me.) read a paper on

THE OPERATIVE TREATMENT ON HYPERPLASIA OF THE UTERUS AND VAGINA, WITH SPECIAL REFERENCE TO THE CURE OF DISPLACEMENTS.

Radical cure of a dislocation of the uterus is very rarely attained; it usually returns after removal of the pessary. In such cases we most frequently find hyperplasia of the uterus, the vaginal walls are thickened, elongated, and do not fall together as in the normal condition. Very often, lacerations of the cervix and rupture of the perineum co-exist. In that event, these alterations must be removed before we can expect cure of the dislocation (most frequently backward). The speaker excises a large wedge from both sides of the cervix and stitches the wound. If a cervi-

cal laceration is present, it is operated upon after Emmet's method. If the perineum is likewise torn, it is sutured with catgut, the uterus replaced in its normal position, and then perineorrhaphy performed. The parts may then be left undisturbed. When in the course of time the hyperplasia of the uterus and vagina has passed away, the uterus will retain its normal position, even when the pessary is removed. The patient must remain quiet for a few weeks after the operation and use vaginal injections.

MEINERT (Dresden), during the past year, has had very good results in some cases of retroflexion of the uterus, in which no pessary proved sufficient, with sewing a gutter of hard-rubber to the rear surface of the posterior lip and, after the most thorough reposition possible of the organ, placing the posterior bow of a lever pessary into this groove. In a few other cases, equally successful, an intrauterine stem was sewed in at the same time.

ENGELMANN (St. Louis) has likewise witnessed excellent results in retro-displacements from the above-mentioned operations on the cervix; the results were so good that frequently he found it unnecessary to operate on the lacerated perineum. The wedge-shaped excision devised by Schroeder was equally effective with that of Gordon. If there is the least laceration of the cervix, Emmet's operation must be performed. After the operation, the speaker does not insert a pessary, but tampons of borated or iodoformed cotton; but subsequently, when the patient resumes her occupation, he inserts, as a precautionary measure, a pessary which is to be worn for one or two months.

August 16th, Morning Session.

President, VENISELOS (Athens).

WERTH (Kiel) read a paper on

THE OPERATIVE TREATMENT OF EXTRAUTERINE PREGNANCY.

In the first months of extrauterine pregnancy, besides the killing of the embryo, its operative removal likewise comes in question. Operation is indicated not only when the ovisac gives rise to dangerous symptoms, but whenever the anatomical relations can be ascertained with sufficient clearness and show the possibility of removing the ovisac. Where the pregnancy is farther advanced and the fetus living, however, the operation shows a fatal prognosis for the mother (17 cases with 15 deaths) unless—which is very rarely the case—the entire ovisac can be extirpated. Hence all operative interference should be rejected; for the dangers impending from the expulsive efforts following the death of the fetus can generally be controlled by appropriate measures. But how long shall we remain inactive? Shall we wait for grave symptoms such as sloughing, spontaneous elimination, etc.? This is assuredly not correct, for the dangers threatening the mother by a dead fetus are always very great. We must wait only until it is probable that the circulation in the placental vessels has ceased, that is, about ten to twelve weeks after the death of the fetus. Of 48

operations performed in from six weeks to one year after the death of the fetus, only 13 terminated fatally (4 of these from diseases independent of the operation). On the other hand, of 15 cases operated on within the first six weeks after the death of the fetus ("intermediary operation"), 11 died. As regards the mode of operation, the ovisac, if pedunculated, is to be extirpated intact. Although recent experience has demonstrated that full-term tubal pregnancy is much more frequent, and abdominal pregnancy much rarer, than had been hitherto believed, still the feasibility of extirpating the closed ovisac is seldom present. As the rule, therefore, stitching of the ovisac into the wound is to be recommended. If we are convinced that the placental circulation has ceased, the placenta should be removed. Unless decomposition of the contents of the ovisac pre-existed, neither drainage toward the vagina nor permanent irrigation is required. Both of these procedures must be unconditionally omitted, as well as irrigation and prophylactic tamponade of the ovisac, where the operation was performed while the fetus was living or shortly after its death, and where there is danger of artificial or spontaneous detachment of the placenta with impending fatal hemorrhage. On the other hand, it is advisable under these circumstances to keep the placenta aseptic by some appropriate substance (equal parts of tannin and salicylic acid, according to Freund). The vaginal operation is but rarely indicated.

KOEBERLÉ (Strassburg) likewise warns against operating too early. After the death of the fetus he has operated five times with but one death. The placenta should always be left behind.

P. MUELLER (Berne) reports three cases of extrauterine pregnancy in which he was forced to operate. The speaker is also of opinion that a late operation offers a better prognosis, but he would object to wait always until the death of the fetus, for meantime ruptures and other accidents might happen which would be equally dangerous as early operative interference.

OLSHAUSEN (Halle) reports a similar case in which the fetus on examination appeared extraordinarily large, but the heart-sounds had ceased for some time. The woman felt perfectly well, but died suddenly, two weeks after the examination, with symptoms of internal rupture.

SÄNGER (Leipzig) spoke of pregnancy in a rudimentary cornu ("gynatretic pregnancy"). He had collected the cases in question: 21 ended fatally by rupture in the first six months; in 3 a lithopedion formed, of which one had been successfully operated on by Koeberlé. In 1880, 1881, and 1882 there was one case in each year, by Salin (recovery), Litzmann-Werth (fatal), and Säger (recovery after laparotomy and extirpation of the cornu) respectively. It is not improbable that several of the cases diagnosticated as tubal pregnancy were really in a rudimentary cornu. Säger believes that the diagnosis here is quite possible, which is of importance, before sacrificing the child's life by waiting. In conclusion, the speaker reports a case of Porro operation in missed labor and multiple myomas, where pregnancy in a rudimentary cornu had been diagnosticated.

WERTH would not always leave the placenta behind, because it

might be detached in appropriate cases without injury and without hemorrhage. The speaker does not underestimate the dangers of waiting, but he believes that they are not as great, with careful supervision and appropriate dietetic treatment, as those serious ones encountered in operating when the fetus is living.

KOEBERLÉ (Strassburg) read a paper on

A NEW METHOD OF PERINEORRHAPHY.

If perineal lacerations do not extend into the rectum, they generally heal readily. It is otherwise where the rectum is lacerated to a depth of 3 to 5 cm. The difficulties are due to the fact that the rectum is dilated by the column of feces, owing to the contraction of the anus. Various authorities, therefore, have proposed to incise the sphincter, some laterally, some subcutaneously, etc. The speaker cuts directly backward, but so high up that the incision extends beyond the lacerated circular fibres of the sphincter. The result is a large, triangular, funnel-shaped opening posteriorly, through which the feces pass freely. The immediate closure of the severed portions is prevented by cauterization with chloride of iron and the insertion of some charpie. After two weeks, when the perineal laceration has firmly united, the posterior incision is allowed to cicatrize.

HEGAR (Freiburg) believes the posterior incision of the sphincter to be absolutely necessary, as the advancing fecal masses are too liable to damage the wound. But it would seem that the cauterization with chloride of iron is not required, nor any other measure to keep the wound open. After two or three stools have passed, there is no more danger. But care must be had that the first stool occur early, on the fourth day, and that it be fluid.

ABSTRACTS.

1. Kuestner: Hydramnios in Twin (Single Ovum) Pregnancies (*Archiv für Gynäkologie*, XXI., 1).—Seven years ago, K. published (*Archiv für Gynäkologie*, X., S. 134) a case of this kind in which, at the autopsy, one fetus, the larger, presented hypertrophy of the heart, implicating alike both ventricles, cirrhosis of the liver and ascites (150 ccm.), and in which the other and smaller fetus presented simply the signs of asphyxia. He thence deduced the following explanation of the hydramnios: the disease of the liver was primary, and, as a result, both the venous and portal circulation was interfered with. Since the portal vein enters as a factor into both the fetal and placental circulation, stasis in both these systems was induced. To overcome this, the fetal heart hypertrophied; when this hypertrophy no longer sufficed, transudation from the portal system necessarily followed, whence the fetal ascites and the extra-fetal hydramnios. Such was the obvious sequel of events. The

following case, observed in 1880, agrees in many particulars with his former case, and helps to throw additional light on the subject of hydramnios.

N., æt. forty, married thirteen years, XIpara. Last menstruation in February, 1880. About the middle of May, her abdomen began to increase suddenly in size, and she felt life. Beyond difficult micturition, had no special symptom. Entered hospital last of June. Abdomen measured, standing, from symphysis to umbilicus, thirty-eight cm.; from the same to upper limit percussion dullness, sixty-three cm.; and to xiphoid cartilage, seventy-three cm. Her girth at umbilicus was one hundred and twenty-four cm. Fetal heart to be heard in left iliac region, one hundred and forty-eight to the minute; external os open to one finger; no edema of lower extremities; complained of shortness of breath. Patient had infrequent pains up to June 28th, when membranes ruptured and an enormous amount of liquor amnii escaped, followed by a fetus, head first. A short time after, the second membranes ruptured, very little water escaped, and a second fetus, transverse presentation, was born. The first fetus lived one quarter-hour; the second was born dead. Both were female. The first, from the hydramniotic sac, measured thirty cm. from vertex to heel, twenty-one cm. to breech, seventeen cm. to umbilicus; its liver was enormously enlarged; there were a few teaspoonfuls of ascitic fluid in the abdominal cavity; the umbilical arteries widely dilated, also the vessels and perivascular spaces of the liver; no increase of connective tissue in liver; atelectasis of lungs; hypertrophy of the muscularis of both ventricles; the dimensions of the heart being thirty-five mm. in length, thirty-seven in breadth; the liver ninety mm. broad, its right lobe sixty mm., and its left sixty-two mm. in length. Nothing noteworthy about second fetus, except absence of any cause of death. The after-birth weighed four hundred and ten grms.; the placenta oval, the chorion with soft and edematous cotyledons. The placental attachment of cord from first fetus was central, from second velamentous. The amount of amniotic fluid was estimated to be about fifteen thousand ccm., and contained .07% urea.

The results from this autopsy, compared with those from the first case, agree in the following particulars: Fetuses (in each case) of same sex, and differing in development; the smaller fetus relatively sound, the larger presenting great pathological changes affecting the heart and the liver. The hearts alike hypertrophied to a size similar to that of a four months' child (weight sixteen hundred and fifty grms.). There existed ascites in both fetuses—a greater amount (one hundred and fifty grms.) in first reported case than in second (a few teaspoonfuls). It is in the macro- and microscopic appearances of the liver that the cases differ. In the earlier case we have the gross and minute appearances of cirrhosis, that is to say, the organ was small, bloodless, its circulatory system compressed by new connective tissue; in the second case, the liver was enlarged, its blood-vessels and perivascular spaces widened, presenting, in brief, appearances such as, in the adult, precede the cirrhotic liver, and which are the accompaniments of any condition which prevents the free circulation through the portal system. In both conditions the effect is identical—a stasis of blood in the venous system lying between the liver and the placenta.

What now is the causal relation between the fetal condition and the excessive amount of amniotic fluid? There existed venous stasis and

hypertrophy of the heart. Each of these conditions are of worth as productive factors. K. pointed out, in the report of his first case, how hindrance to the portal circulation, through connective-tissue formation in the liver, might cause both fetal ascites and intra-amniotic exudation. The fluid in both cases resulted from heightened tension in the portal system; and, the umbilical vein being simply a branch of the portal, high tension in the latter means the same in the former. It must be granted, then, that a part of the amniotic fluid resulted from high tension in the venous system. Fehling's observations have already shown how twists in the umbilical cord are likely to be accompanied by an increase in amniotic fluid. As for the fetal ascites, it was greater in the first reported case than in the second. This is what should be expected; for, obviously, venous stasis will be greater and tension higher in cases of contracted liver than in hypertrophic. Whilst transudation will account for a portion of the amniotic fluid, it will not for all; for it must be remembered that as high as .07% urea was determined in the fluid. In short, a greater proportionate amount of urea existed in the amniotic fluid of this case than what is found in the same fluid under normal conditions. The inference, then, is that there was greater secretion of urine, and for this we must look to the greater amount of work done by the hypertrophied heart. And so we have a double explanation of the hydramnios—venous stasis on the one hand, and hypertrophy of the heart on the other. Another question arising is as to the causal relation existing between the disease of the liver and the hypertrophy of the heart. Either the hypertrophy was the immediate result of the venous stasis, or else, existing primarily to a degree, the stasis simply intensified it. This question is not dogmatically answered by K. Sufficient to state that a portion of the hypertrophy was probably dependent on impediment to the free circulation of blood between it and the circulatory system of the placenta. Without following K. closely in his argument, it is also sufficient to recall the fact that the circulatory systems of one-ovum fetuses are closely connected by anastomatic branches; that ordinarily, hence, one fetus has as free blood supply as the other. Let, however, one fetus, for some reason or other (stronger heart-beat, greater arterial contractility, etc.), gain the upper-hand over the other, and it is obvious that it will thrive better than the other. This in K.'s second case the fetus from hydramniotic sac had succeeded in doing, as shown by the fact that the greater part of the placental circulatory system supplied the circulatory system of this fetus. Its heart then had increased work to do, and, as one result, hypertrophied.

To make still clearer the developmental course of the phenomena in this case, it is to be remembered that the cord from the larger and hydramniotic fetus was inserted centrally, whence both afflux and reflux of blood through it was freer than through the cord of the smaller fetus which had a velamentous insertion. The first fetus, therefore, was, from the start, stronger than the second; it thence gradually encroached on the circulatory field of the second; more work, hence, was thrown on its heart. This, in consequence, hypertrophied, and there resulted a greater urinary secretion in its amniotic sac. The heart, in time, became insufficient; there resulted higher arterial tension and venous stasis, whence increased transudation into the amniotic sac. As a result of long-continued venous stasis, congestion of the liver ensued, as it analogically does

in extrauterine life (this congestive hypertrophy later passes into cirrhosis). This necessitated higher pressure in the umbilical veins, and there followed increased transudation into the amniotic sac.

From these two reported cases where different stages of the same process exist, it is evident that a still earlier stage is yet to be described. Further details may be drawn from other autopsies and thus further light thrown on the pathology of the fetus. Already, for instance, it is interesting to note the strong analogical relation existing between these cases of hydramnios and the formation of acephalous, acardiac, etc., monsters.

There follows a review of the literature of and reference to the reported cases of hydramnios of one amniotic sac in cases of twins developed from one ovum.

E. H. G.

2 R. Lumpe: The Duration of Labor (*Archiv für Gynäkologie*, XXI., 1).—Opinions as to the duration of labor differ widely, for the reason that it is difficult to lay down any rule, applicable to every case, whereby its exact beginning can be determined. One case differs so much from another that only a mean can be drawn, and this mean will vary with the observer, because symptoms which for one man belong to the beginning of labor, for another are the accompaniments of pregnancy. The controversy over the behavior of the cervix towards the end of pregnancy, which began in this century and is not yet at an end, has not tended to throw much light on the duration of labor. To say dogmatically that labor ends with the expulsion of the placenta, and begins with the obliteration of the external os, does not leave much room for individual judgment, since cases differ so widely. L. hopes to lay down a rule more generally applicable, or at any rate to come nearer a solution of this question. The first effective pains which usher in the beginning of labor differ with the woman. One patient will make a great deal of simple false pains, another will go about her usual work without making the least complaint, and yet she will be really in labor. Not infrequently the rupture of the membranes will be the first sign of oncoming labor, and, on examination, the os will be found widely dilated and the head in the vagina. Thus do the pains differ in intensity and in worth. L. has noted the time elapsing between the beginning of sensible pains and the end of labor in one thousand and forty-five normal primiparæ. The mean for this number of cases is sixteen and one-half hours—a result practically in agreement with that of other observers. (The results of many are here cited.) The beginning of labor cannot be dated from the simple obliteration of the cervical canal. The softening and obliteration of this canal is an organic precursor of labor, not depending at all on pains or on uterine contractions. It is universally granted that the uterus by no means plays a simply passive rôle during pregnancy; it is not mechanically distended by the growing ovum. On the contrary, the growth of the ovum accompanies the growth and hypertrophy of the uterus. When the harmonious relations existing between ovum and uterus are for any reason interrupted, contractions set in and the pregnancy is ended. (Witness the hyperdistention of the uterus in hydramnios, where abortion usually results; witness, also, the effects of syphilis of the mother on the ovum; also, the result of interference with uterine development exerted by tumors, exudations, etc.) The ovum grafts itself at one point of the uterine mucous membrane. This membrane of the uterine body becomes the decidua. Generally, the mucous membrane of the cervix

takes no part in this decidual formation. Does, however, the cervical canal become obliterated during pregnancy or first before labor? On theoretical grounds, it is not supposable that during pregnancy (and the end of pregnancy is no exception) any change can take place in the relations existing between the ovum, uterine body, and cervical canal. Any such change will only follow on displacement between the ovum and uterus, and such displacement can result only from contractions. In every case, without exception, the tendency is for the cavity of the uterus and the cervical canal to become an ovoid cavity. The rôle played by the cervix both in pregnancy and labor is purely a passive one. In the fortnight preceding labor, the cervix becomes edematous, and this edema is to-day the index of approaching labor. The existence of this edema is the reason why in most primiparæ the finger can, towards the end of pregnancy, traverse the cervical canal and reach the membranes. The distinction must be made and recognized between patency of the cervical canal and its obliteration. The first is simply the result of the softening process (so-called edema); the second follows on contractions which necessitate a change in the relations existing between ovum and uterus. The contractions drive the ovum against the lower pole of the uterus, and thence, in a longer or shorter time, labor must ensue. The interval between the pains which cause the obliteration of the canal and those which cause the birth of the child may be a long one. Its length depends, on the one hand, on the intensity of the pains, and, on the other hand, on the resistance to dilatation offered by the cervix. Why one woman should have strong pains, another weak, another none at all, is beyond our ken. It can happen at any period of pregnancy, through some known or unknown cause, that labor will seem to be impending; but, unless the cervical canal become obliterated, labor will not take place. Once the canal obliterated, labor will take place, and the interval between this obliteration and the birth of the child may be hours, may be days. Normally, during the course of pregnancy, uterine contractions exist (Braxton Hicks). But these have no other effect on the cervix than a preparatory, a softening one. It is true that not infrequently by primiparæ energetic pains ensue which in a short time, perhaps in a few hours, open up the cervix, so that the examining finger may pass, and that then the pains cease, and one or more days elapse before labor. And this occurrence gives rise to the logical statement that there are cases where the cervical canal becomes obliterated during latter part of pregnancy. L., however, thinks it as rational to suppose that this obliteration is really the beginning of labor. It may, therefore, take hours, or it may take days for the cervix to open wide enough for the fetus to pass. Labor does not simply consist in the expulsion of the child. Its stadium begins, logically, in the opening and obliteration of the cervical canal. This opening of the cervix, if it take days, must not be considered pathological. L.'s observations have taught him that eight or more days may be required; and this was true both of pathological and physiological cases, for the simple fact of duration involved no bad after-effects.

The knowledge of where labor actually begins is of the highest importance to the accoucheur. He will thus derive knowledge as to when interference is called for and when it is not. Rarely is the premature rupture of the membranes, dilatation and incision of the cervix, called for. Many cases of the kind will, of course, suggest themselves. It is

because indications for action are not understood, that the number of women suffering from chronic disease is on the increase—women whom even modern gynecology can no longer help.

To resume in a few words : In primiparæ the opening of the cervical canal begins from eight days to a fortnight before the beginning of labor, preparatory to it, and is solely a consequence of softening of the cervix, the so-called edema of pregnancy (*Schwangerschaftsödem*). The stretching is brought about by uterine contractions which, in longer or shorter time, end in labor; and that the interval between contractions and the onset of labor may be long or short is to be emphasized. These are the deductions L. has reached from close observation of one hundred primiparæ for from eight to fourteen days before labor. The cases were all normal, and fifty out of the hundred are tabulated. The cases were not picked, but taken in order of entrance at the second clinic in the Vienna hospital.

E. H. G.

3. Breus: The Diaphoretic Treatment of Puerperal Eclampsia by means of Hot Baths (*Archiv f. Gynäkol.*, XXI., 1).—In the *Archiv f. Gynäkologie*, XIX., 2, B. reported six cases of eclampsia treated by hot baths, with the result of five recoveries and one death. In the present paper, eleven additional cases are reported with similar mortality. The treatment consists in placing the patient in a bath (38° C.) raising the temperature gradually, then wrapping her in blankets, thus securing profuse diaphoresis. Of these eleven cases, four had convulsions at the beginning of labor, two during the first stage, one during delivery, and four after. In the majority of cases the eclamptic seizures were severe. In the fatal case, the issue was more probably dependent on peritonitis, possibly septic, than on the convulsions. The two series together, then, afford a total of seventeen cases with but two deaths—one of these latter not from convulsions. In eclamptic seizures, the danger to life depends only indirectly on the convulsions. The main danger lies in the altered characteristic of the blood—hydremia—and the consequent albuminuria and anasarea. These accompaniments of Bright's can obviously best be met by profuse diaphoresis—a treatment which, whilst it can have but little effect towards curing the kidney trouble, undoubtedly relieves the symptoms which are threatening the life of the sufferer. From the use of this hot-bath treatment B. has seen nothing but good effects. It is not apt to cause either abortion or premature labor, or hemorrhage. On the contrary, he would advise the use of hot baths at any time of pregnancy when the presence of albumin in the urine and co-existing edema causes apprehension of ill; indeed they are indicated as a prophylactic measure. A striking case is recorded when a patient, æt. 26, 0para, highly dropsical, with a large percentage of albumin in urine, was subjected to the hot-bath treatment at the eighth month of pregnancy, went to term, was delivered of child weighing 2700 gm., receiving during this interval forty-five baths, with positive good effects both as concerned her own condition, the continuation of the pregnancy, and the safety of the child.

The general conclusion to be drawn from this paper is, then, that we have in this treatment the best possible method for cases of the kind. It answers where eclampsia exists, and also in those cases where the occurrence of eclampsia may be apprehended.

E. H. G.

4. Krukenberg: Gangrene of Myoma of the Uterus during Pregnancy (*Arch. f. Gynäk.*, XXI., 1).—This occurrence during the lying-in period is not uncommon. During pregnancy, however, owing to the great afflux of blood to the genital organism, these tumors almost invariably increase in size, their degeneration being a rarity. K. reports here a case of the kind, which in brief is as follows: Patient *æt.* 43, married two years, *Opara*, general health always good, menstruation regular, latterly more profuse. Early in April, patient remarked that the abdomen had increased in size, and considered herself pregnant. On June 10th, had a chill, accompanied by pain above the symphysis. She took to her bed, and remained there till the end of June, her physician making the diagnosis of pelvic peritonitis, located on the right side. It was in this locality the pains were most intense, running down the right thigh. She entered the Bonn clinic July 20th, and was then badly nourished and anemic, heart and lungs sound, liver and spleen pushed upwards, but normal; the abdomen prominent below, more on the right than the left. Palpation and percussion revealed absence of ascites; two tumors in the lower part of the abdomen, of which the right reached the umbilicus and was hard, whilst the left was two fingers broad, not extending so high, and elastic to the touch. Both tumors flat and uneven. A third, the size of a walnut, occupied the middle abdomen, hard, and with slight unevenness. On auscultation, uterine souffle heard once in right hypogastrium. No fetal heart. Vaginal examination revealed the cervix high up and backwards, faint ballottement on left. From the history and examination, Professor Veit was unable to make a certain diagnosis. There existed, probably, pregnancy between the fifth and sixth month. The left tumor was probably the gravid uterus, the right a myoma. If so, then how account for her previous illness? For an interstitial fibroid to become acutely inflamed during the fourth month of gestation was a great rarity. More likely, then, this tumor was ovarian, which, from torsion of its pedicle perhaps, had degenerated. This supposition did not accord, however, with the findings, the tumor being dense and hard. The diagnosis, then, was left open, and the future history of the case was that, on the 1st of August, the left tumor had risen to the umbilicus, the right was unchanged in position. The left tumor had also increased in breadth, and the fetal heart was to be heard over it, and by pressure, the fetal head could be felt over the cervix. The *status* of the left tumor was thus settled. The question of the nature of the right had still to be left open. On the 10th of August, the fetal heart ceased to be heard; the patient's temperature, which had always been irregular, though usually higher than normal, remained the same, though, whilst she had usually had slight chills with the evening rise, they now increased in intensity. She became stupid, the pulse irregular and small, coated tongue, cyanosis, urine cloudy and diminished in amount; on August 10th, small bed-sore. The night of the 11th, a sharp chill: one hour later, temperature 41.1 C. By vaginal examination, cervix shortened, os closed. Veit determined on an exploratory incision. An opening 12 cm. long was made on the right side, between umbilicus and anterior superior spinous process. The result was negative, only a few teaspoonfuls of clear fluid were obtained; a distinction between right and left tumor was impossible, the third tumor determined as a small fibroid. The opening was sewed

up. At seven o'clock the evening of August 12th, labor pains set in and in eleven hours a well-formed, dead, not macerated fetus was born. The abdomen still large and sensitive after delivery; to the right of the uterus and connected with it, a hard tumor. The following day patient vomited occasionally. Complained of much pain; slightly delirious. The day after, August 14th, herpes labialis, and on 16th, left-sided parotiditis. The temperature varied between 35.7 (Aug. 13th) and 39.4 (Aug. 17th). The pulse between 90 and 130. Death the morning of the 18th.

Autopsy.—In left parotid, yellow miliary foci. Heart and lungs and brain normal. Purulent matter in abdomen. Intestines bound together by fibrinous adhesions. From the pelvis rose a flat tumor as large as a fetal head, covered with fibrino-purulent patches. Ovaries and tubes hyperemic, left ovary containing corpus luteum. The uterus lay to the left of the tumor, in intimate connection with it. Just above os internum, on the right, existed an opening into uterine cavity. The tumor was so degenerated that it could not be kept for further examination. The clinical history was thus explained by the autopsy. At the fourth month of pregnancy, from disturbance of nutrition or some irritative process, an interstitial myoma began to degenerate. The inflammatory process spread to the right parametrium, and there remained localized till after labor, when it became generalized. From the post-mortem finding, it is likely that, before labor, the purulent foci had engrafted themselves at that part of the uterus where the connection between its cavity and the tumor existed, and that the formation of this connection had brought on labor. That the exploratory incision likely enough had nothing to do with the onset of labor is proved by the fact that the vaginal examination before the incision pointed to imminent labor.

E. H. G.

5. Carrard: A Contribution to the Anatomy and Pathology of the Labia Minora (*Zeitsch. f. Geb. und Gyn.*, X., 1).—Medical literature contains but little in regard to the histological structure of these organs. Some authorities contend that in structure they are mucous membrane; others that they approximate histologically the cutis. There is equal controversy in regard to the nerve terminations of the parts. As to the pathology of the labia minora the most common affection is hypertrophy following usually on either pruritus or excessive onanism. The cause of the pruritus is not well understood, by some being considered as dependent on diabetes mellitus, by others (Hildebrand, for instance) as due to a dilatation of the capillaries. C. has made a special study of the healthy labia, as well as of two specimens of hypertrophy. One of these specimens came from a patient suffering from pruritus, and yet examination revealed no dilatation of the blood-vessels. The method pursued by C. in the preparation of his specimens was the following: The sections were washed in water; left for twenty-four hours in a 6% solution of common salt, then for ten minutes in a 10% sol. of formic acid, washed carefully in water, and, according to the thickness of the section, immersed for from one-half to three hours in a 1% sol. of the chloride of gold and sodium, apart from the light. Again washed in water and left for twenty-four hours in a 10% sol. of formic acid. The gold is thus completely reduced, and the section, after washing in water, can be mounted in glycerin. His conclusions from thorough microscopic examination of the healthy labia are, in brief: The labia minora in all their essential

characteristics resemble the cutis (well-developed papillæ, sebaceous glands, the character and arrangement of the epithelium). On the papillæ regularly formed tactile corpuscles of Meissner are found, such as hitherto have only been determined on the palm of the hand, sole of the foot, edge of the eyelids, male and female nipples, the clitoris, volar surface of the fore-arm, and red border of the lips. As for the pathological specimens, the results in each were the same—a hypertrophy of the connective tissue with included nerves, and a marked increase of the latter in coarse and fine bundles, from three to four times the normal number. Three forms of nerve terminal corpuscles are distinguished. 1. Meissner tactile corpuscles, both at the apex and base of the papillæ. 2. Globular terminal bulbs, such as have been described in the conjunctiva for example, not to be found in healthy labia. 3. A form not as yet described in man, similar to those determined by Ihlder as existing in the tongues of birds. They would appear to be a transitional form between the two preceding varieties. The above microscopic findings concerning the nervous supply of hypertrophied labia will account very well for the symptoms accompanying the affection. They are also interesting for another reason. The formation of tactile corpuscles has been described by Krause and Langerhans as taking place from the earliest period of fetal life up to the seventh month of the fetus. No new ones are formed after birth. In hypertrophy of the labia we have to do not only with an increase in the number of nerve fasciculi, but also, at least from a pathological standpoint, with a passive new-formation of terminal organs. Finally, C. has determined the presence in hypertrophied labia of adenoid tissue on the surface, lying uniformly in the neighborhood of the sebaceous glands. It is possible that the accumulation of this adenoid substance contributes to the uneven appearance of the surface of hypertrophied labia. As to the pathological significance of this adenoid material C. confesses himself in the dark.

E. H. G.

CORRECTION.

IN Dr. Goodfellow's article on Rupture of the Uterus, in the September, 1884, number, on p. 931, line 20, the word 'serenity' should read *severity*.

OMISSION.

IN the report of the recent meeting of the American Gynecological Society in the October number, the election of Dr. G. Granville Bantock, of London, as Honorary Fellow, was accidentally omitted.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY . . . GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

REMARKS ON OTORRHEA IN CHILDREN.

BY

D. F. KINNIER, M.D.,

Randolph, Mass.; Late Clinical Assistant to Dr. T. Colcott Fox, Westminster Hospital
and Victoria Children's Hospital, London.

PURULENT discharge from the ear is one of the most common symptoms of aural disease.

Otorrhea comes on gradually with little pain, and although the smell from the discharge is oftentimes very offensive, it is frequently allowed to continue for months.

As one of the causes of otorrhea, we may mention catarrhal condition of the meatus and tympanum, in which inflammation may spread to the mastoid cells, and finally the brain.

The prominent symptoms are rigors, tongue very furred, rapid pulse, increase of temperature, and pain and swelling of the parts around the ear, which parts assume an erysipelatus appearance.

The following is an illustrative case.

E. C., æt. six, strumous aspect; left ear affected, and patient has been suffering from deafness for two years. Commenced with pain inside the canal, with some heat and swelling; three months before coming to hospital, patient's mother said patient struck the left side of his head against the door. Shortly after this accident, the mother noticed a discharge from the left ear, for which she got something at the chemist's, and at the end of five weeks the discharge had stopped, and the child had no further trouble until a month or so later, when he came to the hospital.

At the time of his coming under treatment, he had a thick purulent and offensive discharge from the inner side of the meatus, which continued for six weeks from commencement of treatment; at the expiration of this time a swelling began at the

back of the ear, which was very hot, painful, and red ; the mother said that the pain was very intense the last few days previous to coming to the hospital.

On examination, I found great swelling of mastoid region, with all the appearance of suppuration extending in all directions, but particularly behind the ear. The swelling was opened and considerable pus evacuated, which greatly relieved the child. The wound was kept open, and it discharged for a month, a very small draining tube being inserted. The wound healed, and the child enjoyed good health, there being little pain or tenderness at the end of six months. Eight months from first coming under treatment, the mother again brought the patient to the hospital, and said that he had been very bad and restless for a week or more. It was again treated by lancing, opiates, and fomentations, and the recovery took place in about a month. He then continued well for three months, when he had another attack with the same treatment and result. At the end of four months, he had still another attack more severe than either of the former ones. The lance was again used, the wound kept open, and soon ceased discharging.

On careful examination during this attack, a polypus was discovered in the meatus, which was removed by the wire snare, the discharge continuing after the operation. Hearing is entirely destroyed, membrana tympani perforated, but no absolute signs of necrosis exist. I saw the child last on July 24th, 1884, he was then in good health, and there was complete absence of pain or tenderness on pressure over the mastoid region. He complains of a little stiffness during mastication, but does not complain of pain.

This seems to be the history and course which mastoid abscess runs, but there are many cases of mastoid inflammation which ought not to go on to suppuration if properly treated.

The plan of treatment which I generally adopt on detecting pain or tenderness over the mastoid region and around the ear is, first to paint with a strong solution of nitrate of silver (3 ss. ad ʒ i.) or several layers of equal parts of tinct. iodi and lini iodi, or with pot. iodi. lini cum sapone, having previously applied leeches if much swelling and redness exist, and subsequently follow up the treatment with warm fomentations or poultices containing a little opium ; sometimes acute symptoms are relieved by lini apii cum belladon.

I think the above treatment, carefully carried out, may sometimes cut short an impending periostitis, and enable the patient to escape the usual surgical proceedings. As regards the treatment of otorrhea by dry powders, the plan I usually

adopt is to cleanse and dry the ear well with a piece of cotton wool twisted upon the end of a grooved ear probe, then with an insufflator to introduce into the meatus whatever powder seems suitable for the case; a small quantity of cotton wool is then lightly placed in the meatus to prevent the powder falling out. This treatment is renewed night and morning. Boracic acid in an impalpable powder answers very well, but the particles must be very fine or it will cause irritation. All persons may not be provided with an insufflator, but an ordinary quill or paper rolled into quill form answers the same purpose.

This method I have seen carried out and prove very effective. But many aurists object to the dry treatment because, as they say, it produces serious results. Yet I saw a great many cases thus treated, and have never known of any bad consequences. But, on the other hand, there is a practice followed by some of stuffing the meatus with various powerful astringent powders, pushing the powder firmly in by means of some small instrument, and then closely packing with cotton wool which is pressed firmly into the meatus, and left there for a long time. This method cannot be free from danger.

I can recall more than one illustration of the bad effects of this treatment, where the discharge, failing to find an exit externally, has extended to the mastoid cells, producing such serious results as caries, necrosis, inflammation of dura mater, cerebral abscess, and death. The various powders used are boracic, tannic and gallic acid, alum, iron-alum, copper, lead, zinc, etc., etc.

If the wet treatment is preferred, the various astringents may be used; boracic acid in the proportion of one drachm of the acid to an ounce of rectified spirit makes a very nice lotion; or carbolic acid and sulphate of zinc, four grains of each to an ounce of water; or a saturated solution of boracic acid in hot glycerin; or glycerite of tannic acid.

The following are a few good lotions:

R Acid. carbol.....	gr. iv.
Sodæ bicarb.....	gr. xij.
Sodæ bibor.....	gr. xij.
Glycerin.....	3 ss.
Aq.....	q. s. ad $\bar{\zeta}$ i.

Ft. lot. ad aurem.

℞ Zinc oxide,
 Bismuth. oxid.....āā gr. v.
 Glycerin..... 3 ss.
 Aquæ.....q. s. ad 3 i.

In connection with the above treatment, great benefit will be derived from the use of the Politzer bag.

CORRESPONDENCE.

TO THE EDITOR, DEPARTMENT OF DISEASES OF CHILDREN.

NOTICING in the last (Oct., 1884) No. of this journal the case of infantile convulsions successfully treated with the subcutaneous injection of morphine by Dr. Hughson, of Sumter, S. C., I asked and obtained leave of Drs. Dowd and Eddy, of this city, to report the trials they had made of this treatment since their attention had been drawn to it.

CASE I.—Reported by Dr. C. M. Dowd.—“A. L., æt. 4 years 8 months; first saw the child August 19th, 1884. Had been sick about a week with what was supposed to be malarial fever, but just now taken with convulsions. After trying other remedies, warm bath, etc., for about an hour and a half with no effect, I administered one-twelfth of a grain of sulph. morph. hypodermically. Seeing no effect after twenty minutes, I gave one-sixth of a grain of the same medicine, in the same way. Within ten minutes the convulsions ceased and did not return; and the next morning the patient seemed doing well, but died in about a week of meningitis.”

CASE II.—“P. R., 3 years 4 months. Visited Aug. 21st, 1884. Child was attacked the previous afternoon with violent vomiting and purging. When I saw him at 2 A.M., he had been in convulsions about an hour and appeared to be moribund. After waiting a little, I administered one-sixth of a grain of morph. subcutaneously. The convulsions almost immediately ceased, and the child slept quietly and naturally the rest of the night. About 7 A.M. he awoke and talked, but soon after another convulsion came on, and the little fellow was dead before I reached the house.”

CASE III.—Reported by Dr. S. C. Eddy.—“June 7th, 1884. Called to see M. R., two months old. Had been in convulsions for two hours, and was apparently in a dying state. Gave one-sixth of a grain of sulph. morph. hypodermically. Convulsions ceased almost immediately, and did not return. Apprehensive

that I had given too big a dose, I administered a little belladonna. The child got well with no further trouble."

This was indeed a pretty stiff dose; but I doubt whether it was at all dangerous, satisfied as I am that, in the convulsive condition, an extraordinary tolerance of opium obtains. One-fourth of a grain of morphine is not too much to administer subcutaneously to a child two years old, in convulsions. This will not be apt to need repeating.

I take this occasion to say that the treatment of puerperal eclampsia by the hypodermic use of morphine appears to be now perfectly established in all this region, and hardly anybody thinks of giving less than a grain the first thing. In fact, no smaller dose is safe. I have heard of no case of its failure when so administered, and in any kind of season.

Very respectfully,

C. C. P. CLARK, M.D.

OSWEGO, N. Y., Oct. 17th, 1884.

TRANSLATIONS.

THE INHERITANCE AND TRANSMISSION OF SYPHILIS.

BY

DR. M. KASSOWITZ,
of Vienna.

Translated from *Jahrbch. f. Kindhlkde.*, xxi. B., 1 u. 2 H.

By J. FEWSMITH, JR., M.D., Newark, N. J.

(Continued from p. 1118.)

[BUT these observations also are a strong argument upon the question of placental retroinfection of the mother, to which we now return, and in support of our opinion concerning the hypothesis of symptomless syphilis. For here we have women who have borne syphilitic children and yet remained free from symptoms of syphilis for some months after birth. Now a supporter of the hypothesis of symptomless syphilis, if asked concerning the condition of these women, would naturally say that they, like all others in like circumstances, had "latent syphilis," or symptomless syphilis; and now at one stroke the injustice of this view is demonstrated most clearly by the infection of these individual cases. These few cases lay bare the great weakness of the argument by which all these women, in spite of the absence of all specific manifestations and in spite of their incapability of transmitting the disease (as we shall see), are declared syphilitic

on the single ground that they are non-receptive of syphilitic contagion.

Since we have shown that the immunity against syphilis, which really exists in the majority of women who have borne syphilitic children, does not alone justify us in saying that they have latent syphilis, we must search further to find whether, when they have no apparent manifestations of syphilis, they are *able to transmit the syphilitic poison, received from the fetus, to a second individual*. Here is the point where, I think, that those who accept the existence of a symptomless syphilis in these individuals, should bring up their forces and proofs. If these individuals contain the syphilitic poison anywhere, in blood, fluids, or tissues, it must be possible, by the transmission of this to a second organisms, to cause specific manifestations in the latter. With this meaning, I, in 1875, wrote: "It is the duty of those who believe every mother of a syphilitic child, even when free from every manifestation of syphilis, to be syphilitic—it is their duty, I say, to prove the truth of their view by direct inoculation." It is only in this way that "latent" or "symptomless syphilis" may be raised above the level of the most vague suspicion. But since no such inoculations have been attempted, we must seek further to find whether such individuals can transmit syphilis in any other way which is under our observation and control. Such a method is by inheritance. [I use this word in the active sense of inheritance from father or mother to child.—J. F.]

Have we now any proof that such individuals can transmit their symptomless syphilis by the act of conception in any form which shall be manifest on the children? Decidedly not! For experience teaches us that such women hold exactly the same relation to their children as non-syphilitics. In our earlier article, as well as in other later essays, I have brought forward so many proofs of this that we may here be content with a short recapitulation. The following are statements of experience, verified by numerous observations, both by myself and others.

1. Syphilis which is not weakened by mercurial treatment is for a certain number of years, even during its symptomless or latent stages, transmitted—with very rare exceptions—to *all* the successive children born.

2. The children which are born in the first years of such a syphilis (untreated) are, with very few exceptions, intensely affected, so that they either die in utero, or are born prematurely with severe symptoms of syphilis, or, at least, these symptoms develop soon after birth. It is only gradually that the intensity of the inherited syphilis become so weakened that the children are born at full term, that they become more robust, and that the manifestations take on a more benign character and often do not appear for some weeks or even months after birth. This process of the gradual, spontaneous decrease of intensity in the inherited syphilis has been established by myself and others, through so many observations, that the rare exceptional cases are of no weight against the rule. Now, if it is a fact that women

who had borne children who were infected with syphilis from their fathers, have thereby, *eo ipso*, become syphilitic—whether they have positive manifestations of the disease or only a symptomless syphilis—this affection must in successive births manifest itself in a severe syphilitic affection of the children. In many cases the cause of this manifestation would not be clear, because the father of the first child might have a recent, virulent, and therefore “intensely inherited” syphilis. If the first fetus, infected by such a virulent syphilis of the father, dies in utero, and the second child and following ones are also strongly affected, it would, of course, be difficult to determine how much of the poisoning of the later children was due to the father, and how much to the mother. But all cases are not like this. A man frequently marries after an old or apparently cured syphilis, and the first child is born with only mild manifestations of the disease. Now, if the mother, during this first pregnancy, had in any way received the poison into her system, this fresh and untreated infection would certainly be transmitted to the next children, and the birth of the first, mildly affected child would *regularly* be followed by abortions or premature births of children with severe syphilis. This is, however, not the case. On the contrary, the intensity of the disease tends constantly and regularly to decrease. The next children are even more slightly affected than the first, and after a short period, of often only two or three years, healthy children are born. From this it certainly is clear that no syphilis is transmitted to the children from the mother—though she is supposed to have become syphilitic from the first fetus. An exceptional report of a case, in which a woman with a recent syphilis bore a child who was only slightly affected, can have no influence on our argument, which is according to the united testimony of all my own and other observations.

Next, let us consider those frequent cases in which, after the birth of several syphilitic children, *the father alone submits himself to specific treatment*, and immediately the next child is born without signs of syphilis. Now if the apparently healthy wife had become latently syphilitic from the preceding pregnancies, this recent disease of the mother would remain after the father's was cured or modified by treatment, and this would, of course, manifest itself in the later children. And since, both according to my own observations and the reports of others, this is not the case, the conclusion may be fairly drawn that these mothers are free from syphilis. Here we may recall Taylor's two cases, and some from Fournier.

Of not less significance are the cases in which women, having borne syphilitic children from a syphilitic father, in a comparatively short space of time marry healthy men and have healthy children. I have in my other article reported such cases out of my own experience, and out of the literature. In one case, for instance, there was an interval of only four years between the conception of the syphilitic child and the healthy one. Since then Nevins Hyde and Hecker have reported cases of the same

kind. In Hecker's case there were only *two years* between the birth of the last syphilitic child and the healthy one—from a healthy father.

These manifold and variable observations, in my opinion, prove indisputably that these women whose condition of health we are now considering, besides being externally entirely free from every sign of syphilis, are also incapable of transmitting syphilis to their children. Therefore no single one of the postulates which we have declared necessary to the establishment of syphilitic infection is fulfilled. Unless we wish to degrade our conception of syphilis to a minus quantity we must certainly determine that individuals, who have never had a symptom of syphilis, and who are incapable of transmitting the disease to others, are *clearly not syphilitic*. Now, if we sum up all that we can learn from our own observations and the reports of others concerning the transmission of syphilis from fetus to mother, and the condition of health of the latter, the result is as follows:

1. *It is fully proved that in a large number of cases, women who have borne syphilitic children from syphilitic fathers have themselves, even when under close observation for years, shown absolutely no symptom of the disease.*

2. *When inheritance from the side of the father is removed, these women, who have a few years previously nourished syphilitic fetuses in their wombs, bear healthy children.*

3. *Clinical observation and experimental inoculation both show that these women are much less receptive of syphilitic poison than other individuals, yet infection in a few such cases is fully proven.*

4. *The reports of cases of manifest syphilis in some mothers, without any preceding primary lesion, are extremely contradictory, and only in a few cases of any scientific weight. The proof of such an occurrence must be gained by far more exact research.*

5. *The separating wall between the fetal and maternal circulatory systems, therefore, in a large number of cases, proves an obstacle to the transmission of the syphilitic poison from fetus to mother, and it has not yet been strictly proved even in single cases that this obstacle can be overcome in this direction.*

IV.—INFECTION OF THE FETUS IN UTERO.

Before entering into an argumentative discussion of this question and a careful examination of the facts bearing on it, I must say a few words as to its historical origin.

Most of the early authors took this method of transmission for granted, and did not consider it necessary to report observations in support of it. But those authors who went more closely into the matter have almost unanimously agreed that this transmission does not take place in *every* case in which a mother is infected during pregnancy, and some of them have attempted to formulate a rule according to which this transmission sometimes occurs and sometimes does not. But conclusive observations are

wanting even in these authors, to show that in any single case they exercised such control as to make their observations trustworthy, to show positively that the syphilis of the fetus or child could actually have been transmitted only per placenta, and could not have originated through the infected semen or ovum.

When I began to study the question, I considered it *a priori* as extremely probable that the syphilitic poison circulating in the blood of the mother was carried, with the nourishing fluids, into the fetal circulation.

This idea was in accordance with physical rules and in analogy with other infectious diseases in which (for example, variola) it is clearly proven that the contagion passes from mother to fetus. On these grounds, I at first met with a good deal of scepticism the remarks of Mandon and Baerensprung, who denied the placental infection of the fetus. Their statements, however, caused me to search in the literature for positive reports of such cases of transmission, and I became convinced that no positive and indisputable cases were to be found, but, on the contrary, many observers reported indisputable cases of women who were infected with general syphilis during pregnancy, and yet bore healthy children. Then, when I myself met with such a case, in which there was absolutely no doubt, I had to give up my previous opinion that the syphilitic poison is without ceremony transmitted from mother to fetus. I could not help seeing that, in numerous cases, the poison which was present in the blood of the mother came to a halt at the dividing wall of the fetal circulation, and now the question arose whether such a transmission ever took place. Here came in the relation between the healthy mother and the syphilitic fetus, and as I became convinced that, in a very large majority of cases, the poison did not pass from the diseased fetus to the healthy mother, and that there was no single case reported in which the transmission of the poison in this direction was positively proved, I was forced to the conclusion that the syphilitic virus in this respect differs from the contagion of most acute infectious diseases, and that it finds an obstacle in the dividing wall between the two circulating systems. According to this, the transmission of syphilis from parents to descendants could only take place by way of the diseased semen or ovum, and this gives an hypothesis which, "for simplicity and strict legality," commends itself as inviting and plausible, not only to me, but to a large part of the profession.

When I first arrived at this general conclusion, the question forced itself upon me, what was the reason that the syphilitic virus differed in this regard from the contagion of other infectious diseases? I believed I had found an explanation for this in the supposition that the syphilitic poison did not circulate freely in the blood-serum, but that it was united with the formed elements of the tissues, the blood, the lymph and pus, and that this relation of the poison to the formed elements of the blood—which is very probable on many other grounds—was probably the reason that the dividing wall of the two vascular systems, which

most probably does not allow of the free passage of the blood-corpuscles, also prevented the transmission of the poison held in these corpuscles. This statement has not been refuted by the attempts of Zeissl and Caspary.

To turn from this theoretical discussion to the examination of facts, we find that this examination must turn in two directions. We must ascertain:

1. Whether mothers affected with indisputable recent general syphilis can bear healthy children, and whether these cases are rare or frequent.

2. Whether there are observations in which the health of both parents is conclusively proved at the time of conception, and in which the syphilitic affection of the child can, therefore, have taken place only through the placental circulation from a syphilis acquired by the mother after conception.

It is evident at a glance that the establishment of the facts in answer to the first question is much less difficult than in the second. In the first, we may leave out of view the most difficult thing to prove in the second, namely, the condition of health of the parents at the time of conception. For it is sufficient to show that a pregnant woman affected with a comparatively recent form of syphilis bears a child who is and who remains healthy, in order to conclude with certainty that, *in this case*, there has been no placental infection. It is then in the highest degree probable that both parents were healthy at the time of conception, but even in the very exceptional case that the mother should be syphilitic at the time of conception, and for some unknown reason not have transmitted this syphilis to the fetus by way of the ovum, yet the simple fact that the child is healthy is enough to exclude in this case the placental transmission of the disease. Such observations have been made and published in comparatively a large number when we recollect that the syphilitic infection of a pregnant woman who has conceived from a healthy man is fortunately not a daily occurrence. In my first article I collected the cases bearing on this point which I myself or others had reported. In some of my cases, Hennig's, and Baerensprung's, the health of the parents at the time of conception was established. But since we know that this is not important, we may accept as proofs the numerous other cases collected by me, from Baerensprung, Köbner, Bidenkap, Ritter v. Rittershain, and Peck, and my combined observations made here in the foundling asylum and the lying-in hospital, in which latter the comparatively fresh syphilis of the mothers was discovered at the lying-in period and the health of the children established. Since then cases of the same sort have been reported from all directions. The observations made in lying-in institutions are here of great weight, on account of their large number, though it may be objected that the children are not kept long enough under observation to establish their non-syphilitic condition. Mewis reports from Dresden thirty-three cases of children born from women who had recent syphilis, acquired probably during pregnancy, the

children remaining healthy while under observation. Anton reports fifteen and Hecker forty-six analogous cases, and Hecker adds to his statistics the remark: "In these cases, therefore, the placenta acts as a filter, which does not allow the poison to reach the child." In the case-list of Fournier there are also quite a number of cases in which women who were infected during pregnancy bore children free from syphilis. In some of these it may be objected that this was the result of mercurial treatment of the pregnant women. But since experience has, alas, shown that, in cases of pregnancy, where we have cause to suspect the child to be syphilitic from conception, such treatment almost never does any good, we must not lay too much weight on it in these other cases, especially when they agree in their final result with the large statistics of the lying-in institutions, where, in most cases, the mothers had no treatment. I will give two of Fournier's cases, because they show so strikingly how different is the fate of the *later children* who, *at the time of conception*, were under the influence of the syphilis of the mother.

Obs. IV. (p. 252). In 1877, syphilis acquired in course of pregnancy, several months' treatment. Fully developed, full-term child which died when a month old, of convulsions. Two pregnancies in 1878 ended in abortions.

Obs. V. In 1874, secondary syphilide during pregnancy, child born healthy at full term—but died three months after from cause not stated. In 1875, abortion at three months. In 1876, abortion at seven months.

In nine other cases, women who were probably infected during pregnancy, and frequently showed the primary lesion, bore children who remained healthy, during a period of observation of some weeks or months.

Even in the statistics of Wolf, who believes that the fetus is infected only through the placenta, there is something of value for our point. Out of ten cases in which infection was supposed to have occurred during pregnancy, in only one was it proved that the father acquired syphilis subsequent to the conception—and behold, this child showed no signs of syphilis, though watched for two and a half months. In the other cases, nothing was known of the father, and it is more than probable that the children acquired their syphilis from the paternal germinal cell.

Single cases of the birth of healthy children from mothers with recent syphilis have been reported by Chiarleoni (two cases), Engelsted (child healthy up to one and a half years), and Fraenkel (three cases). Birch-Hirschfeld examined two still-born children, of mothers with recent syphilis, and found no token, external or internal, of the disease. Finally we must not leave out of consideration the important fact that the supporters of the theory of infection in utero almost unanimously confess that healthy children are *often* born from women who are infected during pregnancy.

Those who deny this fact—and these are only scattered voices—object to the most of these observations that the children are not

kept long enough under observation, and that the symptoms of the disease may appear later. In reply to this I would say that in my experience, which agrees with that of most authors, in the majority of cases the first cutaneous symptoms appear within the first month, and further, that reckoning together all cases of syphilitic births which have come to my knowledge, in only ten per cent of them have the symptoms appeared after the second month, and such a late outbreak as this is seen only in children very slightly affected, from a very old or weak syphilis of the parents. [In a foot-note the author states that he has *never* seen the first symptoms appear later than the end of the third month, and he shows how, in some cases where this has been reported, the first eruptions have been overlooked by the mothers, and how in other cases the absence of earlier symptoms was predicated only on the unreliable statements of mothers or attendants.] Moreover, it is well known that the outbreak of cutaneous symptoms is also preceded for some time by quite striking prodromal symptoms, especially coryza, which is often present from the time of birth. Hence, though we cannot exclude the possibility that of the numerous children here as reported as healthy, some single ones may have at a later date shown symptoms of syphilis, yet we may strictly say, in view of the relative rarity of the later outbreaks, that the greater portion of these cases are indisputable.

Finally we must meet the objection that perhaps these children in later years, at the time of puberty, may show signs of the so-called syphilis hereditaria tarda. To this I reply that I agree completely with those who consider the manifestations of syphilis hereditaria tarda as only the late, tertiary symptoms in individuals who suffered in the usual way from the manifestations of syphilis in their early months. My experience has taught me: First, that gummy products have appeared on the bones and throats of some children in their eighth to twelfth years, whom I had treated for hereditary syphilis in their early months, and second, I have in a large number of cases had children born healthy from syphilitic parents, especially the later children, following those who had syphilitic manifestations, under my observation for many years, and I have never yet known of a late outbreak of tertiary symptoms in those who had no signs of syphilis in their early months. And since no other observer has reported such an occurrence in a child who has been, from birth on, under personal or professional observation, we may concede that the existence of this form of syphilis is undemonstrated.

It is therefore indisputable that in a very large number of cases women who either became syphilitic during pregnancy, or at least showed signs of fresh general syphilis at the time of delivery, have borne healthy children. In these cases, therefore, the poison circulating in the blood of the mothers, in spite of the especially active interchange of fluids between mother and fetus during the last months, could not force its way into the blood of the fetus.

Before considering the other questions, whether there are well-

established cases of infection of the fetus per placentam, we must examine a point which has been heretofore but little noticed. What condition or relation is shown toward syphilitic infection from without by children who, during the fetal period, have been in the midst of a continual interchange of fluids with an actually syphilitic mother, without themselves becoming syphilitic? Has the theoretical analogy between these children and those mothers who have borne syphilitic children without becoming diseased found expression in facts?

My own experience on this point is purely negative, for the character of my material is such that the combination of healthy child and syphilitic mother only exceptionally (the cases reported) comes to my notice, so that I cannot say much concerning their further fate. I can only state that, while the cases of acquired syphilis in small children are not extremely rare, yet I have in no case had ground to suppose the post-partum infection of a child from a mother who became syphilitic before delivery. Fournier, however, who has had frequent cases of this combination, says distinctly that he has never seen such a child infected by its mother, even when nursed by her. Fournier does not stand alone in this observation, for I find from Blaise's work that Profeta and Gailleton have called attention to the fact, and stated such children never become infected from their mothers, even though the latter's syphilis is in fullest bloom.

There is no doubt that the opportunity for such a mother to infect her child is exceedingly ripe. Think of the manifold relations between mother and child, the suckling (through which healthy children are infected from syphilitic nurses), the various handlings of the child in its dressing, etc., and especially the caressing and kissing. It is strange that, with such opportunities, infection is not frequent. Finally we must not overlook the frequent opportunities for infection of the child during birth, especially when the mother has specific ulcers on the genitals. Yet such cases of infection are extremely rare. Nothing of the kind has been reported from the lying-in institutions, where, as we have seen, it is a frequent occurrence for healthy children to be born from women with infecting syphilitic genital affections. That infection *may* take place from the mother's genitals—though perhaps rendered difficult by the vernix caseosa, etc.—is proved by a case observed in our foundling asylum, where a child acquired a *soft* chancreoidal ulcer upon the head from a similar sore on the mother's genitals.

In spite of all this, *two* cases of syphilitic infection per partum have lately become known.

Weil's Case.—First child healthy; conception of second child in July, 1876. Father acquired syphilis in following September. and mother discovered the disease on her genitals shortly before delivery. When the child was four weeks old an ulcer appeared on the side of its nose, which at the age of three months was as large as a penny, covered with a scab, and with a distinctly developed base. Seven weeks after the ulcer appeared (eleventh week of age), the general skin symptoms were manifest.

There was no coryza and no diffuse infiltration of hands or feet. The child was excellently nourished.

Grünfeld's Case.—A woman was infected from her husband about the middle of pregnancy. At time of delivery she had ulcerated papules on the genitals. Up to third week the child was healthy, but in the eighth week Grünfeld found a characteristic chancre on the scalp, which he took to be a primary lesion acquired at birth, and which was accompanied by a general exanthema.

Besides these, two cases have been published of infection of such children from their mothers *post partum*.

Obtulowicz's Case.—In eighth month of pregnancy *ulcus induratum labii pudendi*, syphilis *secundaria recens maculosa*, polyadenitis. The child was born healthy, and during six months' observation showed no specific symptoms. When again seen in the eleventh month, it had condylomata on tongue and tonsils, condylomata lata ad collum, nates, etc., and polyadenitis. According to the mother's statement, these symptoms had existed for one month. The mother at that time had condylomata on tongue and lips, and Obtulowicz believes that the child was infected on the mouth from the mother—a conclusion which a consideration of the facts renders probable.

Arning's Case.—The mother, after bearing two non-syphilitic children, in the fourth month of her third pregnancy acquired syphilis from her freshly infected husband, and, went through a course of treatment by inunction, bore a full-term, healthy child, which was nursed by her, first on both breasts and then only on the right one, on account of fissures on the left nipple. According to the mother's statement, the child, when two and one-half months old, first showed a moist spot on the corner of the mouth. This would not heal, but gradually invaded a portion of the lip, so that when first seen by the doctor, when three and one-half months old, almost the whole upper lip was destroyed by it. The submaxillary glands were much swollen, the other glands as large as beans. A papular syphilide, not invading the palms or soles, has just appeared. The ulcer and the syphilide healed very rapidly under mercurials. The child died some months later. There were no spots of softening in the brain, and only slight rachitic changes in the bones.

(To be concluded.)

REVIEW.

A PRACTICAL TREATISE ON DISEASE IN CHILDREN. By EUSTACE SMITH, M.D. William Wood & Co., New York, 1884, pp. 844.

The author of this treatise is favorably known this side of the Atlantic from his writings on the wasting diseases of children. The introductory chapter of the book before us contains good advice in regard to the mode of examining children. The first lesson which the student in pediatrics should learn is how to approach a sick child, and this the author teaches us in the commencement of the book. Many physicians, learned and skilful, frighten their little patients by the manner in which they approach them, and consequently find it more difficult to conduct the examination properly and ascertain the nature of the malady than if they had pursued a more quiet and gentle course. The physician should, in the opinion of the author, enter the sick chamber quietly; he

should not immediately approach the bedside unless the case be one of urgency, as in convulsions. Sitting at a distance, but where he can carefully observe his patient, he should learn its history from the mother or nurse while he notices its aspect and actions. One familiar with the diseases of childhood can often make the diagnosis by carefully observing his patient aided by the history before he proceeds to a more minute examination. Gentleness and tact so as to relieve the fears and gain the good-will of the patient are necessary for correct diagnosis and successful practice among children. A thorough examination can be made in most cases without exciting the patient or causing him to cry and resist through fright except when the fauces are inspected. The views expressed by the author in reference to the mode of conducting an examination will be especially useful to the young physician.

The peculiarities of childhood, so far as they affect the health and modify the diseases of the child, are also dwelt upon in the introductory chapter, such as the predominant influence of the nervous system, "nervous irritability," through which simple derangement of functions often produces severe general symptoms, and symptoms in organs at a distance from the seat of the affection. The influence of the various diatheses upon the course of diseases and upon the treatment is also fully set forth.

But while our views are in accord with the general tenor of the text, now and then an opinion is expressed which is not sustained by observations on this side of the Atlantic. An example of this is the statement that laryngismus stridulus is a common cause of sudden death. We have reason to believe from English treatises that this is true as regards the large cities of Great Britain, but in the United States this disease is infrequent, and is seldom the cause of sudden death. We must also take exception to the statement that children often die of simple derangement of organs not sufficient to constitute disease or produce lesions. Occasionally in the commencement of a severe malady a child may die from convulsions or coma and, from the suddenness of the attack, the cause cannot be ascertained, but, with this exception, death, according to our experience, seldom occurs without our being able to discover an adequate cause through a carefully conducted necropsy.

The acute infectious diseases are fully described by the author and the treatment recommended is such in the main as is employed by intelligent physicians on this side of the Atlantic. Some remedies, however, which are commonly prescribed and highly esteemed by American physicians are not mentioned by the author. No mention is made of the bromide of potassium or sodium, or of ergot in the treatment of cerebro-spinal fever. These are agents which physicians in this country constantly prescribe, and their use appears to be indicated by the anatomical characters of the disease. Certainly in the acute stage of cerebro-spinal fever, large and frequent doses of the bromide aid in relieving headache, procuring rest, and preventing convulsions, and from what we know of the therapeutic action of ergot, this agent aids in diminishing the inflammatory turgescence of the nervous centres. Therefore the therapeutics of cerebro-spinal fever as recommended by the author seems incomplete without some notice of these medicines.

Rötheln, epidemics of which have not been infrequent in the United States during the last few years, is unfortunately designated epidemic roseola, a term which is calculated to mislead the reader into the belief that it is one of the skin diseases which has

assumed an epidemic character, whereas clinical observations show that it has a specific cause, is propagated by contagion, and has an incubative period. It is as distinct from roseola as is small-pox from impetigo. The description of the disease, though brief, is good, and the statement of the author that *rötheln*, which has been only recently recognized in this country and in Great Britain, is mild in its nature, and without complications or sequelæ, corresponds with the experience of American physicians.

The author does not fully accept the theory that the specific diseases are caused by micro-organisms. Of diphtheria, he writes: "The essence of the disease has been attributed to spherical bacteria . . . ; but as similar bacteria have been found in the secretions thrown out by ordinary non-specific stomatitis, too much importance must not be attributed to the presence of these organisms. The real nature of the virus has yet to be discovered." As might be inferred from the above extract, the general tenor of the book is conservative. New views are not fully and unconditionally accepted, unless substantiated by sufficient experience. This might make the text in some places appear behind the times, but in the main the opinions expressed, and the descriptions of the various diseases, are in our estimation sound and the treatment recommended is fully up to the times. The author is evidently a painstaking and conscientious observer, and he has made himself familiar with current pediatric literature.

The volume is enlarged by the introduction of certain maladies which, although they may occur in childhood, pertain especially to adult life, such as scurvy, cerebral tumors, hydatids of the liver, cirrhotic, fatty, and amyloid livers. These are described at length in treatises relating to the maladies of adults. Although they are not usually treated of among the diseases of childhood, yet as some of them may present peculiar features during this period, their admission into the text is not out of place.

Unpleasant consequences may result from some of the prescriptions recommended by the author unless the reader bear in mind that they are according to the British Pharmacopœia. Thus the dose of tincture of belladonna recommended for an infant between the ages of eight and twelve months for constipation is five minims. This in our pharmacopœia would be about two and a half minims. The author states that he cured a child of four years of enuresis by a draught containing one drachm of tincture of belladonna. This in our pharmacopœia would be half a drachm.

The text is very free from errors of the type-setter, and the print is much better than that of some recently issued medical treatises. The style, though not so terse as that of Henoch, is easy and pleasant, and the volume contains much less to censure than is ordinarily found in first editions.

We welcome the treatise as a useful addition to the literature of pediatrics. While we have pointed out what we think are some of its faults, we have done so, not to condemn the general character of the book, for its excellences are such that we believe it will furnish substantial help to the practitioner who consults it.

J. LEWIS SMITH.

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ORIGINAL COMMUNICATIONS.

ON COMBINED TURNING IN THE TREATMENT OF PLACENTA
PREVIA.

BY

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THE introduction of the method of bimanual version by Braxton Hicks¹ was one of the greatest improvements made in operative obstetrics. Turning has obtained altogether a new feature since its application; operations previously considered impossible are now performed with facility. This is real *practical progress*.

Braxton Hicks recommended his method particularly for cases of placenta previa; he showed its advantages and its practicability on six cases treated thus with success. His method was well received in the discussion on this subject in the London Obstetrical Society, and its advantages were accepted by the profession at large, but little has been heard of its adoption in general practice for the treatment of placenta previa. Occasionally a few cases treated accordingly were published, but no statistics based upon large numbers had proved the superiority of his method in the treatment of placenta previa. In Germany, Hecker and Spiegelberg had

¹ Braxton Hicks, Lancet, 1860, July; Obstetrical Trans., Vol. v., p. 222.

pronounced themselves against combined turning for this purpose. Possibly this was the reason why no earnest attempts had been made to adopt it in this country. In America, no great importance seems to have been attached to this subject. Thus among 240 cases of placenta previa which occurred in the State of Indiana, and have been collected by King,¹ I found only one case mentioned as treated by bimanual version. Text-books have invariably spoken of it of late, but what we want are large collections of cases treated accordingly. Do such exist? If so, what results did they give? Is the prognosis of placenta previa better if treated thus than when treated according to other methods?

I believe we can affirm the last question, and I think it is possible to prove it with abundant evidence.

Hofmeier² was the first who adopted Braxton Hicks' method in hospital practice. Before him, single cases treated thus had been published, but they made no impression. Hofmeier supported his assertions with large number; he proved by facts that the expectations Braxton Hicks had brought forward on this subject were confirmed by practice at large.

Next Behm,³ influenced by Hofmeier's success in the treatment of placenta previa, adopted the method in a large series of cases. His results were brilliant.

Hofmeier gathered his experience on this subject in the policlinic of the University Hospital for Women of Professor Schroeder in Berlin. He operated on all his cases himself. Behm's cases came from the midwifery department of the Charité Hospital in Berlin, superintended by Professor Gusserow. He also operated on all the cases himself.

The cases at my disposal occurred in the policlinic of the University Hospital for Women in Berlin, the same institute which furnished Hofmeier's cases. They begin at the time the latter end, and were operated upon by the different assistants who have been on duty in the hospital since. I am indebted to Professor Schroeder for kindly allowing me to publish the valuable material of his clinic in this paper. The material I am able to lay before the profession has spe-

¹ King, AMERICAN JR. OF OBST., Vol. xiii.

² Hofmeier, Zeitschrift f. Geb. u. Gyn. Vol. viii., p. 82.

³ Behm, Zeitschrift f. Geb. u. Gyn., Vol. ix., p. 373.

cial value on account of its uniformity and of the large number of cases. The fact that the latter were observed in the course of a few years, in the same city, and treated mostly according to the same method, gives a special importance to this collection.

The combined results of Hofmeier's, Behn's, and my cases are:

Cases of placenta previa treated by bimannual version.

	No. of Cases.	Deaths.
Hofmeier's cases.....	37	1
Operated upon by H. alone. Cases of the University Hospital for Women in Berlin.		
Behn's cases.....	40	0
Operated upon by B. alone. Cases of the Charité Hospital in Berlin.		
My cases.....	101	7
Operated upon by 9 differ- ent assistants. Cases of the University Hospital for Women in Berlin.		
	178	8 = 4.5% mortality.

Statistics on placenta previa based on such large numbers have never given any similar results. I will here only mention this as a matter of fact, and with it I will close these introductory remarks.

In what does the method consist?

Turn by the bimannual method as soon as possible, pull down the leg and tampon with it and with the breech of the child the ruptured vessels of the placenta. *Do not extract the child then*, let it come by itself, or at least only assist its natural expulsion by gentle and rare tractions. Do away with the plug as much as possible; it is a dangerous thing, for it favors infection and valuable time is lost with its application. Do not wait in order to perform turning until the cervix and the os are "sufficiently dilated to allow the hand to pass." Turn as soon as you can pass one or two fingers through the cervix. It

is unnecessary to force "your fingers through the cervix" for this. Introduce the whole hand into the vagina, pass one or two fingers through the cervix, rupture the membranes, and turn by Braxton Hicks' bimanual method. Use chloroform freely in performing these manipulations. If the placenta is in your way, try to rupture the membranes at its margin; but if this is not feasible, do not lose time, perforate the placenta with your finger, get hold of a leg as soon as possible, and pull it down.

Up to this moment the treatment is an energetic *active* one. Experience shows that flooding now ceases. The next part of the treatment is of an *expectant* nature. A quick extraction made now would cause rupture of the cervix and fatal post-partum hemorrhage. Wait, therefore; give the patient time to rally her powers, wait until pains set in, and then assist Nature by exerting slow and gentle tractions. If the child is in danger during this time, let it run its risk, let it die if necessary, but do not endanger the mother by quick extraction.

Cervical laceration is always a dangerous thing; it is particularly dangerous, however, in placenta previa, on account of the great vascularity of the tissue of the cervix and its liability to rupture. Atony of the uterus is also a disagreeable complication, especially in cases of placenta previa, where there generally is not much blood to lose. Both these dangers may be got rid of by an *expectant treatment after turning*. Pains generally set in quickly, the cervix distends rapidly, and the child is born generally between one and two and one-half hours after turning.

In his admirable treatise on placenta previa, Sir J. Y. Simpson¹ says: "*Two great sources of danger*, in fact, require to be taken into consideration in relation to the operation of turning in each individual case of placental presentation, namely, *first*, the danger of too long a continuance of the hemorrhage, and consequently the exhaustion, and even the death of the patient, if the operation be not performed sufficiently early; and *secondly*, the danger of contusion and laceration of the cervix uteri and its included vessels, if the operator, afraid of delay and of the effects of hemorrhage, proceeds to deliver too soon." *The method we recommend obviates both of these dangers:*

¹ Sir J. Y. Simpson: *Selected Works*, p. 269.

In turning early, it arrests hemorrhage; in allowing Nature to expel the child, it prevents laceration of the cervix.

True, no regard is taken of the child's life in acting according to such rules. This is a grave objection to the method and must be considered at length. I will here only mention our opinion on this subject, returning to details later on. Our opinion, briefly, is that:

1st. The prognosis for the child being in all events bad in placenta previa (premature labor, anemia of the mother, disturbed circulation in the placenta, abnormal presentations of the child, prolapse of the cord and operations necessitated by such presentations); 2d. The value of the mother's life being incomparably greater than that of the child, it is not right to allow the mother to run even the risk of fatal hemorrhage, in order to save a child's problematic life. 3d. Our results being not worse for the children than those hitherto obtained; our results being, on the other hand, surprisingly good for the mothers, we have a right to claim superiority for our manner of proceeding.

Let us next inquire how this method originated and how it developed.

Historical Notes.

In perusing the literature on placenta previa, we find occasional remarks of some operators, according to which they were acquainted with the bimanual turning even before Braxton Hicks. According to Müller,¹ Hamilton in 1822 and Lee in 1848 already operated thus. Other methods which in many points resemble the proceeding of Braxton Hicks, enabling their authors also to operate when the cervix is not dilated, ought not to be mixed up with this. Rigby and others, for instance, used to dilate the cervix slowly with their fingers and finally introduce the hand into the uterus. Rigby was also acquainted with the danger of quick extraction, and warns against it. His method consequently is in many points similar to that of Braxton Hicks; there is, however, the essential difference of the introduction of the whole hand into the uterus.

Influenced by the success Wigand and other German operators

¹ Müller: Placenta Previa, pp. 57 and 68.

had with the method of turning by external manipulations, Braxton Hicks combined this method with ordinary version, and thus discovered a new method. He is fully entitled to have his name attached to it, for he was aware of its advantages and of its importance, and he endeavored to make it known and imitated by others. This seems to have been the case in England. Thus Barnes,¹ for instance, says in 1870: "This (turning) can almost always be accomplished without passing the hand into the uterus; the bipolar method of turning here finds one of its most valuable applications. . . . Rapid extraction involves a certain amount of violence and shock. Gentle extraction, giving the cervix time to dilate gradually, avoids this mischief." I will here remark that Barnes had excellent results. He only lost six out of sixty-nine mothers.

Leishman² says: "To effect this (turning), the passage of one or two fingers through the os is all that is necessary, and, in so far as placenta previa is concerned, we are convinced that this method is for other reasons particularly applicable."

Whilst the advantages of the method were generally recognized in England, they were not accepted by all in Germany. Thus Hecker³ speaks unfavorably of the method; some remarks he makes about it show, however, that he employed it in quite a different manner from that recommended by Braxton Hicks. He mentions, for instance, a case in which combined turning was immediately followed by extraction. The latter, being very difficult, on account of the cervix retaining the head tightly, necessitated a deep incision into the cervix. Now this means accouchement forcé, and shows that Hecker did not wait to let Nature expel the child, as Braxton Hicks teaches. Besides, Hecker gave no chloroform and therefore operated under disadvantages.

Spiegelberg⁴ and Müller⁵ brought forward against combined version that it is difficult or impossible to perform, and that in placenta previa hemorrhage does not cease when traction is exerted on the leg of the fetus. Now every one who

¹ Barnes: *Obstetrical Operat.*, 1870, p. 428.

² Leishman: *Midwifery*, 1873, p. 440.

³ Hecker, *Bayerisch. Intelligenzblatt*, 1875, pp. 318, 319.

⁴ Spiegelberg: *Lehrbuch*, 1882, p. 371.

⁵ Müller: *Placenta Previa*, p. 284.

really will take the trouble to try it, will soon be convinced that these objections are groundless. Braxton Hicks¹ was right in saying: "I have in all cases narrowly watched for any external or internal hemorrhage without having met with it. . . . Turn and the danger is over if you employ the child as a plug; wait then for the pains, rally the powers in the interval, and let Nature, gently assisted, complete the delivery."

In Germany, Kuhn,² Fasbender,³ and Martin⁴ published each some cases of placenta previa in which combined version had been accomplished with success. Martin did not always abstain from extraction after it; he is disposed to ascribe to this circumstance his three fatal cases out of twenty-nine. Schroeder⁵ has always been convinced of the advantages of bimanual version, and recommends gentle extraction or a quite expectant treatment after its performance. The views he published on this subject in 1877 have remained the same up to-day; in the newest edition of his "*Lehrbuch der Geburtshülfe*" (1884), his advice is: "Rupture the membranes, bring down a leg, but wait with extraction."

Kaltenbach⁶ also warmly recommends this proceeding; he operated with success, according to it, in two cases, and remarks that the difficulties of the operation have been overrated.

I have already mentioned Hofmeier's and Behm's excellent publications; they were the last that appeared on this subject in Germany.

What I have seen of French and of American literature makes me believe that Braxton Hicks' method is not known or not practised in these countries to the extent it ought to be.

Thus Wasseige⁷ only mentions it in remarking that its application is limited to rare cases, for should it not succeed we would render ordinary version more difficult, the membranes having been ruptured by the attempt. Charpentier⁸ does not

¹ Obst. Transact., vol. v., p. 234.

² Kuhn, Wiener med. Presse, viii., p. 813.

³ Fasbender: Beiträge zur Geb. u. Gyn., vol. i., p. 422.

⁴ Martin, Zeitschrift für pract. Medicin, 1877, No. 19.

⁵ Schroeder, Zeitschrift für Geb. u. Gyn., vol. i., p. 225.

⁶ Kaltenbach, Zeitschrift für Geb. u. Gyn., vol. iii., p. 185.

⁷ Wasseige: Opérations Obstétricales, 1881, p. 118.

⁸ Charpentier: Traité d'Accouchement, 1882, p. 561.

even as much as mention the method of Braxton Hicks for the treatment of placenta previa in his new extensive text-book.

I have already quoted that King, in the collection of cases of placenta previa occurring in the State of Indiana, only had one case among two hundred and forty which had been operated upon according to bimanual version. And the proceeding Lusk¹ recommends in his admirable text-book differs essentially from Braxton Hicks' method. Lusk says, "when the cervix has been sufficiently stretched (with dilators) to admit of delivery, two fingers should be introduced, the placenta should be separated, the membranes ruptured, and an extremity should be seized without passing the entire hand into the uterus. Extraction should follow," the "pressure of the fetus preventing any considerable amount of hemorrhage."

I have endeavored to explain why this method of treating placenta previa was not adopted earlier, why comparatively so little about it is found in the extensive literature on this subject. The following circumstances might perhaps account for this:

1. Bimanual turning in obstetrics—like bimanual exploration in gynecology—was unknown.
2. The introduction of chloroform was of great importance.
3. Operators used to labor under the wrong belief that the grand thing that ought to be obtained in placenta previa was a quick delivery.
4. They used to attach too much importance to the life of the child.

Prognosis for the Mothers.

I give here a list of collections of cases showing what has been the result of the inquiry of authors as to the prognosis for the mothers in placenta previa.

¹ Lusk: Midwifery, 1882, p. 561.

Prognosis for mothers according to large collections of cases of placenta previa.

	NO. OF CASES.	DEATHS.	PER CENT.
Charpentier ¹ (cases of Parisian clinic)..	65	22	35%
Depaul ²	71	23	32%
Simpson ³	654	180	29%
Schwarz ⁴	332	80	26%
Charpentier ¹	952	237	25%
Trask ⁵	938	237	25%
Müller ⁶	912	212	23%
King ⁷	240	54	22.5%

Such has been the prognosis of placenta previa for the mothers; text-books, basing upon these numbers, speak of this complication therefore in a very dismal manner. Lusk, for instance, says, "the prognosis of placenta previa is necessarily extremely unfavorable. As many as one mother in four dies during or shortly after delivery. Including deaths from puerperal processes, Müller estimates the total mortality at not less than from 36 to 40%." ("Text-book," p. 556.)

Now what I want to prove is, that *if the proper cases be treated by the method of combined turning, the prognosis for the mothers is much better than what statistics have shown hitherto, and much better than what text-books generally accept it to be.*

Before entering into the details, I wish to make a few preliminary remarks:

The first is, that cases occurring in the practice of *one single operator* often give better results than collections of cases taken from different authors—a circumstance which Playfair⁸ has already insisted upon. The following list of cases will prove this:

¹ Text-book, 1883.

² In Playfair, 1879, French translation, p. 549.

³ Selected Works, p. 179.

⁴ Statistics of Gd. Duchy of Hesse. Monatschrift f. Geb., vol. 8.

⁵ In Bedford's Obstetrics, 1872, p. 476.

⁶ Placenta Previa, 1877.

⁷ Statistics of the State of Indiana, AM. JOURN. OF OBST., xiii., p. 750.

⁸ Playfair: Midwifery, French transl., 1879, p. 549.

Collection of cases of placenta previa occurring in the practice of single operators.

	NO. OF CASES.	DEATHS.	PER CEN
Spiegelberg ¹	102	16	16%
Barnes ²	69	6	8.5%
Hecker ³	70	7	10%
Müller ⁴	15	0	0%
Murphy ⁵	15	0	0%

In order to form an opinion as to the general mortality in placenta previa, selected good results of this kind cannot be of any use. If so, I would choose, for instance, the combined results of Hofmeier, Behm, and myself (*i. e.*, cases personally attended) and I would arrive at the following astonishing statistics for placenta previa:

Hofmeier,	37 cases,	1 death.
Behm,	40 “	0 “
Lomer,	16 “	0 “

93 cases, 1 death.

This would not be a fair calculation. I believe, therefore, that the cases at my disposal, having been under the care of nine different assistants, each of them gathering his experience from them, are particularly well adapted to study the prognosis of placenta previa, for although the treatment was an identical one, the circumstances are somewhat similar to those in general practice.

Another circumstance I wish to note is, that since antiseptics have been in use in obstetrics, *all operations* have become less dangerous and that it is not right to compare statistics of nowadays with those of years ago without mentioning this fact. Cases of placenta previa ending fatally in child-bed on account of puerperal fever must, on the other hand, necessarily be counted. Hofmeier has already insisted upon this point, showing that Hecker published much too favorable results by not adhering to this principle. A method of treating

¹ Spiegelberg: Lehrbuch, 1882, p. 368.

² Barnes: Obstetrical Operat., 1870.

³ Hecker, Bay. Intellig. Bl., 1875, p. 368.

⁴ Müller: Placenta Previa, 1877, p. 75.

⁵ Murphy, Brit. Med. Journ., 1884, p. 215.

placenta previa with repeated and prolonged packing of the vagina with tampons is liable to cause decomposition of the secretions and puerperal fever *more easily* than a method according to which the plug is only used exceptionally, the hand being introduced into the genital canal only *once* in order to perform turning, and labor subsequently left to Nature. The breech of the child does in this case what the cotton rag is expected to do in the other.

The cases occurring in our policlinic are at first attended by a student. When he finds that he has to do with placenta previa, he is ordered to tampon the vagina and send for the assistant. Thus valuable time is often lost and the patients are exposed to infection from the tampons—two dangerous circumstances which might have been prevented, had the operator been present at the first call, as is the case in ordinary practice. Besides, many of our patients had been treated by practitioners in the city with other methods previous to our assistance being asked for. We sometimes found the women already infected on our arrival or in such an extremely anemic condition that the prognosis had to be considered bad at the very first sight of the patient. In a few cases they were moribund on our arrival.

Notwithstanding all these unfavorable circumstances, our results were very good in comparison to general statistical computations, such as shown in table No. 1. Our *total number of cases* was 136; of these 13 died, *i. e.*, 10%. Adding to these the results of Hofmeier and Behm (*i. e.*, their *total number of cases*, including every single case, even such which had been treated according to other methods previous to the employment of the bimanual method of turning), we arrive at the following numbers:

Total number of cases.

	No. of Cases.	Deaths.
Hofmeier	47	4
Behm.....	53	4
Lomer.....	136	13
	<hr/> 236	<hr/> 21 <i>i. e.</i> , about 10%.

Thus, in placing our statistics in the most unfavorable light possible, we have only 10% mortality, out of such a large num-

ber of cases as 236 occurring in hospital practice, where so many circumstances concur to deteriorate the prognosis. If, however, we consider the cases treated by combined version alone, we have the following results:

Cases treated by combined version.

	No. of Cases.	Deaths.
Hofmeier.....	37	1
Behm.....	40	0
My cases.....	101	7
	<hr/> 178	<hr/> 8 i. e., 4.5% mortality.

These are results unequalled hitherto.

To resume: According to text-books, the general mortality of placenta previa is between 40% and 24% (Spiegelberg, p. 368; Charpentier, p. 371). Large collections of cases of different operators give the mortality as about 25% (see table). Single operators have often had much better results than these (see table). Our statistics, based on a considerable number of cases treated by different operators under unfavorable circumstances, including deaths in childbed from puerperal fever, prove that if the suitable cases are treated according to the method of Braxton Hicks, placenta previa is no more such a dreadful complication of pregnancy. Simpson¹ could compare the mortality from placenta prævia with the mortality from yellow fever and from cholera. The time for such comparisons is past!

Prognosis for the Children.

In consideration of such a prognosis for the mother, the prognosis for the child is of but secondary importance. This every practical and busy accoucheur will, I believe, readily admit. It is one of the most surprising facts in medicine that the life of the mother and that of the child have at times been considered equivalent. What happens to a family if a child is born dead? The family will live along as it did before; if it was their first child, the parents will look forward to another one; if they have many children already, they will not feel the loss very severely. How different to this is the eventuality of the mother's dying! What a fearful loss to the husband, what an

¹ Selected Works, p. 180.

irreparable loss to the other children who depend so completely on her! The child's life may always be replaced by another, *nothing* can be done to give a compensation for the death of the mother. It seems really an absurdity to place the life of mother and child on the same footing. Would it not be difficult for a physician to make the husband understand what he means, in asking him which of the two he values most? Would he not answer, "Why, doctor, have I no common sense? You ask me whether you shall endanger my wife in order to save my child? Why, you cannot mean such a question seriously!"

But let us inquire, what is it we give up in sacrificing the child? A life of little value, as statistics show us. I give here a table showing the views of the different authors on this subject.

Mortality of Children in placenta previa according to different Authors.

Schwarz.....	75%.
Hecker.....	67%.
Barnes.....	64%.
Müller.....	64% (average of 2,360 cases).
Fritsch.....	60%.
Spiegelberg.....	50%.
Braun.....	50%.

These numbers refer mostly only to the question, whether the child was born alive, not whether it remained alive. A very important question is thus omitted. Behm¹ had, for instance, 78.5% living children immediately after delivery. Of these, so many were born prematurely that definitely 71% of the total number were dead; this is just the contrary to his primary result. Kuhn² followed the fate of the children of forty-six cases of placenta previa and found that two months after delivery only two of them were alive! Let us take the average given by Müller as the most reliable statement of the preceding table. According to it, a placenta previa child would have about three chances out of ten for being born alive, and six out of ten for being born dead. According to Virchow,³ of

¹ Behm, l. c., p. 379.

² Kuhn, Wiener med. Jr., viii., p. 631.

³ Sander: Oeffentl. Gesundheitspflege, p. 104.

1,000 children born alive in Berlin, 292 died in course of the first year. Thus, at the end of the first year, one placenta previa child would have already lost about every one of its three remaining chances, and should it still have escaped death, there are all the dangers of children's diseases (measles, scarlatina, diphtheria) which it would have to encounter before being grown up, *i. e.*, before arriving at that age at which we would have a right to call its life equivalent to that of the mother. Such is the value of the child's life in placenta previa, and in order to save it should we endanger the mother by performing forcible extraction?

I believe that perhaps now and then a child's life is really sacrificed, if we act strictly according to our rules. It is my opinion, however, that *the danger the child runs by allowing it to be born spontaneously after turning is overrated by theory.* Practice does not accord with it, as our numbers will prove. Practice often leads to different results than to those which theory would make us expect. Thus, for instance, we would believe, according to theory, that the prognosis for the child is absolutely bad in cases in which the placenta is expelled before the infant. This is not the case. Simpson¹ showed that among 141 cases of this kind, as many as 31% of the children were born alive notwithstanding! I have not been able to ascertain in my cases whether the children remained alive or not, my notes only say whether they were born alive or dead. Now this suffices in order to prove an opinion as to the question whether the child is more apt to die when born spontaneously (*i. e.*, with slow and prudent extraction) than when extracted immediately after turning has been accomplished according to the practice generally employed. What have been our results as to the children? Of 139 children, 62 were born dead, *i. e.*, 45%, of 103 cases treated by combined turning (after deducting the children which were dead before turning) 34 were born dead, *i. e.*, 31%. These *may* have perished on account of the method; it is difficult to prove the contrary; at all events this number gives the *utmost maximum* of mischief that may possibly have been done to the child, in allowing it to be born spontaneously. We will return to this subject

¹ Selected Works, p. 197.

further on. In adding together the results of Hofmeier's, Behm's and my cases, as to the children mortality we find:

Mortality of children in cases of placenta previa treated by combined turning.

Hofmeier, 37 cases,	23 deaths	=	65%
Behm, 40 "	31 "	=	77.5%
My cases, 101 "	51 "	=	50%
<hr/>			
178 cases,	105 deaths	=	60%

These numbers are not quite uniform, because in Hofmeier's and in my cases the question was only considered whether the child was alive when born or not, while Behm's cases refer to definite results. However, I think we can draw the conclusions from them that the prognosis for the child seems not to be worse whether it is extracted immediately after version or whether it be allowed to be delivered by Nature. In comparing our numbers with those given by the different authors (in table), the reader will be convinced of this. We close this chapter with the following conclusions:

1. The average mortality of children born spontaneously after turning is not superior to that of children extracted immediately after turning. The danger the child runs by not extracting it has therefore been overrated.

2. A child's life is of so little practical value in placenta previa, compared with the life of the mother, that, should it be endangered by leaving it to be born by natural powers, we are entitled to sacrifice it in cases in which we would endanger the mother by quick extraction. The only objection that could be made to the method is, therefore, of no serious importance. The advantages of the method may thus be summed up:

1. It does away with the tampon and with the dangers of infection and loss of time this involves.

2. It allows us to operate early, *i.e.*, when not much blood has been lost.

3. It arrests hemorrhage with great certainty.

4. It gives the patient time to rally, gives time for the cervix to dilate, for pains to set in. It therefore prevents post-partum hemorrhage (laceration of the cervix, atony of the uterus).

I now enter into the details of my 136 cases.

Frequency.

These 136 cases occurred amongst :

	3,864 polyclinical deliveries (mostly pathological cases),
and	3,018 clinical deliveries (mostly normal cases).

Total, 6,882, *i. e.*, in 1,000 cases there occurred twenty cases of placenta previa. This, referring to hospital practice, is naturally much superior to the general frequency of placenta previa. The latter has been estimated by Schwey¹ as occurring once in 1,564 deliveries (among 519,328 deliveries in the Grand Duchy of Hesse), and by Müller² once in 1,078 deliveries (average of 876,432 deliveries). It seems as if placenta previa occurs somewhat more frequently than this in Berlin. Behm's collection comprises the cases which occurred in the Charité Hospital, and my collection includes all the cases of the University Hospital for Women. We thus have together about the sum of placenta previa cases which occurred in Berlin in course of a known space of time. Behm's 53 cases were observed in two years, our 136 cases were observed in three and a half years. The annual number of cases occurring in the Charité would be about. . . . 26

Our annual number.	39
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Total.	65
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This would be about the total annual number of cases in *hospital practice* in Berlin. This number, not including the cases of placenta previa which occurred in private practice, gives us only *the minimum of frequency* in Berlin. Now, according to the estimate that one case of placenta previa happens among about 1,000 deliveries, we should expect at least about 65,000 annual deliveries in Berlin. This is not the case, the average number of deliveries in the last five years was 46,000 in Berlin, as statistical returns prove. Thus we can conclude that placenta previa occurs in this city in the minimum once in about 723 deliveries.

Primiparæ ? Multiparæ ?

Among the 136 cases we only had 11 primiparæ, *i. e.*, 8%.

¹ L. c., p. 108.

² L. c., p. 148.

The preponderance of multiparæ, especially of such who have had many children, is shown by the following table:

82 women had been delivered at least 5 times before = 60%.

16 women had been delivered at least 10 times before 12%.

In all compilations of cases of placenta previa, the authors have already insisted upon the preponderance of multiparæ. The relative number of primiparæ has been differently estimated. Thus Simpson found 30%, Charpentier 19%, Müller 17%. Our number approaches nearest to the estimation of King with 10%.

Age.

The following table shows the result of our inquiry as to the relative age of our patients. The youngest was 21, the oldest 46 years old

16, i.e., 12, were between 20 and 25 years.

46, i.e., 34%, " " 20 " 30 "

81, i.e., 60%, " " 30 and upwards.

7, i.e., 5%, " " 40 and upwards.

Placenta previa lateralis or centralis.

It is difficult to distinguish central and lateral insertion of the placenta if version is performed early. An os allowing only one finger to pass would, for instance, show us the placenta totally inserted above it, because on examination the finger touches only placental tissue. A renewed examination after dilatation of the os might, however, afterwards show us that the border of the placenta can be reached, that it is placenta previa lateralis only. I believe, therefore, that this distinction has but little practical value. The *symptom* is and remains the great thing. If hemorrhage is profuse, it is a difficult case, whether the placenta be central or marginal. Thus it has already often been stated by authors that the gravity of the case does not always depend upon the different manner of insertion. For the sake of completeness, I have tried to classify my cases as placenta previa centralis and lateralis, counting with the former only those cases in which the diagnosis was quite certain. I have noted 25 cases with central insertion, i.e., 19%, and 111 cases with lateral insertion, i.e., 79%. Amongst the former, in five cases the child was born after expulsion of the placenta. Nothing is more variable in the literature on placenta previa

than the statements as to the relative frequency of central and lateral insertion. I repeat that, when acting according to our own principles, it is difficult to distinguish both with certainty and that such distinction seems to me to have but little practical value.

Presentations of the children.

We have a remarkably great number of transverse presentations in our collection, more than I have found in other publications. Should this be a consequence of our early interference? Possibly transverse presentations are really as frequent as in our collection, but other operators, interfering later, find a certain number of them spontaneously changed into head and breech presentations.

We found:

In 70 cases = 51% head presentations.

“ 44 “ = 32% transverse presentations.

“ 12 “ = 9% foot and breech presentations.

To show the difference of these numbers from the statements of other authors, I will mention some of their observations: Charpentier¹ found among 1,148 cases of placenta previa:

66% head presentations.

24% transverse presentations.

9% foot and breech presentations.

King² has

87% head presentations.

5% transverse presentations.

6% foot and breech presentations.

Premature labor or labor at term.

Our inquiry on this subject leads us to about the same result King³ arrived at, viz.: that premature labor occurs in about in one-half of all the cases of placenta previa. In 56 cases, *i. e.*, 40%, the child was noted as having been full grown. In 38 cases of premature labor, *i. e.*, 29%, we possess statements as to the length of the children, viz.:

¹ Traité d'Accouchement.

² L. c., p. 762.

³ L. c., p. 762.

8	were	from	35	to	40	cm.	long	(8th	month).
13	"	"	40	"	45	"	"	(9th	month).
17	"	"	45	"	50	"	"	(10th	month).

These numbers only refer to cases of which we had exact information as to the length of the children. In the remaining 41 cases the latter was not mentioned.

Methods of Treatment.

Spontaneous delivery after rupture of the membranes occurred 30 times (including a few cases of forceps), *i. e.*, in 22%. In four cases hemorrhage did not cease after simple rupture of the membranes, so that turning had to be resorted to. 101 cases, *i. e.*, 75%, were treated according to Braxton Hicks' method—these include the foot and breech presentations. The whole intact ovum was expelled with the placenta in 5 instances.

The condition of dilatation of cervix in these 101 cases was noted as follows :

In 14 cases the os was completely or nearly completely dilated.

In 51 cases two fingers could be passed through it.

In 13 cases only one finger could be introduced.

In 23 cases no reliable information exists on this subject.

Relative mortality of children when born in head presentations and when born after turning.

Of the 30 cases delivered in head presentations there were.

Children born alive, 23 = 77%.

" " dead, 7 = 23%.

Of the 101 cases delivered by turning and slow extraction there were :

Children born alive, 51 = 50%.

" " dead, 50 = 50%.

I need not mention that the gravity of these two collections of cases, being absolutely different, the difference in these results must not surprise us.

Danger turning represents for the child.

I have tried to determine whether my numbers prove that

a child runs more risk of being born dead when turning is performed early than if the operation is executed when the cervix is completely or nearly completely dilated. My inquiry had the following result:

1. Turning performed with completely dilated cervix:

14 cases, of these $\begin{cases} 8 \text{ children living.} \\ 6 \text{ " dead.} \end{cases}$

2. Turning performed with cervix allowing 2 fingers to pass:

51 cases, of these $\begin{cases} 26 \text{ children living.} \\ 25 \text{ " dead.} \end{cases}$

3. Turning performed with cervix allowing only one finger to pass:

13 cases, of these $\begin{cases} 7 \text{ children living.} \\ 6 \text{ " dead.} \end{cases}$

In 8 cases the children were allowed to be born quite spontaneously after turning, not the slightest attempt to extract them being made.

8 cases, of these $\begin{cases} 4 \text{ children born living.} \\ 4 \text{ " " dead (one macerated).} \end{cases}$

We thus again come to the conclusion that the prognosis for the child does not differ, if turning be performed early or late. Its chance of surviving seems even not to be worse when labor is *completely* left to Nature after turning, but on this latter point our experience is too small to allow us to form a definite opinion.

There is another way of proving that the prognosis for the child cannot be influenced to a great extent by turning and slow extraction, that other circumstances are of much greater importance, warning us not to judge "*post hoc, ergo propter hoc.*"

Of our total number of 139 children (including 3 cases of twins), 62 were born dead. Let us consider the latter more closely, asking ourselves how many of them *possibly* or *probably* died on account of our proceeding. Amongst these 62 children there were:

2 not viable (born before 28th week).

3 macerated; one hemicephalus.

3 had prolapse of pulseless funis at our arrival.

5 had pulseless funis found while turning.

7 born dead in head presentations.

20

These deaths were certainly not caused by the method. There remain 42 cases, *i. e.*, 31%, in which *possibly* death of the child occurred through our manner of proceeding. Among these 42 cases there were 14 in which no fetal heart beatings were heard at the time turning was resorted to. Subtracting these, there remain but 28 cases, or 20%, in which the child *probably* died during turning and extraction. Among these 28 cases there were 4 cases of prolapse of the pulsating funis.

These numbers, together with my previous arguments, prove sufficiently, I believe, that the prognosis for the child is bad in placenta previa apart from the method of treatment adopted, that consequently it is wrong to endanger the mother's life by trying to save the problematic life of the child.

Cases ending fatally for the mother.

Among our total number of 136 cases, we had 13 deaths. In three cases, the patient was moribund at the time of our arrival. These were: Case I., I. P., brought into the clinic in a moribund state with placenta previa lateralis. She was pulseless, had Bright's disease, and enormous edema and anasarca; the pelvis was contracted, the conj. diam. measuring $11\frac{1}{2}$ cm. The child lay in head presentation, turning was performed, and the child found to be dead. The mother died undelivered; no more blood appeared after turning. The post-mortem showed no rupture or lesion of the cervix or of the uterus.

Case II. Moribund patient with placenta previa centralis. The cervix was completely dilated, turning from head presentation was performed, and followed by slow extraction. No hemorrhage after version had been accomplished. The efforts to rally the patient remained fruitless. She died two hours after delivery.

Case III. The prognosis in this case had been pronounced as absolutely bad on our first sight of the patient. She was pulseless, with central placental insertion. The cervix was

not completely dilated, turning was performed and the child was slowly extracted. The mother died without further loss of blood. Now, if we want to study the advantages of our method of treatment, we cannot include in our statistics cases of this kind, for any method and every treatment would have had the same fatal result. We must also deduct from our total number of 136 cases 30 which passed off spontaneously, among these there was one fatal case. Finally we must deduct 2 fatal cases (5 and 6), which our notes tell us most positively were not treated according to the method. *In both cases extraction was performed immediately after turning, although the cervix was not completely dilated, in order to save the child.* In both cases fatal cervical lacerations took place; one child was born dead, the other died a day later. These two cases are very instructive, as they show exactly what the method tends to obviate; they prove that it is not only useless to try to save the child's life under such circumstances, it is even extremely dangerous, for we risk losing both child and mother. Both cases were particularly difficult, the children being very large. The one child weighed 4,000 gms. and was 57 cm. long; the other child had a length of 56 cm. In the one case the pelvis was contracted and extraction very difficult, strong pressure from above had to be resorted to in order to force the head through the brim of the pelvis. Now, although these circumstances complicated these cases considerably, we are convinced that had the children been given up, had a more expectant treatment been adopted, had perforation eventually been performed, the mother would probably have been saved. Turning had been executed with success, and hemorrhage had ceased. There was no reason, no indication whatever, to hasten labor, if not in the interest of the child.

After deducting these cases from our total number, there remain 101 cases with 7 deaths applicable to criticise the method. Cases of puerperal fever are included in this number.

Three patients died of puerperal fever acquired probably by direct primary infection (case 7, 8, 9). Two other patients showed already septic symptoms at the time they came under our treatment. In the first case, ten repeated fruitless attempts to turn had been made by a practitioner in this city; the pa-

tient had a temperature of 38° , pulse of 140, and putrid discharge when she came into our treatment. Besides this, a midwife had given her several doses of ergot. Combined turning was performed, and the leg of the child brought down, but the cervix would not dilate, perhaps on account of the administration of ergot. The septic symptoms of the patient becoming more and more alarming, extraction was made after having waited fruitlessly twelve hours for the cervix to dilate. Post-partum hemorrhage from laceration of the cervix ensued, and the patient died of septicemia. In the other case (11), the patient had been examined previously to the arrival of the assistant by two midwives, two practitioners, and the student. Her temperature was 39° , she also had putrid discharge, and subsequently died also of septicemia.

In one case (No. 12), the placenta had to be removed with the hand after the child was born. It was the patient's third delivery, after each of which the placenta had been removed thus. Post-mortem examination revealed air in the veins of the uterus and in the heart. This case can hardly be counted, as it is very improbable that the method of combined turning had anything to do with the death of the patient. I will here remark that manual removal of the placenta was necessary 5 times among our 136 cases.

The last fatal case I have to mention (No. 13) is the following. Turning had been performed, and hemorrhage had ceased. Slow extraction of the child was then made, ending in its delivery one and one-half hours after turning. Nevertheless, slight rupture of the cervix occurred in extracting the head. The laceration was stitched, and everything went on well up to the seventh day, when suddenly a profuse arterial hemorrhage recurred. Assistance could not be promptly obtained, the patient living at a great distance. She died a few hours later of acute anemia.

Resuming the cases with fatal issue for the mother we find:

3 were moribund at our arrival.

2 cervical lacerations, child being forcibly extracted. } Not applicable

1 died after spontaneous delivery in head presentation. } for a criticism of the method.

3 died of primary infection.	} Applicable for a criticism of the method.
2 were septic when they came under our treatment, and subsequently died thereof.	
1 manual extraction of the placenta post partum. Air in the veins and in the thoracic cavity.	
1 cervical laceration. Death on seventh day post partum of arterial hemorrhage.	
Total, 13	

As to the morbidity of the remaining patients, only five of them had illness of any importance in childbed; they all recovered.

Remarks on the Method.

1. *How should we treat cases of flooding occurring during pregnancy?* In many cases the tampon is necessary here and will probably always remain so. But should we not induce artificial labor in such cases? I must confess that our cases have not proved to us the necessity of such a proceeding. When strong hemorrhage occurred in pregnancy, we used the tampon and examined a few hours later to see whether the cervix was sufficiently dilated to allow one finger to be passed, and to permit of turning to be performed. In this sense our method may be counted, perhaps, among the proceedings having the object to induce premature labor in placenta previa. Operators who have lately followed this plan have had very good results. Murphy,¹ for instance, reports fifteen cases treated thus, of which all the mothers recovered. This again shows that the adoption of active measures early, is the right thing for placenta previa. I can, therefore, not agree with Byford,² who says that "the most cases of placenta previa are over-treated . . . that we should rely more on the plug . . . that unnecessary interference by introducing the hand, and turning, has had more victims than unaided Nature would have made in placenta previa."

As to the necessity of applying sponge tents in pregnancy in order to induce premature labor, I believe that such cases are rare, possibly they occur more frequently in private prac-

¹ British Med. Jr., 1884, p. 215.

² Treatise of Obstetrics, 1870, p. 399, 402.

tice, where the physician is called at the first appearance of hemorrhage. To use sponge tents *as a general method* of treating placenta previa, as Jungbluth¹ lately advised to do, can but cause any busy obstetrician, who has had ample experience with such cases, to smile. Those are methods which will perhaps do very well in single exceptional cases, but must not be recommended as applicable to all cases.

2. *Is bimanual turning an easy operation?*

This is a very important question; for of what good would the operation be, if the ordinary practitioner cannot perform it, if it necessitates the dexterity of a specialist? It would never find general application and would practically be useless. Now to this I can answer that I, for my part, had overrated the difficulties of the operation, that I was astonished at the facility with which it could be accomplished, and that I have not found any difficulty with it since. I have even allowed the students to perform it under my direction, and saw that they mastered it easily too. All I can say on this subject is, therefore, "*try it, and then judge for yourself!*" Kaltenbach² is right in saying that the difficulties of bimanual version have been emphasized especially by authors who, having theoretical doubts as to its practicability, have not tried it themselves. He insists upon the fact that several circumstances concur in placenta previa to make it particularly easy in these cases.

Martin³ has even advanced the opinion that turning ought *generally* to be performed thus, that the introduction of the whole hand into the uterus ought only to be exceptionally made use of.

I will remark that we *always* use chloroform, and that it is perhaps partly on account of this circumstance that we have found the operation so practicable. In this point we fully agree with Braxton Hicks.⁴

3. *Can we rely upon hemorrhage ceasing after turning?*

I can only repeat here what I already said before, that *it is a matter of fact that hemorrhage does cease*. May this or that theory as to the cause of hemorrhage in placenta previa be men-

¹ Jungbluth: Volkmann, Klin. Vortr., p. 255.

² Zeitschrift f. Geb. Gyn., Vol. iii., p. 186.

³ Leitfaden der Geb. Operat., 1877, p. 32.

⁴ L. c., p. 232.

tioned against this, an experience gathered in 180 cases silences all such criticisms. Now, I do not mean to say that a clean napkin placed beneath the patient will not be soiled with a little blood during the expulsion of the child, but any strong and persistent hemorrhage does not occur, and if so it can immediately be stopped by exerting one or two tractions on the foot. It is convenient to have the patient lying transversely with her buttocks resting on the edge of the bed and her legs on a chair. Thus the hemorrhage can always be controlled and tractions easily executed.

But cannot internal hemorrhage occur? Nothing of the kind could be observed in our cases. Only one instance is mentioned by Behm in which some blood collected above the breech of the child in the cavity of the uterus without doing further harm to the patient. Thus, whilst admitting the possibility of such an occurrence, we must call it rare and add that experience shows it to be of little practical importance.

Perforating the placenta with the finger in placenta previa centralis generally causes a very strong hemorrhage. Hofmeier and Behm have already remarked this, and I can fully confirm their observation. This is the only critical moment in the operation. The operator must be prepared for it, and must not lose his presence of mind when his hand is suddenly covered with a stream of blood; he must remember that the most alarming hemorrhage will cease with positive certainty when he pulls down the leg of the child. I have sometimes found it difficult to penetrate through the placental tissue with my finger on account of the rigidity and resistance of the tissue.

4. *How long must we wait for the child to be born by natural powers?*

Experience shows that we have not to wait long, for pains set in quickly after turning and generally the delivery is finished in about one and a half to two hours after. Naturally this depends greatly upon the degree to which extraction is employed. Behm generally allowed the children to be born quite spontaneously and had to wait from half an hour to four hours. In one instance, eleven hours passed before the child was born. In our cases we exert gentle traction now and then until the

cervix is completely dilated, and we then slowly increase the number and the strength of the tractions.

5. *Ought the method of rupturing the membranes in cases of head presentations be abandoned altogether?*

This is by no means our opinion, as can be seen by the fact that we treated thirty of our cases thus. Circumstances must decide in each special case which treatment is most likely to be the best; no general line of conduct can here be given where so much depends upon the individual case. When the placenta is only felt marginally, when the head has entered the pelvis, when pains are strong and hemorrhage not very profuse, then rupture of the membranes seems to be the right thing. It must, however, not be forgotten that, in adopting this method of treatment, the chances for effecting an easy version are lost, and as sometimes hemorrhage does not cease after rupture of the membranes (five times in our cases), turning has then to be resorted to under unfavorable circumstances.

One of the most careful and most scrupulous compilations of placenta previa cases has been given us by King.¹ The author inquires here which is the best method of treatment in placenta previa in order to stop hemorrhage. He comes to the following conclusion: that the evacuation of the liquor amnii, if performed before the os is dilated, is an unreliable agent and ought not to be classed as a means for controlling the hemorrhage. Some of the other conclusions King arrives at are of interest, as they coincide with our views. His numbers prove, for instance, that "the tampon is of no decided value in partial presentations of the placenta, and of none whatever in complete presentations." That "the mortality to both mother and child is much heavier in cases treated by version subsequent to the use of the tampon than in cases in which version alone without the tampon took place." In his summary King says, "from the foregoing study of the treatment of the cases reported in this collection, we have found very unsatisfactory results from every manner adopted for controlling the hemorrhage while the os dilates." I hope that this paper will convince the reader that we *do possess* reliable means to control it, that it will encourage him to try it. We by no means

¹ L. c., pp. 782, 777, 785, 791.

believe that our method is the only correct one, that all other plans of treating placenta previa are wrong. We have already mentioned that Hicks and Spiegelberg, although adversaries of bimanual version, had nevertheless very good results. Very often it is the operator who settles the prognosis in placenta previa, and not the fact that he adopted this or that method.

In perusing the admirable historical review on placenta previa given by Müller, one is struck by the fact, that every new theory and every new method of treatment has invariably been considered by its author as the *only right one*. By-and-by then it was found that the expectations based thereon were exaggerated, that the method when applied by others did not give the brilliant results expected. This was the case especially when *one* method was recommended for *all* cases of placenta previa. Will the method of Braxton Hicks have the same fate? Possibly, but then it must be superseded by a still better method, giving still better results. Meanwhile we must assert that results similar to ours, and basing on such large numbers, have not yet been published. It is not a method which pretends to be unique and universal. Many cases are best treated according to it; others are best treated expectantly or by rupture of the membranes. We do not pretend to do away with the tampon altogether, but we wish to show that its application is very often superfluous and even dangerous. As in all other departments of medicine, individualizing is necessary here also; we hope, however, to have convinced the reader of the advantages and of the practicability of bimanual turning in placenta previa and of the greater security spontaneous birth of the child affords to the mother than forcible extraction.

ANTEFLEXION OF THE UTERUS AND ITS ASSOCIATED PATHOLOGICAL CONDITIONS: THEIR PREVENTION AND TREATMENT.

BY

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IN the first part of this paper, in the September, 1883, number of this JOURNAL, I undertook to show the following: The normal uterus is flexed forward, and so placed in the pelvis that downward pressure does not act directly on it. It is so suspended, surrounded and sustained by an elastic medium, the pressure of which is regulated by the heart's action and the contractility and elasticity of the muscles and fasciæ of the blood-vessels and the abdominal walls—the vital pressure—that the weight of the normal-sized fundus has but a slight tendency to increase the anteflexion; the body of the uterus being sustained and protected very much as though it were surrounded by water. Many authors have overlooked these facts, and have been led to lay too much stress upon the effects of falls, blows, etc., in producing anteflexion, and have taught that bending closes the uterine canal mechanically, and thereby creates the symptoms, etc.

Anteflexion does not constitute the disease, but is the result of imperfect development or of disease—a condition associated with certain pathological changes in the tissues of the organ. The causes of these pathological conditions are, 1st, congenital influences; 2d, general and special causes tending to prevent perfect development, and thereby inducing premature atrophy and degeneration; 3d, diseases which soften the walls, or those which enlarge the fundus, or lengthen the cervix, or relax or overstretch the suspensory ligaments, etc., etc.

Except in rare instances, I do not think that the mechanical effect of the flexion causes the symptoms, dysmenorrhea, etc.; but they are due to the degenerate, hyperæsthetic, and more or less contracted or stenosed condition of the tissues, chiefly at or about the os internum. In other words, the

mere fact that the uterus is anteflexed has, as a rule, little to do with the disease or symptoms.

I now return to the continuation of my subject.

PREVENTION.—The true etiology of a disease, of course, indicates the steps to be taken to prevent it. As long as delicate children are born and enabled to reach maturity, women will suffer with small, imperfectly formed, and degenerated genital organs. We may expect to find anteflexed uteri among those children who, though born of healthy parents, are underfed or badly nourished, whose health was crippled by disease or bad hygiene before or during the development of the generative organs. To prevent anteflexion or imperfect development and degeneration of the generative organs, children with good constitutions should be kept in good health until fully matured, and delicate and stunted children should be so treated as to increase their physical strength and have a surplus of force for the full development of the generative organs. In the country, children are often underfed; while in large towns bad air and excessive mental tension and emotional excitement are the chief hygienic faults. Among the poorer class the law of "the survival of the fittest" is not so much interfered with, and hence the feeble, relatively speaking, seldom reach puberty. This fact is, to me, strikingly illustrated by comparing my office practice with my hospital and clinical service; in the latter a well-marked case of anteflexion is comparatively rare, while in my private practice this condition constitutes a large percentage. Good food and fresh air are always essential to good health, and are generally so considered. Yet it is not, as a rule, considered hurtful to children to begin absorbing mental training too early, and keep it up too intensely. Just when a girl is beginning to develop into a woman, while being urged at lessons and confined from twenty to twenty-three hours in the devitalized air of a furnace-heated house, she is allowed to become interested in society and emotional excitement, which too often leads to bad habits and nervous exhaustion. Of course, many girls have a sufficient surplus of strength to develop fully the organs of generation under all these disadvantages; but even these constitutionally strong ones give way if, in addition, they have a prolonged or severe attack of illness, such as scarlet fever, typhoid fever, etc., and it is rare to see a girl pass from ten

years of age to twenty without more or less anemia. It is during this time that the generative organs degenerate and lay the foundation for anteflexion, dysmenorrhea, sterility, or lacerated cervix, etc.

It is plain that good health throughout development is essential to a child. Besides good food, fresh air and exercise, the moral and mental environments should be healthful and not too stimulating and absorbing. To the constitutionally feeble, or to convalescents, besides good physical, mental, and moral hygiene, the syrup or elixir of the hypophosphites, iron, selected and partially digested food should be given. It is surprising how much can be done by careful feeding with selected food suitable to age, etc., even with feeble, puny, stunted children; but too often this careful attention about food, etc., is relaxed, and the children are left to judge for themselves, and are only seen or directed when really ill with some active disease.

Lacing without doubt has a marked influence upon the abdominal organs; it prevents normal action of the abdominal muscles and directly compresses and displaces the liver and kidneys, especially the latter, and as it adds greatly to the abdominal pressure and destroys the normal elasticity of the abdominal walls, it certainly tends to displace the uterus downward, and may increase anteflexion. The corsets which extend below the waist and compress the abdominal walls, are by far the worst.

Lacing, if begun early, can do great harm, and yet be so gradual as not to be noticed; it may, by destroying the functions of the abdominal walls, prevent their full development and permanently weaken them.

Local treatment, as a preventive in cases due to imperfect development, is hardly practicable, for usually flexion already exists when the first symptom (dysmenorrhea) occurs. In cases of flexion acquired after puberty, the prevention is indicated by the etiology. Local treatment, if given at all, should aim at stimulating healthy development.

TREATMENT.—These cases usually seek medical advice on account of dysmenorrhea or of irregular menstruation. Yet not infrequently a woman will bear with the pain for years, and finally consult a physician for sterility.

Sometimes menstruation is painful from the first, but many cases are preceded by irregular and usually scanty menstruation. I rarely make a local examination in unmarried women for either dysmenorrhea or amenorrhea, without first trying the effect of general treatment—suitable food, Bland's pills, syrup of hypophosphites, laxatives, etc., as indicated; often these measures give complete relief, they nearly always do good and prepare the way for successful local treatment. If leucorrhea be one of the symptoms—and it often is—I order half a gallon of hot water to be injected into the vagina once or twice in twenty-four hours. If the patient is under eighteen years, or is likely to be married soon, I usually defer a local examination for several months, and even longer if the symptoms are not urgent. After a fair trial of this treatment, I insist upon a local examination, for undoubtedly the longer local treatment is delayed after improving the general health, the more difficult will it be to effect a complete cure, and the more likely is it that the case will become complicated by ovarian or other perituterine disease. Under the head of pathology, the physical conditions were minutely described. I made two distinct classes for the sake of clearness, but practically the two often merge into each other.

By far the most common type among the well-to-do class is that where imperfect development is plainly indicated. Often the external genitals are small and undeveloped, and the vagina may be found narrow and short. The cervix is most frequently found near the vulva, small and hard, with its axis curved forward and parallel to that of the vagina. The anterior lip or vaginal part of cervix is short and frequently quite sharply flattened (snouty), while the posterior wall of the cervix is curved forward and put on the stretch, the mucous membrane smooth and the vaginal portion relatively long, as compared with the anterior lip. The fundus, flexed forward on the cervix, can be plainly felt by bimanual examination.

In Sims' position, with his speculum in place, a properly curved sound can be readily passed to the os internum; at this point there may be some resistance, and as the instrument passes, the patient usually complains of sharp pain. The fundus may be found sensitive to the touch of the sound, and not

infrequently its withdrawal is followed by blood, although it may have been passed with the greatest care.

Not infrequently such a case is complicated by more or less hyperesthesia and perhaps some atresia of the vagina and sensitive ovaries, with an abnormal fulness especially marked on the left side in the broad ligament, undoubtedly in many cases due to varicose veins. I usually begin the local treatment of such a case by inserting a small piece of borated cotton, saturated with pure glycerin, against the anterior lip of the cervix, crowding it somewhat backward in the vagina. The cotton rolls into a ball which tends to displace the cervix backward and lift the fundus. A string is left attached to the cotton, by means of which the patient can remove it, which she is instructed to do in twenty-four hours. The object of this tampon is to soften the tissues, which it does by causing a profuse watery secretion. It seems to improve the local circulation, and invariably tends to relieve the hyperesthesia of the vagina and pelvic tissues. The pledgets are put in two or three times a week, until the patient bears the vaginal examination without pain. The patient is also instructed to use hot-water vaginal douches during the intervening days.

If the case is complicated by peri-uterine congestion or "sub-acute inflammation," the simple glycerin pledgets are replaced by similar pledgets saturated with a mixture of alum one part to pure glycerin fifteen parts, and sufficient carbolic acid to act as a disinfectant, usually one drachm to the pint. The size of the pledgets is gradually increased. As a rule, an ordinary case will be much changed in two or three weeks by this preparatory treatment; the vagina is less sensitive and larger, the cervix is less pointed and its axis directed farther backward, the peri-uterine tissues are softened and less painful upon examination. Occasionally a case of long standing, in which the nervous system has been seriously affected, will require six or eight weeks of such treatment before the next step can be safely undertaken.

Dilatation.—The patient being in Sims' position, the vagina is sponged out with a solution of 1 to 3000 of bichloride or 1 to 20 of carbolic acid. All instruments are kept in a solution of carbolic acid. Then a Sims' uterine dilator is inserted into the

uterine canal. The dilator, when properly curved, can be passed almost as easily as a curved sound; the blades should be forced apart about two lines. The amount of force required for this amount of dilatation will, of course, vary greatly, but usually in old cases it is considerable, especially those of the imperfectly developed type. This procedure causes more or less, and in some cases intense pain, similar to that due to menstruation. The dilator is withdrawn and a cervical protector introduced to the os internum. An applicator previously wrapped with cotton is dipped into pure carbolic acid; the free acid having been rubbed off, it is passed through the tube of the protector directly to the os internum, and thoroughly applied by turning it about and slightly withdrawing the tube and applicator. About twenty grains of iodoform are blown against the cervix as the speculum is withdrawn. In some cases the pain is immediately relieved, rarely the patient complains of cramping pain for several hours. When properly performed as directed above, and if antiseptic precautions were used, I have never seen any harm from this treatment. The first dilatation can be made at the patient's home, and she is kept in bed for the rest of the day or until all disturbance has ceased. As a rule, it is best to allow at least a week to pass before the dilatation is repeated. The glycerin pledgets can be inserted as usual. Sometimes I repeat the dilatation three times between the menses, but usually twice is sufficient, and if the dilatation can be carried to the point where the blades are four lines apart at the os externum, the dysmenorrhea is relieved in the majority of cases where there is no active endometritis or endocervicitis, and in favorable cases it is the beginning of a permanent cure. Much will depend on the condition of the general health and on the readiness with which the lining membranes of the uterine canal respond to the treatment. Where the tissues are not sensitive, and the uterus is small and atrophied, I use iodine in place of carbolic acid, or I apply electricity directly to the uterine tissues and persist in the treatment until the uterus develops.

Besides the vaginal and peri-uterine hyperesthesia, any complication such as vaginitis, acute endo-cervicitis, local cellulitis, or peritonitis should be treated before resorting to dilatation for the removal of dysmenorrhea. In those cases where

catarrhal disease or anemia are active agents in producing the stenosis or hyperesthesia of the os internum, the dysmenorrhea will of course be likely to return unless these conditions are permanently corrected. The changes produced by this treatment are: a shortening of the cervix, a nearer approach to the normal direction of the axis and shape of the vaginal cervix, and a complete alteration in the mucous lining. Any subacute congestion with faulty secretions is usually cured, the secretion of the cervix becoming transparent like the white of an egg. After this treatment, a sound can be passed to the fundus without causing pain.

In married women, if nothing is done to prevent impregnation, sterility will often be cured by this simple treatment. But in a certain number of cases this method gives only temporary relief. The stenosis is accompanied by so great a change in the muscular walls, or there is so strong a tendency to spasmodic contraction of the os internum, that simple dilatation will not cure the dysmenorrhea or sterility, and it becomes necessary to resort to Sims' operation which is a combination of divulsion and incision, with the use of a glass plug or stem pessary, or, what I like better, a hard-rubber drainage tube.

Sims' Operation.—In some cases the simple dilatation cures the dysmenorrhea, but not the sterility, and here again it is necessary to resort to Sims' method of operation. I do the operation as performed by Dr. Sims, with some modifications. I employ the above preparatory treatment with pledgets of cotton saturated with glycerin, etc., always carefully treating any complication such as peri-uterine inflammation beforehand; for as long as the uterus is fixed by old adhesions, any dilatation or operative procedure is attended with risk. When the uterus is movable, so that the cervix with little resistance or pain can be pulled well down to the vulva, while the patient is in Sims' position, it is usually safe to operate. My patient is instructed to take a laxative and bath, to have fresh clothing, bedding, etc., and to take a vaginal douche of solution of bichloride one to three thousand, preparatory to the operation. Instruments needed: one doz. sponge holders with new aseptic sponges; Sims' uterotome; Sims' speculum, depressor, forceps, sound, tenacula (two), flexible director, uterine dilator, applicator,

hard-rubber or glass plug, a needle holder, and a threaded slightly curved needle with silver wire, to be used in case the circular artery is severed. A straight bistoury can be used in place of a Sims' uterome and, unless the tissues above the os internum are to be divided, it will do nearly as well. These instruments are all immersed in five-per-cent solution of carbolic acid. The patient being etherized, is put in Sims' position; the vagina is carefully sponged out with the one to three thousand bichloride solution, a strong tenaculum is fixed in the anterior lip of the cervix which is pulled to the vulva. With the sound the uterus is explored; the dilator is then introduced and the uterine canal slightly dilated so as to allow the blade of the uterotome to pass readily. The straight blade of the latter is then introduced, cutting edge backward, up to the os internum or the point of flexion; the posterior wall of the cervix is divided in the median line for half an inch or more, according to the length of the infra-vaginal part of the cervix; the lining mucous membrane is divided the full length of the cervix, and the muscular walls are also divided for some distance under the external mucous membrane covering the infra vaginal cervix. As a rule, there is very little bleeding, and where the circular artery is normally placed it will not be divided. It is a mistake to divide all the muscles, and especially the vaginal mucous membrane of the cervix, as far as the vaginal junction. After this incision, the dilator is introduced and the os internum freely divulsed. Dr. Sims always divided the anterior wall at the os internum with his uterotome, but I have lately trusted to the dilators to overcome all constriction at this point, for I have never seen a full-sized plug introduced after incision as far as the os internum without the free use of the dilators. The dilatation should be done slowly so as to give time for the tissues to stretch and not tear. In many cases the amount of force needed to overcome the constriction is very great, not less than the full force of the grip of one hand; if this force is kept up for a minute or so, it will usually suffice, and it is well to repeatedly try to introduce the plug, and to be satisfied with the amount of dilatation when a full-sized moderately curved plug can be introduced to its full length (two to two and a quarter inches) and remains in place without being held in position. It is at this point of the operation where fail-

ure is often made, for many times the point of the plug, especially if straight, strikes against the os internum at the point of flexion, and forms a pouch behind it, which, with the elongation caused by the stretching of the cervix, allows the tube to enter the cervix almost to its full length. This leads the operator to think that it has entered the cavity of the fundus, and he inserts his tampon, etc. Now the test whether the plug has passed the os internum and entered the fundus is that it will remain in place and not tend to spring back and out as soon as pressure on it is removed. Once it is well through the os internum, it is grasped by the circular fibres and remains in place. In some cases, of course, it is more difficult to pass the tube than in others. Where the flexion is decided and the lining membrane of the cervix lax, the point of the plug invariably glides behind the os internum and puts the lining membrane and the posterior wall of the cervix on the stretch. More than once I have seen this operation done and the tampon put in to hold the plug forcibly in place, and afterward had the opportunity to prove that the cavity of the fundus had not been entered by either the knife or plug. Even in the hands of Dr. Sims himself I have seen failure to get a good-sized plug in at one sitting. I am quite certain that this difficulty accounts for some of the failures to do good by this operation. In certain cases it would seem next to impossible to introduce Simpson's or Peaslee's uterotome or any straight instrument without first dilating the external os by tents or other means to relax the os internum and straighten the canal. Straight dilators may be made to pass up into the cervix two inches or even more, but they will push the os forward or to one side and will not enter the cavity of the fundus of the uterus. I have had Sims' dilator made with blades curved so that it can be passed as readily as a sound. It should be of the very best steel so as not to yield under pressure, and the joint should be sufficiently far back to allow the blades to open and remain nearly parallel.

When the plug can be readily passed into and through the os internum, it is well to apply a little pure carbolic acid on an applicator to the lining membrane of the cervix, then to replace the plug, cleanse the vagina, and blow into it a half drachm of iodoform; this is absorbed more slowly and

remains longer than any other antiseptic. Dr. Sims made it a rule to insert styptic cotton against the plug and cervix, and then over this a firm tampon, the object being to prevent hemorrhage and at the same time keep the plug in place. I do not place the pledgets until the hemorrhage is completely checked, and usually the pressure of the plug alone stops the bleeding. If the circular artery is cut, I ligate it by passing a silver suture around and twisting it. Then I insert boro-glyceride or other antiseptic pledgets of cotton sprinkled with iodoform, until the vagina is moderately tamponed, the object being mainly to keep the plug in position. After each urination the vulva should be washed with an antiseptic solution (bichloride 1 to 5,000) and an antiseptic dressing kept on it for a week. On the second day I remove the cotton if there is the least odor, and insert fresh pledgets after thoroughly cleansing the vagina; the plug being left undisturbed. On the sixth or seventh day I remove the plug, and after cleaning it and the vagina, I replace it and keep it in position with either iodoform cotton pledgets or a vaginal pessary. It is allowed to remain for a week or two longer, as the case may seem to require it. Dr. Sims usually removed the plug on the fifth day and left it out, but I prefer to retain it in place until the surface has entirely healed. Unless the plug is a very large one, it can be safely left in place during menstruation. I prefer plugs which have one or more deep grooves in them so as to permit of free drainage, and my objection to the iron or styptic cotton and large firm tampons is, that drainage is obstructed and thus the risk of septicemia increased. When the case has been properly prepared and the above precautions have been taken, the risk is very slight and the result most satisfactory. To get good results, one must do all that can be done to improve the general health of the patient, and give the proper preparatory treatment which not only lessens the chances of doing harm, but also enables one to carefully study the case and patient and eliminate complications, such as diseased tubes or ovaries, etc. If one takes a delicate and weak woman, with an imperfectly developed uterus, with a degenerate and granular eroded mucous lining, and divides the cervix *too freely*, he may relieve the dysmenorrhea, but he will do his patient harm, and sooner or later she will have an everted and

diseased cervix resembling a lacerated cervix, and requiring the same treatment.

If one dilates the cervix or divides it with a knife, while the patient has a diseased tube tense with an irritating or poisonous fluid, or a diseased ovary filled with tense cysts, any of these may burst or break and cause local peritonitis. Or if one operates, without using antiseptics or preparatory treatment, upon a patient with a diseased mucous membrane, he may cause local poisoning of the deeper uterine or peri-uterine tissues, and get what we call inflammation; but this is the fault of the operator and not of the operation. Eliminate failures to diagnose serious complication, and blood poisoning from lack of care in cleanliness or from prejudice against the use of antiseptics, and this operation becomes one of the simplest in uterine surgery.

I have never believed in the necessity of the bilateral operation. In those cases where Dr. Sims recommended it, I would dilate and drain, or divulse and keep open with a drainage tube.

If the dilatation is imperfectly done, the relief, of course, is only temporary, but when thoroughly done and repeated, say twice in two or three months, it will often effect a permanent cure in cases of even ten years' standing. I know several whom I treated as long ago as six or eight years and they are well to-day. Undoubtedly there are cases that can be relieved of dysmenorrhea, but not permanently cured of sterility; for there are some cases in which the organs have reached only a very imperfect degree of development, or have atrophied and changed so much that they cannot be fully developed by any treatment. In some of these the local application of electricity will do good, by stimulating development. It is a simple matter to apply electricity, but its use must be kept up for several weeks before it will have a perceptible effect. The galvano-electric pessary of Simpson may be used in certain indolent cases, but it must be closely watched. A good drainage tube of hard-rubber is much safer and perhaps equally efficacious. If a woman with anteflexion is to marry, she should marry early in life, for the chances of pregnancy and full development then are undoubtedly better than later. I am certain it helps these cases, for normal

erotic excitement stimulates development and averts abnormal functional derangement and bad habits.

Child-bearing is the best means of completing development and making a permanent cure of such cases. One would expect that small imperfectly developed uteri would be torn more frequently than in the average cases of labor, and this is a fact, especially if the labor is quick or premature, for the cervix requires time to get into the best condition for full expansion. I have found that more depends upon the condition of the mucous membrane at the time of the laceration and shortly after labor, than upon the size of the os or the extent of the tear. Diseased tissues heal badly and tend to swell, evert, etc.

Sponge tents.—For more than ten years I have not used tents in these cases. Without doubt, some cases can be cured with them, but their use is more dangerous than that of the dilator, and much more uncertain in results. I have my tents made with iodoform mixed in the gum, and I use iodoform and bichloride tampons to keep them in place. I never allow them to remain more than twelve hours, for they form a most excellent nidus for germs, and for a time they prevent drainage from a very much irritated, rapidly secreting mucous membrane.

DILATATION BY SOUND.—Whenever I see a specialist using a set of graded uterine bougies or sounds, especially such as are nearly straight, I know that he is travelling over an old road that was pointed out by Mackintosh many years ago. The uterus is too movable and elastic to permit of the use of sounds to the best advantage, and many a case has been sounded up to the os internum and not beyond.

To Simpson belongs the credit of the first uterotome, but Sims was the first to perfect that instrument and to understand fully how to use it. He pointed out the mistake of using automatic and straight uterotomes in the uterus.

In June, 1873, Dr. John Ball, of Brooklyn, N. Y., read a paper before the Medical Society of Kings County, on rapid dilatation of the cervix uteri for the relief of stricture, etc., and in 1877 he presented a paper on the same subject before the N. Y. State Medical Society. About this time Ellinger, of Stuttgart, advocated forcible dilatation. Dr. Ball reported a number of cases successfully treated by rapid dila-

tation. There can be no question about the success of divulsion, but there are many cases which can be perfectly cured by moderate dilatation, which is not only less dangerous, but I think gives better results in those cases where imperfect development is more marked than spasmodic stricture. For it is not a powerful and spasmodically constricting muscle that is to be overcome, but a feeble degenerate organ that needs to be stimulated to healthy development. In cases of atresia of the cervix where the vaginal cervix is large and only slightly flexed and pointed, where Dr. Sims advised the bilateral incision, I do use divulsion and insert a drainage tube, and I do the same in cases of chronic catarrh where there is indication of spasmodic stricture of the cervix. But when I am treating sterility, and have a flexed, hard, and pointed cervix to deal with, I am sure that the knife, applied as stated above, is an improvement upon simple divulsion, for the os externum tends to contract and close the opening after the latter operation, unless the tissues are torn by the instrument. Still I confess that we could more easily dispense with the uterotome than with uterine dilators. There is less risk in opening the uterine canal with dilators than with the uterotome, but dilators can do serious harm when the dilatation is carried too far, and if one uses a screw to force open the dilators, the risk is greater than when he uses his hand or hands to regulate the amount of dilatation.

USE OF PESSARIES IN ANTEFLEXION.—If the use of pessaries in such cases had never been taught, much harm would have been averted and more progress made in the right direction. Very rarely only can we afford some relief by the use of an anteflexion pessary, but at best it is only palliative and can be dispensed with. It is not so much by straightening the flexion that the pessary does good, as by preventing prolapse and perhaps by steadying the fundus. The instrument I prefer is a simple one which I devised and first used six years ago. It is easily introduced, has no joints, carries the cervix backward while it sustains the fundus, and cannot become displaced in the vagina. I usually introduce it with a small silk string attached, and instruct the patient to pull it out whenever it becomes uncomfortable. But I must confess that now I very rarely use any pessary for anteflexion. As to the use of stem-

pessaries for straightening the canal, they may do good by stimulating development, but many of them are dangerous instruments.

In some cases, especially those affected with chronic catarrh, I use a glass plug, or I should say drainage tube, made of tough glass, or better, hard rubber, with one or more deep grooves in it. The object is to keep up perfect drainage and perhaps stimulate development, but not with the idea of using a splint on a fractured or bent uterus. The good results obtained by the use of glass stem pessaries are undoubtedly due to their help in stimulating development, in perfecting drainage, and permanently overcoming the tendency to atresia and hyperesthesia at or near the os internum. Cases of anteflexion acquired after full development I treat very much in the same way if there is stenosis of the cervix or subacute endometritis, but often the peri-uterine inflammation, diseased Fallopian tubes, or other disease associated with it, is the condition to be treated.

COMPLICATIONS.—Anemia, chlorosis, or phthisis are always serious complications, and unless we can improve the general condition, we might expect to relieve some of the symptoms, but not to cure the imperfect development of the uterus. Catarrh or a low grade of inflammatory condition of the mucous lining would be expected in a degenerated and imperfectly developed uterus, and in various stages it is a common complication. In most cases it is confined to the canal of the cervix, and is usually cured by the same treatment which develops the uterus. When the lining membrane of the fundus is inflamed, the condition is more serious and the symptoms are so changed that the affection is not classed as anteflexion. In these cases, dilatation, drainage, and careful and efficient applications are essential to a cure, and where these fail we can be pretty certain that the disease has extended to the Fallopian tubes, and then we must remove the tubes and ovaries to effect a cure. The tubes are much more frequently involved than has been heretofore supposed. It is this fact which makes gonorrhea and septic endometritis so serious a complication. Diseased tubes explain the majority of peri- and para-uterine complications. The tubes cannot be dilated, drained, and treated locally, and the disease, once there, takes its course.

Some of the conclusions may be concisely stated:

1st. There is undoubtedly a certain number of cases in which a marked degree of anterior curvature gives no painful symptoms.

2d. Anterior displacements are the result, rather than the cause of pathological changes in the uterus. They may add to and sometimes intensify disease, but are rarely, if ever, the primary cause.

3d. Dysmenorrhea with ante flexion is rarely, if ever, chiefly and directly due to the flexion, but the latter in some cases may aggravate the pathological conditions which are the real cause of the pain.

4th. The attempt to correct anterior displacements by the use of pessaries is rarely, if ever, sufficient to effect a cure, unless the cervix is dilated at the same time or other pathological conditions are treated. The use of mechanical supports may give some relief, but they are merely palliative, and as used by many they frequently do harm.

5th. The true morbid condition of the uterus in most cases of ante flexion is one of imperfect development, while the uterine canal is more or less stenosed by the degenerate and contracted state of the uterine tissues, and the mucous lining is degenerate and atrophied, often hyperesthetic, especially in that part of the organ where the circular fibres are most powerful and contracted, at the os internum.

6th. If the above is true, the treatment obviously would be to stimulate development by improving the general health and by the local use of electricity, to relieve the stenosis by dilatation or division and divulsion, to perfect the drainage, and bring about a healthy condition of the mucous lining.

THE OPERATIVE TREATMENT OF HYPERPLASIA OF THE
UTERUS AND VAGINA, WITH SPECIAL REFERENCE TO
THE CURE OF DISPLACEMENTS.

BY

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THAT the treatment of a large majority of cases of retroversion of the uterus, so far as obtaining radical cure is con-

cerned, is unsatisfactory, is now a quite generally accepted fact. That an immense amount of good has been done in way of relief is fully conceded, and that in a very large number of cases complete cures have been the result, there can be no question. Artificial support by a great variety of pessaries has been the chief reliance, and under no circumstances could we dispense with them. We shall, by careful analysis, find, however, that most of the actual cures, so that no support is required, will be among unmarried women, and those who, being married, have never borne children, or having borne children, suffered no accident at parturition either by laceration of cervix or perineum. In these cases we shall invariably find the walls of the vagina in their normal relation, that of close apposition. But there remains a much larger class of cases among parous women where this displacement exists, and all our efforts to retain the uterus in place, after the artificial support is removed, will prove futile. I think I am not stating the proposition stronger than the experience of the profession will justify, when I say that no one of the text-books or any of the current literature of the day claim any method of *radical cure*, in this class named. In America, Thomas and Emmet have done much to popularize the use of pessaries, and many other men have aided in giving them their deserved credit, yet no one of them claims for this class of cases radical cure. Relief follows, but once the artificial means are given up, the displacement returns. Schroeder, in speaking of certain forms of displacements, says, "but we must ever keep the fact in mind that in the majority of these cases the displacement as such does not admit of a permanent cure." The essential elements, in the lesions alluded to, which prevent a satisfactory result are: 1st, hyperplasia of the uterus and vagina, and 2d, a lacerated cervix or perineum, and in many cases both. These accidents, occurring at parturition, are ordinarily the cause of the arrest of involution, so that we have an extremely large and heavy uterus and capacious vagina, with the walls entirely separated, and the canal constantly open, from loss of power of the sphincter vaginæ and loss of perineal support, with flatulency of the vagina as an almost constant symptom.

In proportion as these accidents are greater or less, we have all degrees of displacement, either backward, forward or

downward—generally the former and latter. An attempt to make a radical cure in this class of cases, without first removing these two conditions, will surely fail. Either being eliminated will not suffice. The uterus must be made smaller and lighter, the vagina must be shortened and narrowed, and the walls be brought in apposition. The various means for reduction of hyperplasia of the uterus that have been employed, it would be inappropriate to introduce into a paper like this, on this occasion; but from the days of Bennett who believed its pathology to be inflammation, to the modern and, as I believe, more rational view taught by Thomas, that of subinvolution, the practice has been substantially, depletion by local blood-letting and glycerin, and the use of cautery, caustics, escharotics, iodine, etc., etc. Added to this, Dr. Emmet introduced an important element to the profession in the frequent and abundant use of very hot water douches. In my opinion the principal value of this was and is in relieving the chronic passive congestion, and promoting absorption of the exudate consequent upon the previous attacks of inflammation. Various operative procedures were instituted by Thomas, who excised a small portion of the cervix, and by Martin, of Berlin, and Schroeder, who made what they denominate amputation of the cervix.

Emmet made an important step in advance when he discovered that, what had so long been considered ulceration was really ectropion of the cervical canal, with granular degeneration, and made trachelorrhaphy; to do which, it became necessary to remove all the cicatricial tissue and unite the healthy parenchyma, and thus set up a process of fatty degeneration and absorption, that resulted in a more complete involution. We knew before that excising a portion of the hypertrophied tonsil induced this process, but it was reserved for the genius of Emmet to apply it to this condition, practically for the laceration, but the involution followed as a necessary consequence. This is a step to be taken wherever the laceration exists and taken first. But there are a large number in the class we are treating of, in which laceration is not an element to be considered, for it does not exist. The uterus is oftentimes enormously enlarged and almost cartilaginous. For these cases I make the following operation: I remove from each side of the cervix a

V-shaped portion, cutting through the entire parenchyma from the os uteri upward, making the angle at a point as high as the vaginal junction, taking care not to open into the peritoneum. At this point considerable hemorrhage may follow the cutting, but if at all troublesome, we can easily control it by passing the superior suture directly through the vessel, which can be easily seen. The operation is best performed by long-bladed sharp scissors, curved on the flat. One stroke of the scissors should divide the half of each side with the exception of the angle which should be clearly and carefully trimmed, so that the edges shall be symmetrical. Now, if I design making both the uterine and vaginal operation, I use the catgut suture, which I tie in three knots to prevent slipping. Having put in all that are necessary to place the edges in complete apposition, without strangulating them, I replace the uterus, adjust a well-fitting pessary and proceed to make the operation upon the perineum and vagina. The only caution I would enjoin in this portion of the treatment, is, to be sure and denude the vagina high up in the centre, at least a little beyond the crest of the rectocele (if one exists). I would also caution against denuding too high on the sides of the vulva, lest we close the introitus vaginae and interfere with the sexual relation. Each suture should embrace its own amount of denuded tissue and be buried in the recto-vaginal septum, until we come to the last, which should be brought out at or near the centre, and then passed through the mucous membrane above, at one or two points, in order to bring down the posterior wall, and in this way to shorten that wall by the amount of the denudation. It is practically an amputation of so much of the mucous membrane, aside from the effect it produces upon the process of involution. In this way we convert a rectocele into the normal curve of the posterior wall, unite the separated ends of the sphincter vaginae muscles and bring the walls in their normal relation, in apposition, and shut in the uterus supported by the pessary. Everything can now remain a sufficient length of time to restore the circulation to its normal condition, to allow the process of involution to begin, with an assurance that it will be completed without any further displacement. There is no occasion for disturbance of the pessary for a long time, therefore the perineum will remain intact. Without having a

pessary here, the uterus becomes displaced, and we often attempt to replace and adjust a pessary while the perineum is yet sensitive and weak. During the entire operation I keep a constant application of very hot water by sponges and douche. The time for removal of the pessary will depend much upon the character of the case. In some cases only a very short time, in others much longer, and even changing for shorter and especially narrower ones; but with much care for a few months, we will find in a great majority of cases we can dispense with them altogether, involution of both uterus and vagina having taken place. I believe that far too much time has been lost, in this class of cases, in keeping the patient on what is called "preparatory treatment." The condition existing in a large portion of them, is one of chronic passive congestion and not of chronic inflammation. The latter term is, in my opinion, a misnomer, and leads to false conclusions in therapeutics. "Heat, pain, redness, and swelling" are not necessarily confined to inflammation, but are found in chronic passive congestion, and are relieved by the operative measures instituted. They exist on account of the displacement, and exudate from attacks of inflammation, and the fact that either pain or heat exist in the pelvis does not contra-indicate operative measures as inflammation would. I therefore advise the operation as the best means of relief, unless it can be shown by well-marked symptoms that an attack of inflammation, which lasts but a few days, is actually present. The hemorrhage is the best relief to the tenderness and exudate.

The merits of the above operation upon the uterus, over any other proposed, are, in my opinion, 1st, a much more rapid change in the entire uterus, inasmuch as it extends up into the neck farther and affects more of the substance. 2d, it leaves the organ more symmetrical. 3d, the advantage of using catgut sutures and putting the womb safely in place with the pessary. 4th, obviating the necessity of interfering with the parts for several weeks. 5th, allowing the patient to move about earlier and receive the benefit of out-door air and exercise, and lastly, operating upon the vagina at the same time, we have uniformity in the process of involution in each organ.

The bowels and bladder should be allowed to act at will—avoid catheterization if possible. Move the bowels in a very few

days if they do not act for themselves. Keep the parts thoroughly cleansed by warm water injections frequently repeated, feed the patient well, and give her the full benefit of rest and good care in every respect.

THE MODIFIED GEHRUNG PESSARY IN PROLAPSUS UTERI
ET VAGINÆ.

BY

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I HEREWITH introduce to the notice of the profession a pessary suitable to cases of prolapsus uteri of the second and third degrees. Although not entirely new in principle, it is new in construction and form. It is a modification of Dr. Gehrung's, of St. Louis, double horse-shoe pessary. It is composed of soft rubber, and anatomically constructed. Being moulded on a soft copper wire, it is adjustable and easily fitted to each case. It has been tested for over a year in my own practice with satisfactory results. It is especially servicable when there is uterine hyperplasia and much weight is to be borne. A general description is as follows: The *superior portion* is rounded (see diagram) in front at the pubis; rounded on top where the base of the bladder rests; and rounded at the sides where the base of the bladder folds over it; concave behind this rounded superior portion, to receive the cervix, fundus, and utero-vesical junction. The *superior portion* is separated behind the concavity into two branches which are curved backwards on each side of the cervix uteri; downwards along the sacrum on each side of the rectum, and forwards parallel to the upper portion; the branches uniting in front opposite the ostium vaginæ.

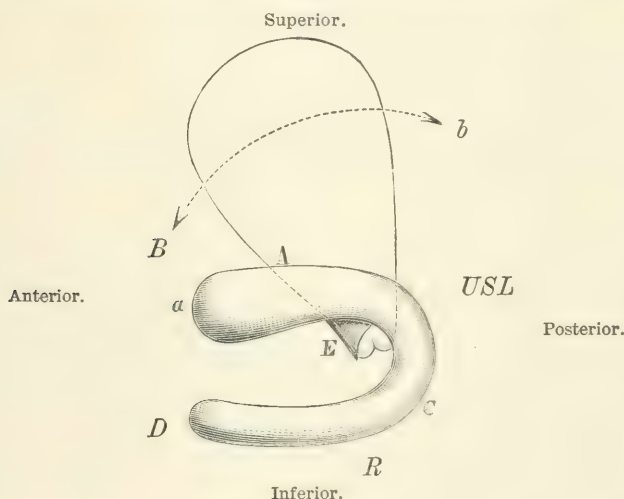
The following diagram is a side-view showing the cervix uteri resting in the concavity and may be more minutely described as follows:

Commencing at a point *a* which is opposite the pubis to *A* is the rounded superior portion, one and one-half inches from side to side, on

which the base of the bladder rests; A along the dotted line to E represents the concave portion behind *a* to A which receives the convex anterior surface of the cervix and fundus.

E represents the flexible dependent portion or apron in which the concave portion A to E terminates, so that the flexibility of E makes it a point of motion. That is, when the fundus uteri is pushed back by a full bladder in the arc B to *b*, the cervix describes a small arc forward at E; the varying conditions of the bladder rendering continuous pressure on any portion of the anterior surface of the uterus impossible along A to E or at E.

C represents the right side-branch of the pessary. The left side-branch is like it. Both branches pass backwards on each side of the cervix uteri; downwards along the sacrum, each branch being immediately under each utero-sacral ligament if the rectum is distended, thence forwards resting on the soft parts on either side of the rectum, and termi-



nate in front by uniting in a curve at D immediately opposite the ostium vaginæ. C to R is the portion that straddles the rectum, resting on the soft parts on the most dependent portion of the floor of the pelvis. D represents the point where the branches running forward unite. It also represents the ostium vaginæ, the curve D being the part of the pessary first touched by digital examination when *in situ*. Passing the finger over D, the full rectum can be felt between the branches at R. Raising the point of the finger to E, the cervix uteri will be felt on a level with the lower border of the flexible apron E and resting upon it. Pressing the finger laterally, the base of the bladder may be felt folding over each side of *a* to A and encroaching on the space below the upper convex portion *a* to A.

B along the dotted line to *b* represents the curve through which the fundus swings when the bladder is full or empty. The space contained by lines connecting the letters *a* B A would indicate the position of the

bladder when collapsed. U S L indicate where the utero-sacral ligament are joined to the cervix uteri; by lifting up the uterus in the way indicated, shows how much the tension will be taken off the utero-sacral ligaments.

I append a few cases in which it has been successful.

CASE I.—Mrs. C. came into my hands Sept. 26th, 1882. I found her suffering from uterine areolar hyperplasia, chronic uterine congestion; metrorrhagia; menorrhagia. The flooding had been continuous for four months. She had miserable health for the past two years or since the birth of last child. The case was complicated by the usual secondary or remote diseases. Anemia, impaired digestion, congestion, and functional disorder of the liver, neuralgias, inability to walk, etc.

By the first of January, 1883, most of the symptoms were improved, except the locomotion and distressing pains resulting from prolapsus uteri. I had tried an excellent pessary recommended for such cases, viz., Dr. Gehrung's, of St. Louis. It answered very well for a day or two, but was too hard and not of sufficient breadth to carry the weight. The effect on the tissues was to form a deep depression on the anterior portion of the cervix, with tumefaction of the tissues above and below the depression.

After many experiments and failures, I succeeded in producing the pessary presented to you. As soon as it was applied, she said she seemed very comfortable, and that she thought it would succeed. I visited her every three or four days for the next two weeks, and was gratified to find that it was entirely satisfactory. She immediately began visiting friends in the adjacent city, walking three or four miles every day without inconvenience. From the time the pessary was first applied, she remained under my care for six months, gaining strength the whole time. By recent correspondence, I learn that her health has been completely restored. After wearing it for three or four months, she stated that she never was so free from backache since she was a girl.

CASE II.—Was a patient of Dr. Woolverton, of the city of Hamilton, who stated that it acted satisfactorily.

CASE III.—Mrs. W. came into my care in March, 1884, suffering from complete or external prolapse of the uterus of eight years' standing. Being poor and unable to pay for an operation for the purpose of diminishing the capacity of the vagina and narrowing the outlet, I concluded to try the pessary or support as I called it. She had extensive erosion of the cervix, with great difficulty urinating, was a constant sufferer, and expected to be laid up for a month. I reduced the dislocation by putting her on her hands and knees and using gentle pressure. I introduced Dr. Gehrung's double horse-shoe pessary, recommended by Dr. Mundé, in his work on Minor Gynecology. It caused a ring of ulceration in three or four days. I should explain that I used it because I had none of my own on hand. I removed Dr. Geh-

ring's and applied my own. She has been wearing it with perfect comfort and success ever since, that is nearly four months, and began attending to her regular duties immediately, and states that she is not conscious of wearing anything, and wishes she could have seen me eight years ago.

The following letter was addressed to me, which is conclusive of continued success.

PARKDALE, August 29th, 1884.

DEAR DR.:—It is with much pleasure I am able to say that I am so much improved. I suffered over ten years. Since I came under your treatment I have been quite a new creature. I cannot express my thankfulness too much for what you have done for me. It is now six months since I commenced wearing your support, and I attribute my restoration to its use.

Yours truly, A. W.

I should say in explanation that she is entirely recovered from the complete prolapsus, and is now wearing the pessary for a slight cystocele. The success in this case, the patient being over sixty years of age, is due to extinction of the ovarian function, causing atrophy of the uterus when placed in the natural position.

CASE IV.—Mrs. S. was a young woman, about twenty-four years of age. Has had one child which was delivered with the forceps two years ago by another physician. The entire perineum, external and internal sphincter of the rectum were destroyed. I applied the pessary after advising an operation for restoring the continuity of the parts. This she intends doing as soon as her means will permit. She wore it only a short time, discontinuing its use on account of pregnancy.

CASE V.—Miss G. came to me suffering from prolapsus. This patient being a virgin, was quite unsuitable for applying a pessary. I applied it, and the relief was such that she would continue wearing it, although I advised its removal because she was not entirely free from pain. She continued under my care for only a few days, which is too short a time to test the efficiency in her case.

I will now submit a few facts based on my own experience, with a few principles and propositions as regards pessaries in general.

I. The great physiological mobility of the uterus consequent upon the varying conditions of the bladder and the small intestines, or the varying positions of the body, prevents congestion and accelerates the blood-currents of the uterus and ovaries.

II. Descensus of the uterus, while it scarcely impedes the arterial current, produces strangulation more or less of the veins and accompanying lymphatics. The venous current retreating from the fundus to the cervix passes up again at an acute angle before entering the internal iliac veins. The greater

the prolapse, the more acute the angle of venous return. It is plain that, when the uterus is misplaced and twisted, the return of the blood either by the internal iliac or by the ovarian veins must be very much impeded.

III. That, when the uterus is fixed in one position, most commonly descensus, the natural supports lose their tone and ultimately become so much elongated that they are useless.

IV. That an immovable uterus is as much liable to hypostatic congestion and inflammation as the lungs; endocervicitis being the analogue of bronchitis.

V. That the centre of gravity of the uterus in the erect position is anterior to the cervix.

VI. That a support of a prolapsed uterus must naturally be in the line of the centre of gravity of the normal uterus, and that it must be convex to enable the bladder to fold over it, concave to receive the uterus and cervix, and separated behind and below to make room for the rectum, and the concavity to receive the cervix must be flexible, so as not to chafe, ulcerate, or interfere with its movements.

VII. That those cases most suitable for the application of supports are women who have borne children, and conversely that cases unsuitable for supports are virgins and nulliparæ. That women after the menopause are exceptionally suitable for the application of pessaries on account of the cessation of the ovarian function, and a tendency of the organs to atrophy.

VIII. That no pessary *per se* will meet all the indications, and that alteratives, tonics, sedatives, local applications, etc., must be part of the treatment.

IX. That the principle of rest applies no more to a congested uterus than to a congested liver. That as soon as functional activity of the circulation can be brought about the better, or in other words, lift the womb up to its position of natural mobility, take out-door exercise, inhale plenty of oxygen.

X. Sometimes Nature tolerates a very considerable degree of displacement, but it is generally the exception. Rest is indicated where artificial means of support are contra-indicated, such as ovarian tenderness, acute attacks of endometritis, enlargement of the ovaries associated usually with an anterior displacement and anterior flexion.

XI. That soft-rubber can be worn for months without producing any odor, the only effect on the rubber being to bleach it. It can be worn through the menstrual periods without irritation.

XII. That should any odor arise after applying a pessary, it indicates that there is ulceration of the mucous lining of the vagina, destruction of tissue. This calls for immediate removal and proves the unsuitability of the pessary for the case, or else its improper application.

XIII. The true indication of the suitability of a pessary is that it is in place, and worn without any sensation of its existence by the patient, together with a diminution of vaginal and uterine discharges.

XIV. That all pessaries should be examined every day or two at first, as the insensibility of the vagina may lead to ulceration into the bladder, rectum, or even through the peritoneum.

XV. That where a heavy uterus is to be sustained together with a full bladder and the small intestines, the ordinary hard-rubber pessaries and rings are not broad enough for the purpose in very bad cases.

XVI. That gravity, the force of the respiration, together with weakened ligaments, tend to force out the uterus after the manner of a rupture. Complete prolapse takes place when the internal forces are stronger than the controlling power of the sphincter, hence mechanical support is as necessary in the one case as in the other.

XVII. That this pessary is entirely different from everything that has preceded it, is shown by the experience of eminent gynecologists as stated in their works. Emmet says he has never been enabled to apply a pessary anterior to the cervix. This pessary is applied below the line of the centre of gravity of the uterus, and consequently anterior to the cervix. Thomas says in his work on Diseases of Women, page 403, ninth line: "I very rarely attempt to sustain a completely prolapsed uterus by an internal pessary, because I usually despair of success." This pessary has been in use in one of the cases referred to in this paper (No. III.) for over four months with complete success. The uterus diminished in size from that of a California pear to normal, entirely supported by the liga

ments. The only difficulty remaining is a slight cystocele which is easily retained by the pessary. This exceedingly happy result could only take place in a patient who was beyond the menopause.

XVIII. It has been my experience to see erosions of the cervix heal, vaginal and uterine discharges disappear, and all the dragging pains from above caused by tension of the ligaments and sympathetic neuralgiæ cease; also in cases where there is boring of the posterior vaginal wall by the cervix of a congested uterus, a small amount of lifting gives great relief; for such a case I find Gehrung's double horse-shoe pessary very useful. This case would be prolapsus of the first degree.

JUNE 4th, 1884.

SUPPURATING OVARIAN CYST; PUNCTURE; DRAINAGE;
PARTIAL RECOVERY.

BY

ARTHUR DEVOE, M.D.,

Indiana, Pa.

MISS J. M., aged twenty-three years, of healthy parentage, good habits and constitution, "caught cold" during the second week in January, 1883, during her menstrual period, after overwork and exposure while engaged in nursing a sick brother. The menstrual flow was suppressed, and great pain was complained of in the region of the left ovary which continued for several weeks without external abdominal signs. Then suddenly in two days' time (from Friday morning till Sunday), there occurred extensive enlargement of the abdomen; according to patient's report, she then suddenly grew as large as she afterward appeared at time of the first tapping. Pain continued excessive for a period of six weeks after the beginning of swelling. She was greatly wasted by fever. Purgative, diuretic, and sudorific preparations were freely administered, and blisters and jugs of hot water were used externally. In March, her condition became somewhat improved as to general comfort and strength, but the swelling remained unabated. She was now brought from a distant city to her friends in this county. I first saw her on April 28th, 1883. She was greatly emaciated, pulse 120, temperature 102; suffering from sleeplessness, anorexia, diarrhea, dysuria, and the great burden of abdominal pressure. The rotund contour of abdomen, transmitted aortic pulsation, history, etc., indicated a cyst. I

soon became assured that patient could not long endure the great strain and waste that were bearing her down. Surgical assistance of some kind was the only hope. On May 4th, Dr. Chas. M. St. Clair saw the patient with me and approved and performed at once the operation of paracentesis abdominis with a common trocar and canula. About nine (9) quarts of thick, creamy, fetid fluid was drawn off at this first tapping. Cyst rapidly refilled, and the inflammatory symptoms rose higher than before the tapping; the pulse rising to 136, and thermometer registering 102½. On May 29th, I tapped her again, and a slightly reduced quantity of fluid was withdrawn and the cavity washed out with a 5% solution of carbolic acid. For a short time after this there was marked improvement in appetite, pulse, temperature, and sleep. But the cyst soon filled again. On June 11th, Dr. Thomas St. Clair, of Indiana, an experienced ovariologist, saw the patient with me, and advised puncture and permanent drainage, which was inaugurated at once. This time the cavity was washed out with weak tincture of iodine. This injection, gradually increased in strength up to the officinal tincture, was repeated many times after intervals of a day or two. A rubber drainage tube was retained constantly in the cavity. The injection was made with a common hard-rubber syringe, and the fluid withdrawn by the same means. Improvement was prompt and rapid, the cyst contracting in a few weeks to quite small dimensions. Patient has taken on considerable flesh, and is now able to do a fair share of housework. At this date, Aug. 28th, 1884, she still wears the rubber drainage tube in cyst, which now has a capacity of only a few drachms. The drainage tube has been her constant companion for over a year. Patient believes that if she should now attempt to dispense with the tube, the cyst would soon refill and enlarge. If so, an attempt at ovariectomy might then be warrantable, although the extensive adhesions unquestionably present might render removal of the cyst very doubtful. The continued presence of the drainage tube carries but little danger with it.

I would call attention especially to the following points in this case:

FIRST. The extreme pain and acuteness of all symptoms at the beginning.

SECOND. The very sudden and large development of the swelling.

THIRD. The successful drainage and injection of a large suppurating cyst through an incision effected in the ordinary operation of tapping, and through which air was frequently admitted to the cavity of the cyst.

FOURTH. The very long and apparently harmless wearing of the drainage tube.

Although the result in this case cannot be claimed as a clear success, yet it is much, considering the very bad condition of patient when treatment was begun.

In the AM. JOURN. OF OBST. for March, 1883, Dr. Geo. F. French, of Minneapolis, reports a case of suppurating cyst of the ovary treated by free incision and drainage. Dr. F.'s case came clearly and definitely within the limits of what Spencer Wells regards as the only proper field for this operation, the cyst being extensively and firmly adherent to the intestines, so that "ovariotomy could not be completed."

Schroeder, Thomas, Gross, and other surgeons caution repeatedly against the admission of air to the punctured cyst, fearing inflammation. If evacuation of the cyst is to be practised without subsequent drainage in a non-suppurating tumor, perhaps the entrance of air should be sedulously guarded against. But in any case of ovarian cyst where drainage is to be instituted, if inflammation does not exist beforehand, it is desired and often promoted by the injection of tincture of iodine, or other irritating fluid, in which case the ingress of air may be beneficial instead of injurious. Similar cautions against the admission of air to the cavity of any large abscess are generally given by surgeons. And it may be surmised that the lining surface of ordinary abscesses must be more obnoxious to all the septic influences admitted with air than obtain in cases of a cyst. But I have several times made puncture of large spinal abscesses with an ordinary trocar, regardless of hermetic precautions, yet recoveries have followed in due course.

As to the prospects of cure by puncture or incision and drainage, the article of Dr. French, above cited, refers to successful cases in this line by Dr. Ehrich, Dr. Schreiber, and Dr. Drysdale. Also by Drs. Janvrin and Thomas, of New York.

Even simple puncture has much to be said for it in extreme cases. Professor Carl Schroeder says that, "Thomas, Southam, Spencer Wells, and Chrobak have reported cases of cure following a single puncture. Telford reports a case in which collapse and death ensued after the puncture; while Burns has seen death from exhaustion follow this operation."¹

As an aid to diagnosis, as a palliative measure, and also as a

¹ Ziemssen's Cyc., Vol. x., p. 401.

means of deciding concerning the propriety of performing the operation of ovariectomy, simple puncture should always be borne in mind. Spencer Wells finds that ovariectomy is quite as likely to be successful, though previous puncture has been several times repeated. "Death rarely follows simple puncture, and one should not hesitate to perform it. Those cases in which a fatal result has been observed are counterbalanced by those in which a radical cure has followed."

Authorities are opposed to the practice of permanent drainage of ovarian cysts under any circumstances where ovariectomy can hopefully be performed. How far the mere suppuration of an ovarian cyst may render it unpromising for the higher and more radical operation, may depend much upon the constitutional condition of the patient, as well as upon the greater prospect of morbid adhesions and attachments. Moreover, in the reduced bodily and mental condition of such subjects, neither they nor their friends will always permit the major operation, but will readily assent to the trocar and drainage tube, in which they cannot see any capital danger.

CORRESPONDENCE.

CLEANLINESS IN OBSTETRICS.

EDITOR AMERICAN JOURNAL OF OBSTETRICS.

IN the October number of the JOURNAL, Dr. Clark, of Oswego, combats the idea of the necessity of thorough cleansing of the uterus after abortion. Not to call attention to the many vulnerable points in his article, permit me to point out the author's own inconsistency. The doctor mentions, as his clinching argument, a certain case in which "not bothering himself much about what might be left in the uterus," he allowed the placenta to remain until it began to stink, when, in making "a more painstaking effort" to evacuate the uterus, he had the misfortune to poison himself to the jeopardy of his own life.

If the doctor's principle were right, why did he make any "more painstaking effort"?

Again, the doctor evidently considered that the decomposed placenta contained some poisonous thing, for he attributes his

¹ Loc. cit.

own septic phlebitis to contact with the mass; would it not have been wisdom to have made the painstaking effort at an earlier date when both his patient and himself would have been spared the risk of septic phlebitis?

The details of Listerism have been modified, but cleanliness will not prove a "fleeting fashion" in surgery, and it will be a better day for womankind when thorough cleanliness is as universal among obstetricians as it is to-day among surgeons.

Very respectfully yours, DAVID INGLIS, M.D.

DETROIT, Oct. 17th, 1884.

LISTERISM IN ABDOMINAL SURGERY.

BY

GEO. GRANVILLE BANTOCK, M.D., London.

TO THE EDITOR OF THE JOURNAL OF OBSTETRICS.

DEAR SIR:—I do not know that any good is likely to result from the correspondence between Mr. Lawson Tait and Mr. Thornton, in the June and September numbers of your JOURNAL, under the above heading, in which my name has been so freely used. Nor is it my intention to prolong the correspondence on the subject in dispute. But it seems to me that I should not allow a paragraph in Mr. Thornton's letter to pass unnoticed.

Mr. Thornton says: "Mr. Tait goes to the Transactions of the Obstetrical Society of London for Dr. Bantock's results, but I will proceed to show that no one must go to those Transactions for this purpose, or he will get a false impression of Dr. Bantock's success in hysterectomy." He then goes on to mention several instances of exhibition of specimens by me before the Society, and also notices my Worcester paper. Referring to one of the former, he says: "On March 7th, 1883, he showed another five specimens, and referred to those shown three months before, *as if he was reporting a complete and continuous series, but forgot to mention* a fatal case, making the seventh death in the series." Now this sentence must have been written with the intention of misleading your readers, as the preceding ones were with the intention of confusing them; for in the next, in which he seems to have been caught napping, he says: "It was at the meeting that he spoke of the *group of cases* which Mr. Tait has *so fairly* selected for comparison with mine." Silent contempt would probably be the best way of treating the gross imputation conveyed in the italicized words of the former sentence; for I am sure your readers will not think it necessary for me to repel the imputation, and, indeed, I might have been content with the partial

answer he himself furnishes. But as silence is sometimes "taken for assent," it will perhaps be as well that I should repeat what was made plain at the time, viz.: that the specimens were exhibited because of interesting points in each case, and as evidence that, at least in my hands, the extraperitoneal method of treatment had given much better results than the intraperitoneal. This also was the gist of my Worcester paper, in which I gave a complete table for the purpose of comparison—a practice which Mr. Thornton carefully avoided in his paper at Liverpool. Not content with such misrepresentation, he continues: "On May 7th of this year, he again brought forward four successful cases, but omitted all notice of *four* fatal cases which died in rapid succession after his cases were shown in March, 1883." Now if, contrary to all precedent, except that which he alone has set, Mr. Thornton thought himself justified in quoting, without my permission, my unpublished cases from the records of the Samaritan Free Hospital, to which his position as my colleague has given him access, he ought at least to have been very careful to give them correctly. And here I say Mr. Thornton stands convicted either of wilful misrepresentation or culpable ignorance. I did *not* have *four* fatal cases in rapid succession, though, unfortunately, I had *three*. But only *one* of these belonged to the group of cases treated extraperitoneally; the others were cases of enucleation, etc., and one of these was a *complete extirpation* of the uterus with a large fibroid weighing sixteen pounds.

Mr. Thornton is welcome to all the comfort he can get out of my cases of this group up to the present time, and it is this, viz.: that while out of thirty-four cases treated extraperitoneally—being chiefly cases of supravaginal hysterectomy—I have had five deaths; he, on the other hand, has had as many deaths out of twelve up to August, 1883. I will not go to his unpublished cases since then. But I must refer to one of his twelve cases. Some three years ago, Mr. Thornton performed hysterectomy on a pregnant woman—three to four months—whose uterus was at the same time the seat of fibroid tumor of small size. The case has been erroneously styled a Porro, and has been repeatedly quoted a successful case. The fact, however, is, that the patient died some weeks after, never having recovered from the operation. This, surely, throws a lurid light on Mr. Thornton's statistics and arguments. I am, dear sir,

Yours faithfully,

GEO. GRANVILLE BANTOCK.

REVIEW.

AN ADDRESS ON ABDOMINAL SURGERY. Delivered August 25th, 1884, before the Canadian Medical Association at its Annual Meeting in Montreal. By LAWSON TAIT, F.R.C.S., Surgeon to the Birmingham and Midland Hospital for Women. (*Medical News*, September 6th, 1884.)

Mr. Lawson Tait's address deserves more than passing comment. The boldness, enthusiasm, and success of this operator have secured for him a place in the van of his specialty, and professional men the world over are eager to hear of his achievements and anxious to sound the reason of his marvellous results. In the time allotted to him, Mr. Tait was unable to do more than cursorily sketch the history of abdominal surgery and of those recent advances in which he himself has played so prominent a part. Why so prominent he fails to tell us. He candidly admits that he knows of no special reason why after the touch of his knife patients should so uniformly be restored to health, often almost to life, unless indeed the fact that he limits his practice strictly to abdominal surgery enters as a factor. The most he can do is to tell us the fundamental rules which govern his practice, and, aside from the adeptness derived from great experience, we may state the mainsprings of this practice to be absolute control over his patients and nurses, the strictest and most scrupulous cleanliness. The further factors entering into Mr. Tait's successes are, we are told, the abandonment of the clamp, the rejection of antiseptics, the discontinuance of tapping, and the resort to early operation.

Of Mr. Tait's renewed attempt to deprive MacDowell of the laurels which are justly his, and to place them on Robert Houston's brow, we have but little to say. We have elsewhere advertised on this, and refused to yield the palm to Houston, for the conclusive reason that by his own statement Houston did little more than make an exploratory incision. Surely Mr. Tait need not be told the difference between a completed and uncompleted operation! Houston cured "a dropsy of the ovary by a large incision in the side." MacDowell opened the abdomen and removed an ovarian tumor. Could there be more striking difference?

There are three prominent operations, the worth and standing of which are more particularly discussed in this address. The operative treatment of uterine myomas, the removal of the diseased uterine appendages, Battey's operation.

Mr. Tait's experience with uterine myomas leads him to the opinion that in the great majority of cases a cure can be effected by the removal of the uterine appendages. For the present, therefore, he prefers this method to supra-vaginal hysterectomy. The objections which have been brought to bear against the operation are: its mortality, and that it sterilizes the patients and destroys their sexual appetites. The first objection, he tells us, is not a permanent one, seeing that high mortality is the inevitable accompaniment of early work, and that, just as with ovariectomy, the mortality is bound to diminish with increased experience. The second objection will not hold, because in the vast majority of cases the myoma itself is a cause of sterility. The third objection is groundless, because it is an established fact that the sexual

appetite remains after the removal of the uterine appendages. There is, however, a species of myoma on which removal of the appendages exercises but little influence. This is the soft edematous myoma, and for this kind hysterectomy must be practised.

The second operation discussed by Mr. Tait is one peculiarly his own, and we had hoped to find stated in this address the diagnostic points of diseased uterine appendages. Vague, however, as are the symptoms accompanying many cases of salpingitis, the diagnosis, even for Mr. Tait, is a very difficult one. It is evident that in the majority of cases this diagnosis can only be assured by an exploratory incision. Once assured, however, the indication is a clear one, and had Mr. Tait done nothing else than call attention to the frequency of disease of the tubes and point out the sole method of treatment, his place in medicine would have been amply assured. His large experience in this field warrants him in the statement that the great majority of his patients are relieved of their sufferings by the operation, and that nothing short of abdominal section will suffice for a cure.

With Battey's operation Mr. Tait's experience has been limited. In not one of the six patients on whom he has undertaken it were the uterine appendages found normal. His opinion, therefore, one way or the other can carry little weight. It is significant, however, that he goes so far as to say, "but I think if I had a daughter with feeble health, the result of pronounced menstrual epilepsy, I think I would advise her to have the operation performed." He grants, then, that there are cases where the operation is worthy of trial, and this is exactly the opinion to which operators in general are tending. The field is a narrow one, but in well and carefully selected cases the operation is one of great promise.

These are the main points touched upon by Mr. Tait in his interesting address. We have so far purposely refrained from noticing the scant courtesy which Sir Spencer Wells receives. His is and will remain an honored name in medicine. That he has made mistakes is but the lot of all men. If, in his declining years, he is unwilling or unable to render allegiance to new methods, this in no wise detracts from a past honorable record. Mr. Tait's position is assured. He cannot add to it by detracting from that of Spencer Wells. However just his animus, we regret its utterance, and are convinced that we but echo the voice of the profession here when we firmly claim for Sir Spencer Wells his due.

EGBERT H. GRANDIN.

ABSTRACT.

1. Hofmeier: The Pelvis Spinosa and Perforation of Douglas' Pouch during Labor (*Zeitsch. f. Geb. und Gyn.*, X., 1).—The following case forms the basis of H.'s paper: Patient æt. 23, IIpara, one premature labor three years before, with resulting vesico-vaginal fistula operated on by Schröder one year after. Two years after entered hospital again pregnant. Examination revealed sharply projecting promontory. Premature labor at 36th week with version and extraction. Eclampsia and collapse. Death in two and one-half hours. Child's head presented

a deep spoon-like depression ; weight, 2,350 grm. The dried pelvis gave the following measurements: Between spines, 23.5. Between crests, 25.5. Conjugata vera, 7.6 ctm. Transverse at inlet, 12.3 ctm. Left and right oblique, 11.4 ctm. Transverse at outlet, 10 ctm. The pelvis then, had it presented no other anomaly, would have been classed with the generally contracted flat pelvis. In addition, however, there was complete blending of the first sacral with the last lumbar vertebra, and from the promontory there existed a projection forward from the body of the first sacral vertebra, and directed downward nearly opposite the symphysis, giving the pelvic inlet the form of a heart. This specimen is almost unique, since H., after a thorough search through literature, has found but one case resembling it. The main interest of the case, however, depends on the fact that it offers an explanation for an occurrence which hitherto has been explained only theoretically. *A priori* it would seem likely that the existence of a deformity such as the above should cause perforation into Douglas' fossa rather than into the bladder, for the pressure on the vagina must be greater posteriorly than anteriorly. Wherever in the text-books reference is made to the occurrence of perforations at other parts than the bladder, it is the pelvis with a sharp projection or exostosis which is the cause. The older authorities considered that there resulted usually uterine rupture, or, according to Nägele, recto-vaginal fistulae. Spiegelberg considered the sharply projecting promontory a not uncommon cause of laceration of posterior cervical lip, though rarely did this come under observation, leading as it did to uterine rupture and death. Perforation of the peritoneum, he thought, rarely occurred, the anterior wall suffering from greater and more prolonged traction. Schröder does not consider this laceration of the posterior wall very infrequent, but he thinks we rarely see it, because a localized peritonitis occurs before the opening into Douglas' fossa. That this view of the matter, apart from theory, is the correct one, a further analysis of H.'s case will prove. A careful examination of the specimen revealed on the posterior vaginal wall opposite the site of the vesical fistula, about one and one-half ctm. under the border of the external os, a deep funnel-shaped cicatricial depression, and in Douglas' fossa a thick broad depression. Evidently, therefore, a perforation had occurred here, accompanied by inflammatory adhesion of the layers of the pouch. An analogous case was seen by H. the previous year where the perforation into the bladder, as the result of protracted labor, stood boldly in the foreground, and where the injury done to the posterior vaginal wall would have escaped notice had not a careful vaginal examination been made immediately after delivery. These two cases, therefore, negative Spiegelberg's assertion that such lacerations, when posterior, implicate only the cervical lip and not the vaginal wall ; and when we consider how in both these cases the lesion occurred without symptoms, it is allowable to argue that a similar lesion occurs more frequently than is noted ; and the insignificance of the symptoms is explained by the occurrence, as pointed out by Schröder, of a localized peritonitis before the perforation, whereby the peritoneal cavity is shut off.

E. H. G.

DEPARTMENT OF DISEASES OF CHILDREN.

EDITED BY . . . GEORGE B. FOWLER, M.D.

ORIGINAL COMMUNICATIONS.

CHLORAL VERSUS MORPHINE IN THE TREATMENT OF CONVULSIONS OF CHILDHOOD.

BY

HENRY DWIGHT CHAPIN, A.M., M.D.,

Attending Physician to the Out-Door Department, Bellevue Hospital (Children's Class).

It can safely be laid down as an absolute rule of practice, always to employ the safest possible therapeutic measures. The best plan is to use the least powerful drug that will produce a desired effect, for the more potent an agent may be, the greater are its possibilities for evil as well as for good.

There have lately been reported in this JOURNAL a number of cases—five in all—in which hypodermic injections of morphine were used to control the convulsions of infancy and early childhood, and their use commended. In the April number Dr. C. C. P. Clark, of Oswego, reported two cases. In the first instance an infant of eighteen months, who had been in convulsions two hours, was given morphine, gr. $\frac{1}{8}$ hypodermically with a successful result. In the second case, a child, four and one-half years old, had a spasm of a half hour's duration controlled by a "good dose" of the same remedy used subcutaneously. In the October issue, Dr. J. C. Hughson, of Sumter, S. C., relates a case in which, after using valerian, sinapisms, etc., with no effect in a boy of two years suffering from convulsions, morphine gr. $\frac{1}{15}$ was given hypodermically and followed by recovery. In the November number, Dr. Clark reports three more cases, occurring in two other physicians' practices. These cases do not seem to have been so successful, at least as regards final results. In the first instance, a child four years eight months

old had morphine gr. $\frac{1}{12}$ administered hypodermically and in twenty minutes gr. $\frac{1}{8}$ more. The convulsion which had lasted an hour and a half then ceased in ten minutes, but in about a week the child died of meningitis. In the next case, a child of three years four months, was seen when almost moribund, having been convulsed about an hour; morphine gr. $\frac{1}{8}$ was given hypodermically, and the convulsion almost immediately ceased. In about five hours, however, the child died in another attack before aid could be summoned. The last and most remarkable case is that in which an infant of only two months had morph. gr. $\frac{1}{8}$ administered subcutaneously, after convulsions had lasted two hours. Fearing too large a dose, a little belladonna was given; the infant made a good recovery. We have then five cases in which convulsions were quickly controlled by morphine given hypodermically; in two, however, death eventually took place, one in several hours from renewed convulsions, and the other in a few days from meningitis. Neither of the fatal cases had cerebro-spinal fever; at least such fact is not stated. Doubtless no one will be prepared to dispute the fact that infantile eclampsia can always be quickly controlled by morphine used subcutaneously, but that it is an extremely dangerous remedy, tending to hurry the congested brain into a fatal stupor, is equally clear to my mind. In his first communication on this subject, Dr. Clark states that the customary treatment by warm baths, emetics, purgatives, friction, and so on, has proved very unsatisfactory. The inefficiency of these agents will be cheerfully conceded by those who have relied upon them; but the present practice of many in this city of employing rectal injections of chloral has proved so extremely satisfactory that it is rarely, if ever, necessary to resort to such a dangerous agent as morphine. I have seen many cases, as critical as those in which the morphine was given subcutaneously, yield almost as quickly to chloral administered by the rectum, as doubtless have many others who have thus employed the latter. Some weeks ago I was called late at night to see a baby fifteen months old who had been in a convulsion over an hour. The infant was lying in a deep stupor, with jaws tightly set, thumbs pressed into the palms, and having convulsive seizures every few minutes. I tried in vain to pry open the teeth and get down a solution of potassium bromide. An enema was next

given, although the mother positively stated that no indigestible substance had been taken. No fecal matter came away, and the water remained in the bowel; as no time was to be lost in waiting for it to be expelled, I at once injected five grains of chloral in solution into the rectum. The convulsive movements soon became less violent and with longer intervals between. The chloral was repeated several times, and the convulsions soon stopped entirely. The stupor remained, but in several hours it was gradually replaced by a natural sleep, and the next morning the baby was quite bright, although looking pale. In this case I could find no cause for the convulsions, but I think if sufficient morphine had been given to control them, the stupor would have ended fatally.

Some years ago I watched a patient for Dr. J. Lewis Smith that showed in a wonderful manner the safety and efficiency of chloral in these cases. A little girl of eight years, who was recovering from an attack of measles, was suddenly seized with severe and prolonged convulsions. After they had ceased the child was left in a condition of deep stupor, and from the subsequent symptoms it appeared as if there were some serous exudation. Potassium bromide was freely given by the mouth, as she could swallow with difficulty; but in about an hour twitching began and all the other signs of a general convulsion. Dr. Smith had left directions that, if the bromide proved unavailing, to begin with rectal injections of chloral. This was accordingly done and the child quieted down almost immediately. All through the night and the following day this was repeated. The profound stupor remained, and at intervals she would begin to get very restless, tossing about the bed, with twitchings of the hands and arms, working of the mouth, and rolling of the eyes; but the chloral never failed to check these untoward symptoms and prevent the beginning convulsion. The stupor did not leave her for two days, when it gradually disappeared, and with it the constant convulsive tendency. She eventually made a good recovery and has never had any further cerebral trouble. This case, with its prolonged and dangerous stupor and eventual recovery, shows, I think, the advantage of chloral over morphine in such an emergency. Indeed, we have but to consider the condition of the brain in convulsions to appreciate this fact. There is naturally a condition of great passive con-

gestion of that organ from retardation of the circulation and interference with respiration, and serous effusion or extravasation of blood may take place at any time. A drug is then indicated that will check the intense irritability of the cord, without increasing the stupor that is pointing to dangerous cerebral congestion, if not extravasation. Chloral seems to fulfil this indication, as it markedly lessens the reflex activity of the spinal cord in doses not sufficient to overpower the brain. In large doses it is an intense depressant of the centres at the base of the brain and upon the spinal cord. Opium likewise checks general reflex irritability, but with a much more powerful narcotic action on the cerebral centres. The sensitiveness of infants and young children to opium is well known, and there are probably few men who have not had this fact unpleasantly brought to their notice in the early period of their practice. Althaus mentions a case on record where one-ninetieth of a grain caused death in an infant one month old. We are told that in infantile eclampsia there is an extra tolerance of morphine, but five cases hardly afford sufficient material for generalizing. By using it hypodermically the potency of the drug is greatly enhanced, and, to consider one-fourth of a grain thus given a proper dose for a child two years old, in convulsion, will possibly yield grave results. An appeal to clinical facts is the only means of settling vexed questions on the theoretical and practical working of drugs; as to the clinical effects of morphine used subcutaneously in infantile eclampsia, we have not enough cases to generalize, but certainly all the knowledge we have on the subject contraindicates such practice. A few successful cases are reported and its use advised; the instances in which it is dubious as to whether a fatal stupor has been hastened by the narcotic effect of the drug will probably not take up much space in medical literature. The object of this paper, however, is to strongly urge that, as we have in chloral administered by the rectum an efficient and safe agent for the control of eclampsia, it will very rarely be necessary to use an agent that most will concede to be dangerous. The chloral thus given is soon absorbed and acts almost as quickly as subcutaneous injections, when a child is unable to swallow. An infant of from one to two years can have five grains injected in watery solution. I have even seen temporary benefit follow

its use in the severe convulsions of cerebro-spinal fever. At the same time, it is never safe to claim any drug as a cure-all, and I should hardly feel like assuring parents that, if they threw their children into fits, I could always cure them. I am convinced, however, that, when faithfully tried, it will very rarely fail in controlling convulsions. If a child can swallow, bromide of potassium is very efficient, in fact will often be all that it is necessary to give. In addition to diminishing the reflexes, it acts favorably by lessening cerebral hyperemia. After removing all irritation, gastro-intestinal or otherwise, and administering bromide by the mouth and chloral by the rectum, the doubtful question of using morphine by hypodermic injection will very rarely be raised by the conservative practitioner.

TRANSLATIONS.

THE INHERITANCE AND TRANSMISSION OF SYPHILIS.

BY

DR. M. KASSOWITZ,
of Vienna.

Translated from *Jahrbch. f. Kindhlkde.*, xxi. B., 1 u. 2 H.

By J. FEWSMITH, JR., M.D., Newark, N. J.

(Concluded from p. 1230.)

I WILL not stop to closely criticise these four cases. They are not wholly free from objection, but on the whole they give the firm impression that infection of children during or after birth has taken place. At least it is scarcely credible that all four observers have been in error, and if only one of the cases is firmly established, it is sufficient to prove, first, that the syphilis of the mother existing during pregnancy has not been in even the slightest degree transmitted to the fetus, and second, that the immunity which theory and observation show that most of these children possess, was wanting in this case.

We come now finally to the question whether there are well authenticated cases which justify us in accepting the theory of syphilitic infection of the fetus per placentam. Eight years ago I answered this question in the negative, because I had seen no case myself, and could find none in the literature in which existed the necessary proofs for the affirmative answer to the question, namely, healthfulness of both parents at the time of

conception and indisputable syphilitic infection of the child before birth. In 1878, A. Weil, though on theoretical grounds a warm supporter of the intrauterine theory, confessed that no positive case was known to him. But since then, perhaps provoked by my sharper defining of the question, several such cases have been published, which I will now concisely reproduce.

Zeissl's case:—Infection of the husband given as in second month, established primary lesion during the third, syphilitic sclerosis on labium of the wife in seventh month of pregnancy, and some weeks later general symptoms in the latter. Birth of a living, ripe child, who was attacked on the eleventh day with variola syphilitica on the soles of the feet and with a general maculo-papular syphilide, from which it soon died.

Hudson's case:—First child healthy. During the second pregnancy the man (according to his own story only) was infected from without, and from him the mother, upon whom a fresh genital ulcer was discovered in the third month, and who later had constitutional syphilis. The child had an eruption *at birth*, with thickening of the ends of the bones, and died after two weeks. In the next pregnancy, abortion.

Vajda's case:—The woman, who had been found to be healthy in the sixth month of pregnancy, was, in the seventh month, infected on the genitals from her husband, who was infected two and one-half months before. The child was born at full term and healthy, but seven weeks post partum had papules, which were followed afterward by psoriasis, pustules and ozena. The general symptoms did not appear in the mother until after parturition.

Behrend's case:—The husband was treated for an indurated chancre while his wife was four months pregnant. The woman was first examined during the last month and then had various symptoms of general syphilis. She stated that she had noticed a hardening on the genitals in the fifth month. The child was born at full term and apparently healthy, and was not under observation later, but was said to have had *varicella* in the second month, and died in the third. The corpse showed clear traces of papules, and of diffuse infiltration of the palms and soles, with desquamation of the epidermis and some traces of vesicles or pustules. A second case of Behrend's is omitted because not enough was known of the father.

Engel's case:—The man was infected during the second month of his wife's pregnancy, and immediately infected her so that she had an indurated ulcer, six weeks later an indolent bubo, and two months later angina and psoriasis palmaris. A hearty boy was born, who, however, had a copper-colored eruption about the anus (when?) and one week after birth suffered from coryza. These symptoms soon disappeared under specific treatment, and did not return.

We have here, therefore, five cases in which—taking for granted the correctness of the observations, which we have no reason to doubt—we must acknowledge a placental infection of a child conceived by healthy parents. The question is whether such infection has occurred more frequently, or whether we have here to do only with, to a certain extent, exceptional circumstances. In this connection it is worth noting that the very authors who claim the frequent occurrence of placental infection have produced no really convincing case. Thus Fournier, in his cases bearing on this point, has paid no attention to the condition of the fathers; and this is true also of Hutchinson, for in only one of his four cases is the father mentioned, and there we

learn that the latter had been treated for syphilis for a year, so that he probably was syphilitic at the time of conception of the child. Moreover, most of his cases came under his notice only after delivery, one of them not till the child was twelve years old. In one case the pregnant woman had a chancre on the tongue one week before delivery, and the hearty child had a syphilitic eruption eight weeks after birth which rapidly disappeared under treatment.

These cases are as little convincing as the experience detailed by some authors, without observation of, or attention to, the condition of the fathers, that when the mother is infected near the end of pregnancy, healthy children are more often born, and when the infection takes place in the early months the children are more apt to be syphilitic. Some of the latter cases might have fallen in the category of infection per placentam, had we been better informed as to the condition of both parents at the time of conception, but an exact weighing of the circumstances which usually prevailed in these cases speaks against the frequency of such an occurrence. It is fair to suppose that in the majority of cases the person who infects the mother is the father of the child, and infection of the pregnant woman from a third party is more rare. Now it is well known, and Fournier out of his rich experience has expressly stated, that infection during wedded life rarely occurs from a primary lesion in the husband, but principally through infectious secondary symptoms, fissures, eroded papules, etc.; Fournier even going so far as to say that this is also the case outside of wedded folk. Hence in the larger number of cases in which the infection of the pregnant woman takes place in the earlier months of pregnancy it is probable that the husband, who now has secondary symptoms, was syphilitic at the time of conception, and that the fetus acquired the disease in this way and could not be infected a second time per placentam from the later infection of the mother. We know further that women who are infected during pregnancy often abort, so that this also decreases the number of cases of placental infection of the fetus. Moreover, such placental infection is possible in only a *first* pregnancy, for after this the next fetus is already poisoned from the ovum, and not receptive toward new infection per placentam. There remain, therefore, in toto only a few cases in which circumstances admit of placental infection—namely, when the fetus is healthy at the time of conception and when the infection of the mother does not lead to abortion. And now experience teaches that, *even of these few cases, in a large proportion there has surely been no placental infection of the fetus.* Even the statement of authors, that women infected in the later months of pregnancy often bear healthy children, speaks with decisiveness for the fact that there must be some obstacle in these cases which prevents the transmission of the disease from the mother to the fetus, for just at this time the interchange of fluids between the mother and the already large fetus is particu-

larly active. We therefore again reach the conclusion, *that also in the direction from mother to fetus the placenta acts as a filter which, as a rule, does not permit the passage of the syphilitic contagion, and that this occurs only in comparatively rare cases and under circumstances not yet understood.* At any rate, one thing is clear, that in comparison to the very frequent transmission of the poison by way of ovum or seminal cell, the infection of the fetus in utero plays a very secondary rôle.

Before leaving this point, we must endeavor to ascertain how, in cases where we must accept the observations of placental infection, the disease of the child differs from the pictures of acquired infantile syphilis and true hereditary syphilis; for in its genesis it differs as markedly from the acquired as from the syphilis beginning with conception. In acquired syphilis the poison enters at some point of the general surface and leads to a general blood poisoning by successive steps (the first incubation, the primary lesion and primary glandular swellings; the second incubation, etc.); while in the placental infection, according to theory, the poison enters the general circulation *at once*, and in the true hereditary affection the developing individual is infected from his very earliest beginning, while in the placental infection the poison, at a certain moment, enters an advanced organism which had been previously healthy. That such difference in genesis may find expression in the symptoms of the disease is shown by a comparison of hereditary and acquired infantile syphilis; and the study of those manifestations which are most characteristic of the hereditary form; namely, the diffuse syphilitic infiltration of the skin, the bullous syphilide, the coryza which *precedes* the skin symptoms, the specific affections of the heads of long bones, the development of gummy visceral affections, and most of all the severe *constitutional impairment*, which in a certain number of cases renders the viability of the child doubtful, and in other cases shows itself through a very light weight at birth, a delicate constitution, and a severe cachexia—all of them manifestations to which an acquired syphilis, even if acquired in the child's first days (as during circumcision) can offer nothing similar.

We should, therefore, expect that the infection of the fetus per placentam would leave an impression on the resulting syphilis which would in some way characterize it as compared with the hereditary or the acquired forms. The analysis of the five cases given above disappoints us in this regard. In regard to the viability and constitutional condition of the children at birth, there was nothing pointing to any weakness, but, on the contrary, they were born at full term and most of them apparently healthy. Engel speaks especially of the healthy appearance of the child. The children were most severely affected in Zeissl's and Hudson's cases, because they only lived two weeks. Behrend's child died in the third month; the other two lived. These two appeared like children with an extremely mild acquired syphilis, having none of the very characteristic manifestations of hereditary syphilis.

The ozena mentioned by Vajda as occurring *after* the skin symptoms is frequent also in acquired syphilis. Only the prodromal coryza is characteristic of hereditary syphilis. On the other hand, in Zeissl's case there was "*variola syphilitica*," a pustular syphilide; in Hudson's case, thickening of the ends of the bones; and in Behrend's, diffuse infiltration of the palms and soles—all of them manifestations which are characteristic of hereditary syphilis. But again there is no mention of any prodromal coryza or of the characteristic habitus or striking physiognomy of children with hereditary syphilis. In regard to the time of appearance of the symptoms, Hudson's child and, strange to say, Engel's lightly-affected child had them at birth; Zeissl's on the eleventh day, Vajda's in seventh week, and Behrend's two months after birth. From all this it is evident that it is impossible to find any characteristic moment through which a child infected in a late stage of fetal development may be distinguished from one who has received the poison with its first germ, and this is not the least of the very many difficulties in the way of a solution of this important theme. We cannot too earnestly desire that those who have the rare opportunity of observing such cases would search them in every direction and, particularly when the placental infection seems most surely established, note most exactly the manifestations and the course of the disease in the child.

This concludes the exposition of the facts. The following theoretical conclusions may be drawn from them, already frequently hinted at, but now thus recapitulated:

1. *The syphilitic poison, in a large number of cases, comes to a halt at the dividing-wall between the maternal and fetal circulations and does not pass this, in spite of the active interchange of nutritious fluids continued for months.*

2. *Either of the two organisms which, in spite of this interchange of fluids, has remained free from syphilis possesses a high degree of immunity against syphilitic infection.*

We must deny ourselves a further theoretical discussion of these two statements. Such discussion must be directed to the inquiry as to what circumstances prevent the passage of the syphilitic contagion into the neighboring circulation, and why this obstacle is overcome in some cases; and second, how can we account for immunity against syphilis without syphilitic disease? A satisfactory answer to these questions—which immediately involves a deeper insight into the nature of the syphilitic contagion—is still in the far distance, but here is nevertheless a path which may lead to priceless results.

[In making this translation, I have adhered quite closely to the text, not only in the fulness of the argument, but also in the forms of expression. I have, however, omitted foot-notes, references to authors by the name and date of their publications, and some few detailed cases. Those who desire a closer acquaintance with the literature of the subject may find on pp. 52-55 of the *Jahrbuch* (XXI. Bd., 1. u. 2. H.) a very full list of the publica-

tions of the last eight years, sixty-seven in number, arranged chronologically. I know of no better index anywhere to recent syphilological literature.—TRANSLATOR.]

ABSTRACT.

1. **Pott: Fecal Fistula in the Scrotum** (*Jahrbch.*, XXI. B., 4 H.).—Professor Pott reports a case of the above in a well developed boy of four months. When he was two weeks old, a red point appeared on right side of scrotum, the whole scrotum swelled, finally broke and discharged pus and fecal matter. Since then the fistula had become smaller and only discharged fecal matter on straining, coughing, etc. Gas generally came with the discharge. The interesting point in the case is that by following a thickened cord up the inguinal canal, it seemed to be shown that this was one of those cases of *hernia vaginalis dextra*, which are known to occur in children, in which the *processus vermiformis* is the protruding part, and in which the end of the latter becomes gangrenous, ruptures, and then forms an adhesive union with the inner wall of the scrotum. Such cases of Nature's cure have been reported by Gooch and Armsby, one in a child of six weeks, and the other in one of three years. No operation was allowed by the parents in this case of Pott's.

J. F. JR.

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